PHILOSOPHICAL

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OF

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(A.)

FOR THE YEAR MDCCCLXXXIX.

VOL. 180.

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MDCCCXC.

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Adjudication of the Medals of the Royal Society for the year 1889, by the President and Council.

The Copley Medal to the Rev. George Salmon, D.D., F.R.S., for his various Papers on subjects of Pure Mathematics, and for the valuable Mathematical Treatises of which he is the Author.

A Royal Medal to Walter Holbrook Gaskell, F.R.S., for his Researches in Cardiac Physiology, and his important Discoveries in the Anatomy and Physiology of the Sympathetic Nervous System.

A Royal Medal to Thomas Edward Thorpe, F.R.S., for his Researches on Fluorine Compounds, and his Determination of the Atomic Weights of Titanium and Gold.

The Davy Medal to William Henry Perkin, F.R.S., for his Researches on Magnetic Rotation in relation to Chemical Constitution.

The Bakerian Lecture, "A Magnetic Survey of the British Isles for the Epoch January 1, 1886," was delivered by Professor A. W. Rücker, F.R.S., and Professor T. E. Thorpe, F.R.S.

The Croonian Lecture, "Les Inoculations Préventives," was delivered by Dr. E. Roux.

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(A.)

VOL. 180.

LIST OF ILLUSTRATIONS.

PHILOSOPHICAL TRANSACTIONS.

I. *On the Mechanical Conditions of a Swarm of Meteorites, and on Theories of Cosmogony.*

By G. H. DARWIN, *LL.D., F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge.*

Received July 12,—Read November 15, 1888.

MR. LOCKYER writes in his interesting paper on Meteorites* as follows :—

"The brighter lines in spiral nebulæ, and in those in which a rotation has been set up, are in all probability due to streams of meteorites with irregular motions out of the main streams, in which the collisions would be almost *nil*. It has already been suggested by Professor G. DARWIN ('Nature,' vol. 31, 1884–5, p. 25)—using the gaseous hypothesis—that in such nebulæ 'the great mass of the gas is non-luminous, the luminosity being an evidence of condensation along lines of low velocity, according to a well known hydrodynamical law. From this point of view, the visible nebula may be regarded as a luminous diagram of its own stream-lines.'"

The whole of Mr. LOCKYER's paper, and especially this passage in it, leads me to make a suggestion for the reconciliation of two apparently divergent theories of the origin of planetary systems.

The nebular hypothesis depends essentially on the idea that the primitive nebula is a rotating mass of fluid, which at successive epochs becomes unstable from excess of rotation, and sheds a ring from the equatorial region.

The researches of ROCHE† (apparently but little known in this country) have imparted to this theory a precision which was wanting in LAPLACE's original exposition, and have rendered the explanation of the origin of the planets more perfect.

But notwithstanding the high probability that some theory of the kind is true,‡ the acceptance of the nebular hypothesis presents great difficulties.

Sir WILLIAM THOMSON long ago expressed to me his opinion that the most probable origin of the planets was through a gradual accretion of meteoric matter, and

* 'Nature,' Nov. 17, 1887. The paper itself is in 'Roy. Soc. Proc.,' Nov. 15, 1887 (No. 259, p. 117).

† 'Montpellier, Acad. Sci. Mém.'

‡ [The very remarkable photograph of the nebula in Andromeda, exhibited to the Royal Astronomical Society by Mr. Isaac Roberts on December 6, 1888, affords something like a proof of the substantial truth of the nebular hypothesis.—G. H. D. December 19, 1888.]

the researches of Mr. LOCKYER afford actual evidence in favour of the abundance of meteorites in space.

But the very essence of the nebular hypothesis is the conception of fluid pressure, since without it the idea of a figure of equilibrium becomes inapplicable. Now, at first sight, the meteoric condition of matter seems absolutely inconsistent with a fluid pressure exercised by one part of the system on another. We thus seem driven either to the absolute rejection of the nebular hypothesis, or to deny that the meteoric condition was the immediate antecedent of the Sun and Planets. M. FAYE has taken the former course, and accepts as a necessary consequence the formulation of a succession of events quite different from that of the nebular hypothesis.* I cannot myself find that his theory is an improvement on that of LAPLACE, except in regard to the adoption of meteorites, for he has lost the conception of the figure of equilibrium of a rotating mass of fluid.

The object of this paper is to point out that by a certain interpretation of the meteoric theory we may obtain a reconciliation of these two orders of ideas, and may hold that the origin of stellar and planetary systems is meteoric, whilst retaining the conception of fluid pressure.

According to the kinetic theory of gases, fluid pressure is the average result of the impacts of molecules. If we imagine the molecules magnified until of the size of meteorites, their impacts will still, on a coarser scale, give a quasi-fluid pressure. I suggest then that the fluid pressure essential to the nebular hypothesis is, in fact, the resultant of countless impacts of meteorites.

The problems of hydrodynamics could hardly be attacked with success, if we were forced to start from the beginning and to consider the cannonade of molecules. But when once satisfied that the kinetic theory will give us a gas, which, in a space containing some millions of molecules, obeys all the laws of an ideal non-molecular gas filling all space, we may put the molecules out of sight and treat the gas as a plenum.

In the same way, the difficulty of tracing the impacts of meteorites in detail is insuperable; but, if we can find that such impacts give rise to a quasi-fluid pressure on a large scale, we may be able to trace out many results by treating an ideal plenum. LAPLACE'S hypothesis implies such a plenum, and it is here maintained that this plenum is merely the idealisation of the impacts of meteorites.

As a bare suggestion this view is worth but little, for its acceptance or rejection must turn entirely on numerical values, which can only be obtained by the consideration of some actual system. It is obvious that the solar system is the only one about which we have sufficient knowledge to afford a basis for discussion. This paper is accordingly devoted to a consideration of the mechanics of a swarm of meteorites, with special numerical application to the solar system.

The investigation has entailed a considerable amount of mathematical analysis;

* 'Sur l'Origine du Monde,' Paris, GAUTHIER-VILLARS, 1884; 'Annuaire pour l'an 1885, Bureau des Longitudes,' p. 757.

there is, however, no analysis in §§ 1 and 2. The reader who only wishes to know the arguments and results, without a consideration of the mathematical details, is therefore recommended, after reading §§ 1 and 2, to pass on to the Summary.

§ 1. *On the Effective Elasticity of Meteorites in Collision.*

When two meteoric stones meet with planetary velocity, the stress between them during impact must generally be such that the limits of true elasticity are exceeded; and it may be urged that a kinetic theory is inapplicable unless the colliding particles are highly elastic. It may, however, I think, be shown that the very greatness of the velocities will impart what virtually amounts to an elasticity of a high order of perfection.

It appears, *a priori*, probable that, when two meteorites clash, a portion of the solid matter of each is volatilised, and Mr. LOCKYER considers the spectroscopic evidence conclusive that it is so. There is, no doubt, enough energy liberated on impact to volatilise the whole of both bodies, but only a small portion of each stone will undergo this change.

A rough numerical example will show the kind of quantities with which we are here dealing.

It will appear hereafter that the mean velocity of a meteorite may be at the least about 5 kilometres a second; and, accordingly, the mean relative velocity of a pair would then be about 7 kilometres a second.* Hence, if two stones, weighing a kilogramme, move each with a velocity of $3\frac{1}{2}$ kilometres per second directly towards one another, the energy liberated at the moment of impact is $2 \times \frac{1}{2} \times 10^3 (3\frac{1}{2} \times 10^5)^2$ or 12×10^{13} ergs.

Now JOULE'S equivalent is $4{\cdot}2 \times 10^7$ ergs; hence, the energy liberated is about 3 million calories.

It is quite uncertain how much of each stone would be volatilised; but, if it were 3 grammes, there would be a million calories of energy applied to each gramme.

The melting temperature of iron is about 1500 degrees Centigrade, and the mean specific heat of iron may be about $\frac{1}{7}$.† Hence, about 300 calories are required to raise a gramme of iron from absolute zero to melting point. I do not know the latent heat of the melting of iron, but for platinum it is 27, and the latent heat of volatilisation of mercury is 62. Hence, about 400 or 500 calories suffice to raise a gramme of iron from absolute zero to volatilisation. It is clear, then, that there is energy enough, not only to volatilise the iron, but also to render the gas incandescent; and the same would be true if the mass operated on by the energy were 30 grammes instead of 3.

It must necessarily be obscure as to how a small mass of solid matter *can* take up a very large amount of energy in a small fraction of a second, but spectroscopic evidence seems to show that it does so; and, if so, we have what is virtually a violent explosive introduced between the two stones.

* If v be the velocity of mean square, $v\sqrt{2}$ is the square root of the mean square of relative velocity.

† 'Physikalisch-Chemische Tabellen.' LANDOLT and BÖRNSTEIN.

In a direct collision each stone is probably shattered into fragments, like the splashes of lead when a bullet hits an iron target. But direct collision must be a comparatively rare event. In glancing collisions the velocity of neither body is wholly arrested, the concentration of energy is not so enormous (although probably still sufficient to effect volatilisation), and, since the stones rub past one another, more time is allowed for the matter round the point of contact to take up the energy; thus, the whole process of collision is much more intelligible. The nearest terrestrial analogy is when a cannon-ball bounds off the sea. In glancing collisions fracture will probably not be very frequent.

From these arguments, it is probable that, when two meteorites meet, they attain an effective elasticity of a high order of perfection; but there is, of course, some loss of energy at each collision. [It must, however, be admitted that on collision the deflection of path is rarely through a very large angle. But a succession of glancing collisions would be capable of reversing the path; and, thus, the kinetic theory of meteorites may be taken as not differing materially from that of gases.*]

Perhaps the most serious difficulty in the whole theory arises from the fractures which must often occur. If they happen with great frequency, it would seem as if the whole swarm of meteorites would degrade into dust. We know, however, that meteorites of considerable size fall upon the Earth; and, unless Mr. LOCKYER has misinterpreted the spectroscopic evidence, the nebulæ do now consist of meteorites. Hence, it would seem as if fracture was not of very frequent occurrence. It is easy to see that, if two bodies meet with a given velocity, the chance of fracture is much greater if they are large, and it is possible that the process of breaking up will go on only until a certain size, dependent on the velocity of agitation, is reached, and will then become comparatively unimportant.

When the volatilised gases cool, they will condense into a metallic rain, and this may fuse with old meteorites whose surfaces are molten. A meteorite in that condition will certainly also pick up dust. Thus, there are processes in action tending to counteract subdivision by fracture and volatilisation. The mean size of meteorites probably depends on the balance between these opposite tendencies. If this is so, there will be some fractures, and some fusions, but the mean mass will change very slowly with the mean kinetic energy of agitation. This view is, at any rate, adopted in the paper as a working hypothesis. It was not, however, possible to take account of fracture and fusion in the mathematical investigation, but the meteorites are treated as being of invariable mass.

§ 2. *On the Velocity of Agitation of Meteorites, and on its Secular Change.*

The velocity with which the meteorites move is derived from their fall from a great distance towards a centre of aggregation. In other words, the potential energy of their mutual attraction when widely dispersed becomes converted, at least partially,

* Added Nov. 16, 1888.

into kinetic energy. When the condensation of a swarm is just beginning, the mass of the aggregation towards which the meteorites fall is small; and, thus, the new bodies arrive at the aggregation with small velocity. Hence, initially, the kinetic energy is small, and the volume of the sphere within which hydrostatic ideas are (if anywhere) applicable is also small.

As more and more meteorites fall in that volume is enlarged, and the velocity with which they reach the aggregation is increased. Finally, the supply of meteorites in that part of space begins to fail, and the imperfect elasticity of the colliding bodies brings about a gradual contraction of the swarm.

I do not now attempt to trace the whole history of a swarm, but the object of the paper is to examine its mechanical condition at an epoch when the supply of meteorites from outside has stopped, and when the velocities of agitation and distribution of meteorites in space have arranged themselves into a sub-permanent condition, only affected by secular changes. This examination will enable us to understand, at least roughly, the secular change in the velocity and in the distribution of the meteorites as the swarm contracts, and will throw light on other questions.

§ 3. *Formulæ for Mean Square of Velocity, Mean Free Path, and Interval between Collisions.*

We have to investigate whether, when the solar system consisted of a swarm of meteorites, the velocities and encounters could have been such that the mechanics of the system can be treated as subject to the laws of hydrodynamics. The formulæ which form the basis of this discussion will now be considered.

For the sake of simplicity, the meteorites will, in the first instance, be treated as spheres of uniform size.

The sum of the masses of the meteorites is equal to that of the Sun, for the planets only contribute a negligible mass. If M_0 be the Sun's mass, and m that of a meteorite, their number is M_0/m.

If, at each encounter between two meteorites, there were no loss of energy, the sum of the kinetic energies of all the meteorites would be equal to the potential energy lost in the concentration of the swarm from a condition of infinite dispersion, until it possessed its actual arrangement. In such a computation the rotational energy of the system is negligible.

Suppose the Sun's mass to be concentrated from infinite dispersion until it is arranged in the form of a homogeneous sphere of radius a and density ρ. Then let the sphere be cut up into as many equal spaces as there are meteorites, and let the matter in each space be concentrated into a meteorite. When the number of meteorites is large, the potential energy lost in the first process is very great compared with that lost in the subsequent partial condensation into meteorites.* Thus, the energy lost in the partial condensation is negligible.

* It depends, in fact, on the square of the ratio of the diameter $2a$ to the linear dimension of one of the equal spaces.

If μ be the attractional constant, the lost energy of condensation is well known to be $\frac{3}{5}\mu M_0^2/a$. But on the hypothesis that there is no loss of energy at each encounter, this must be equal to the sum of the kinetic energies of all the meteorites. If, therefore, v^2 be the mean square of velocity of a meteorite, we must have $\frac{1}{2} M_0 v^2 = \frac{3}{5}\mu M_0^2/a$, so that $v^2 = \frac{6}{5}\mu M_0/a$.

But homogeneity of density and uniformity of kinetic energy of agitation are impossible; for the meteor-swarm must be much condensed towards its centre, so that we have largely underestimated the lost potential energy of the system. Also, the velocity of agitation must decrease towards the outside, or else the swarm would extend to infinity. Besides this, the partial conversion of molar into molecular energy, which must take place on each encounter, has been neglected.

We shall see below reason for believing that throughout a large central volume the mean square of velocity of agitation is nearly uniform, and that outside of this region it falls off.

Suppose, then, that M is the mass and a the radius of that portion of the swarm in which the square of velocity of agitation is uniform; let v_0^2 be that square of velocity, and let it be defined by reference to the potential of M at distance a, so that

$$v_0^2 = \beta^2 \frac{\mu M}{a}, \qquad \ldots \ldots \ldots \ldots \quad (1)$$

where β is a coefficient for which a numerical value will be found below.

The square of velocity of agitation outside of the radius a is to be denoted by v^2, and subsequent investigation will be necessary to evaluate v^2 in terms of v_0^2.

If we denote by a_0 the Earth's distance from the Sun, and by u_0 the Earth's velocity in its orbit, we have

$$u_0^2 = \mu \frac{M_0}{a_0}. \qquad \ldots \ldots \ldots \ldots \quad (2)$$

Whence,

$$v_0 = \beta u_0 \left(\frac{M a_0}{M_0 a}\right)^{\frac{1}{2}}. \qquad \ldots \ldots \ldots \ldots \quad (3)$$

If in any distribution of meteorites w is the sum of the masses of all the meteorites in unit volume, or the density of the swarm at any point, and if λ be that distance which is called in the kinetic theory of gases "the mean distance between neighbouring molecules," we have

$$\lambda^3 = \frac{m}{w}. \qquad \ldots \ldots \ldots \ldots \quad (4)$$

Now, the mean density of that part of the swarm in which the kinetic energy of agitation is constant being ρ, we have

$$\rho = \frac{3M}{4\pi a^3}, \qquad \ldots \ldots \ldots \ldots \quad (5)$$

and

$$\lambda^3 = 4\,\pi\, a^3 . \frac{m}{M} . \frac{\frac{1}{3}\rho}{w}. \qquad \ldots \ldots \ldots \quad (6)$$

Suppose that s is "the radius of the sphere of action" of a meteorite, so that when two of them approach so that the distance between their centres is s there is a collision.

Let L and T be the mean free path and mean interval between collisions. Then, since the mean velocity is $v\sqrt{(8/3\pi)}$, we have, according to the kinetic theory of gases,*

$$L = \frac{\lambda^3}{\pi s^2 \sqrt{2}}, \qquad T = \frac{L}{v}\sqrt{\tfrac{3}{8}\pi}. \qquad \qquad (7)$$

Then, on substitution from (4), (5), and (6), we have

$$L = \left[\frac{a_0^3\sqrt{8}}{M_0}\right]\left(\frac{a}{a_0}\right)^3 \frac{M_0}{M} \cdot \frac{\frac{1}{3}\rho}{w} \cdot \frac{m}{s^2}, \qquad T = \left[\frac{a_0^3\sqrt{3\pi}}{M_0 u_0}\right]\left(\frac{a}{a_0}\right)^{\frac{7}{2}} \cdot \frac{1}{\beta}\left(\frac{M_0}{M}\right)^{\frac{3}{2}} \frac{v_0}{v} \cdot \frac{\frac{1}{3}\rho}{w} \cdot \frac{m}{s^2}. \qquad (8)$$

Now let

$$\left.\begin{aligned} u_0 &= \left(\frac{\mu M_0}{a_0}\right)^{\frac{1}{2}}, & u &= u_0\left(\frac{a_0}{a}\right)^{\frac{1}{2}} \\ l_0 &= \frac{a_0^3\sqrt{8}}{M_0}, & l &= l_0\left(\frac{a}{a_0}\right)^3 \\ \tau_0 &= \frac{a_0^3\sqrt{3\pi}}{M_0 u_0}, & \tau &= \tau_0\left(\frac{a}{a_0}\right)^{\frac{7}{2}} \end{aligned}\right\}, \qquad (9)$$

and we have

$$\left.\begin{aligned} v &= \beta u\left(\frac{M}{M_0}\right)^{\frac{1}{2}} \\ L &= l\,.\left(\frac{M_0}{M}\right) \cdot \frac{\frac{1}{3}\rho}{w} \cdot \frac{m}{s^2} \\ T &= \tau\,.\frac{1}{\beta}\left(\frac{M_0}{M}\right)^{\frac{3}{2}} \cdot \frac{v_0}{v} \cdot \frac{\frac{1}{3}\rho}{w} \cdot \frac{m}{s^2} \end{aligned}\right\}. \qquad (10)$$

We now proceed to calculate u_0, l_0, τ_0, and also $2a_0/l_0$, using the centimetre-gramme-second system of units.

The Sun's mass may be taken as 315,511 times that of the Earth, and the Earth as $6{\cdot}14 \times 10^{27}$ grammes†; hence

$$M_0 = 10^{33{\cdot}28718} = 1{\cdot}9372 \times 10^{33} \text{ grammes.}$$

The attractional constant and the Earth's mean distance from the Sun are

$$\mu = \frac{648}{10^{10}}, \qquad a_0 = 1{\cdot}487 \times 10^{13} \text{ cm.}$$

* MEYER, 'Kinetische Theorie der Gase.'

† Here and elsewhere I generally use EVERETT'S 'Units and Physical Constants.'

With these values

$$\left.\begin{aligned} u_0 &= 10^{6\cdot46323} = 2{,}905{,}600 \text{ cm. per sec.} \\ l_0 &= 10^{6\cdot68129} = 4{,}800{,}600 \text{ cm.} \\ \tau_0 &= 10^{0\cdot25366} = 1\cdot79334 \text{ sec.} \\ \frac{2a_0}{l_0} &= 10^{6\cdot79204} = 6{,}195{,}000 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

The dimensions of l_0 and τ_0 are not those of length and time; but, if meteorites of 1 gramme mass, with sphere of action 1 centimetre, and "velocity of mean square" of agitation equal to the Earth's velocity in its orbit, have density of distribution equal to one-third of the mean density of the sphere M, then l_0, τ_0 will be the mean free path and time, as stated in centimetres and seconds. We may thus regard l_0, τ_0 as a length and time, provided care be taken in the subsequent use of the symbols to adhere to the c.g.s. system of units.

§ 4. *On the Equilibrium of a Gas at Uniform Temperature in Concentric Spherical Layers under its own Gravitation.*

It is assumed provisionally that the conditions are satisfied which permit us to regard the swarm of meteorites as a quasi-gas, subject to the laws of hydrostatics.

The solution of this problem, then, becomes a necessary preliminary to the discussion of the kinetic theory of meteorites. The equilibrium of a gas under its own gravitation has been ably discussed by Professor RITTER in one of his series of papers on gaseous planets.* The intrinsic interest of the problem renders an independent solution valuable. Suppose, then, that a mass M_1 of gas is enclosed in a spherical envelope of radius a_1, and is in equilibrium in concentric spherical layers. Let v_1^2, the mean square of the velocity of agitation of the gaseous molecules, be defined by reference to the potential of the mass M_1 at the radius a_1, so that

$$v_1^2 = \beta_1^2 \frac{\mu M_1}{a_1},$$

where β_1^2 is a numerical coefficient, and μ is the attractional constant.

Let p and w be the pressure and density of the gas at radius r, and k the modulus of elasticity, so that

$$p = k\,w,$$

$$k = \tfrac{1}{3} v_1^2 = \tfrac{1}{3} \beta_1^2 \frac{\mu M_1}{a_1}.$$

* "Untersuchungen über die Höhe der Atmosphäre und die Constitution gasförmiger Weltkörper," 'WIEDEMANN'S Annalen' (New Series), vol. 16, 1882, p. 166. A very elegant solution of part of my problem has also been given by Mr. G. W. HILL in the 'Annals of Mathematics,' vol. 4, No. 1, p. 19 (February, 1888). Mr. HILL'S paper only reached my hands after my own calculations had been completed, and I therefore adhere to my own less elegant method. Mr. HILL has obviously not seen M. RITTER'S papers.

Then the equation for the hydrostatic equilibrium of the gas is

$$\frac{r^2}{w}\frac{dp}{dr} + 4\,\pi\,\mu \int_0^r wr^2\,dr = 0. \qquad \qquad (12)$$

It is obvious that $\dfrac{-r^2}{\mu w}\dfrac{dp}{dr}$ is equal to the whole mass enclosed inside radius r, and this relation will hold however the equation be transformed, provided we do not multiply the equation by any factor.

In consequence of the relation between p and w this may be written

$$k\left[r^2\frac{d}{dr}\log w + \frac{4\pi\mu}{k}\int_0^r wr^2\,dr\right] = 0.$$

If ρ_1 be the mean density of the mass M_1, we have

$$4\,\pi\,\mu = \frac{3\mu M_1}{\rho_1 a_1^3} = \frac{9k}{\beta_1^2 a_1^2 \rho_1}.$$

Hence, we may write the equation (12) in the form

$$ka_1\left[\frac{r^2}{a_1}\frac{d}{dr}\log w + \frac{9}{\beta_1^2}\int_0^r \frac{w}{\rho_1}\frac{r^2}{a_1^3}\,dr\right] = 0.$$

Now, let

$$x_1 = \frac{a_1}{r}, \qquad \frac{w}{\rho_1} = \tfrac{1}{9}\,\beta_1^2\,e^{y_1},$$

and the equation becomes

$$\tfrac{1}{3}\,\beta_1^2\mu M_1\left[-\frac{dy_1}{dx_1} + \int_{x_1}^{\infty}\frac{e^{y_1}}{x_1^4}\,dx_1\right] = 0. \qquad \qquad (13)$$

By differentiation we obtain the equation

$$\frac{d^2y_1}{dx_1^2} + \frac{e^{y_1}}{x_1^4} = 0. \qquad \qquad (14)$$

It is obvious from (13) that $\frac{1}{3}\,\beta_1^2\,M_1\,dy_1/dx_1$ is the mass enclosed inside radius a/x_1, and therefore $\frac{1}{3}\,\beta_1^2\,dy_1/dx_1$ is equal to unity when $x = 1$.

A general analytical solution of (14) does not seem to be attainable, and recourse must be had to numerical processes. Although this is an equation of the second degree, and its general solution must involve two arbitrary constants, we shall see (as pointed out by M. RITTER) that the general solution, as applicable to our problem, may be deduced from one single numerical solution. M. RITTER proceeds by a graphical method, which he has worked with surprising accuracy. I shall therefore adopt an analytical and numerical method, which, although laborious, is susceptible of greater accuracy.

Whatever be the arrangement of the gas, the density at the centre must have some value. I therefore start with a central density ω, corresponding to the value η of y_1, so that

$$\frac{\omega}{\rho_1} = \tfrac{1}{9}\beta_1^2 e^\eta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

For the sake of brevity the suffixes 1 will be now omitted from the various symbols, to be reaffixed later when they are required.

At the centre, where x is infinite, dy/dx, d^2y/dx^2, &c., are all zero, and we put $y = \eta$.

Let $\xi = e^\eta/x^2$, and let us assume

$$\begin{aligned} y &= \eta + v \\ &= \eta - A_1\xi + A_2\xi^2 - A_3\xi^3 + \ldots \end{aligned}$$

Now, the differential equation (14) to be satisfied is

$$x^2\frac{d^2y}{dx^2} = -\frac{e^y}{x^2} = -\xi e^v.$$

But

$$x^2\frac{d^2y}{dx^2} = -2\,.\,3A_1\xi + 4\,.\,5A_2\xi^2 - 6\,.\,7A_3\xi^3 + \ldots,$$

and by expanding e^v we obtain

$$\begin{aligned} -\xi e^v = &-\xi + A_1\xi^2 - (A_2 + \tfrac{1}{2}A_1^2)\,\xi^3 + (A_3 + A_1A_2 + \tfrac{1}{2.3}A_1^3)\,\xi^4 \\ &- (A_4 + A_1A_3 + \tfrac{1}{2}A_2^2 + \tfrac{1}{2}A_1^2A_2 + \tfrac{1}{2.3.4}A_1^4)\,\xi^5 \\ &+ (A_5 + A_1A_4 + A_2A_3 + \tfrac{1}{2}A_1^2A_3 + \tfrac{1}{2}A_1A_2^2 + \tfrac{1}{2.3}A_1^3A_2 + \tfrac{1}{2.3.4.5}A_1^5)\,\xi^6 - \ldots \end{aligned}$$

By equating coefficients in these two series, I find

$$A_1 = \tfrac{1}{6}, \qquad A_2 = \tfrac{1}{120}, \qquad A_3 = \tfrac{1}{1890}, \qquad A_4 = \tfrac{61}{1{,}632{,}960},$$
$$A_5 = \tfrac{629}{224{,}532{,}000}, \qquad A_6 = \tfrac{34\cdot07383\ldots}{156\times10^6}, \quad \text{\&c.,}$$

and

$$\log A_1 = 9\cdot2218487, \qquad \log A_2 = 7\cdot9208188, \qquad \log A_3 = 6\cdot7235382,$$
$$\log A_4 = 5\cdot5723543, \qquad \log A_5 = 4\cdot4473723, \qquad \log A_6 = 3\cdot3392964\,;$$

whence, by extrapolation,

$$\log A_7 = 2\cdot243, \qquad \log A_8 = 1\cdot13.$$

In M. RITTER's paper, already referred to, he takes a certain function u as equal to 1·031, when the radius is unity. Now, RITTER's function u is equal in my notation to

$\frac{1}{2}(w/\rho) \div \frac{1}{9}\beta^2 a^2/r^2$ or $\frac{1}{2}e^y/x^2$. It follows, therefore, that RITTER takes the surface value of $y = \log_e 2{\cdot}062$. But he intends the central density to be 100 times the surface density; hence, to take the same solution, we must have e^η equal to 100 times 2·062. Therefore, his value of η would be

$$\eta = \log_e 100 + \log_e 2{\cdot}062,$$

or

$$\eta = 5{\cdot}3288465.$$

Now, as I want to make a comparison between my solution and his, I start with this value of η. The only object attained by the choice of this particular value is that the two solutions become easily comparable. It will be seen below that the value of η does not make the central density exactly 100 times the surface density, but only satisfies that condition approximately. In RITTER's graphical treatment of the problem this value 100 is the exact datum, whereas in my method we start with an exact value of η, and proceed to find the ratio of central to surface density.

With this value of η (whence $\log_{10} e^\eta = 2{\cdot}3142888$) I find the following series for y:—

$$y = 5{\cdot}3288465 - \frac{34{\cdot}3667}{x^2} + \frac{354{\cdot}321}{x^4} - \frac{4638{\cdot}79}{x^6} + \frac{67{,}532{\cdot}0}{x^8} - \frac{1{,}044{,}280}{x^{10}} + \frac{16{,}789{,}000}{x^{12}} - \frac{2{\cdot}77 \times 10^8}{x^{14}} + \frac{4{\cdot}4 \times 10^9}{x^{16}} - \ldots; \quad . \quad . \quad (16)$$

and, by differentiation, the series for dy/dx is obvious.

This series will be very accurate from $x = \infty$ to about $x = 8$. Thus, when $r/a = {\cdot}1$, or $x = 10$, we have

$$y = 5{\cdot}016558, \qquad \frac{dy}{dx} = +{\cdot}0568910,$$

and even the far less convergent series for d^2y/dx^2 gives $-{\cdot}0150891$, agreeing with $-e^y/x^4$ to the last place of decimals. When $r/a = {\cdot}125$, or $x = 8$, we have*

$$y = 4{\cdot}863925, \qquad \frac{dy}{dx} = {\cdot}101168,$$

whence,

$$\frac{d^2y}{dx^2} = -{\cdot}031624,$$

with y correct to four, and probably to five, places of decimals, and dy/dx probably correct to four places of decimals. This is amply sufficient for our purpose. Indeed, accuracy of this order would be altogether pedantic, were it not that the errors accumulate.

* Even when $x = 5$, I find from this series $y = 4{\cdot}342$, which lies very near to $y = 4{\cdot}332$, found below. But the series for dy/dx is useless.

We cannot, then, rely on this method of procedure beyond the region included between $x = \infty$ and $x = 8$, and must now make a new departure.

Since

$$\frac{d^2y}{dx^2} = -\frac{e^y}{x^4},$$

$$\log\left(-\frac{d^2y}{dx^2}\right) = y - 4\log x;$$

therefore,

$$\frac{d^3y}{dx^3} = \frac{d^2y}{dx^2}\left(\frac{dy}{dx} - \frac{4}{x}\right). \qquad (17)$$

Now, let

$$A_n = \frac{1}{n!}\frac{d^ny}{dx^n},$$

where, after differentiation, x is put equal to c, a constant.

Then (17) may be written—

$$A_3 = \frac{2!}{3!}A_2\left(A_1 - \frac{4}{c}\right). \qquad (18)$$

Now, it is clear that

$$\frac{d^pA_n}{dc^p} = \frac{\overline{n+p}!}{n!}A_{n+p}$$

Hence, differentiating (18) $n - 3$ times, we have

$$\frac{n!}{3!}A_n = \sum_{q=0}^{q=n-3}\frac{\overline{n-3}!}{\overline{n-3-q}!\,q!}\,\frac{2!}{3!}\,\frac{\overline{n-q-1}!}{2!}A_{n-q-1}\left\{\frac{\overline{q+1}!}{1!}A_{q+1} + (-)^q\frac{4(q!)}{c^{q+1}}\right\},$$

or

$$A_n = \frac{1}{n.\overline{n-1}.\overline{n-3}}\sum_{q=0}^{q=n-3}(n-q-1)(n-q-2)A_{n-q-1}\left\{(q+1)A_{q+1} + (-)^q\frac{4}{c^{q+1}}\right\},$$

or

$$A_n = \frac{1}{n.\overline{n-1}.\overline{n-3}}\left\{2.1A_2\left[(n-2)A_{n-2} + (-)^n\frac{4}{c^{n-2}}\right]\right.$$
$$\left. + 3.2A_3\left[(n-3)A_{n-3} - (-)^n\frac{4}{c^{n-3}}\right] + \ldots\right\}. \qquad (19)$$

Now, if, for a given value of x, viz., c, we know y, or A_0, and dy/dx, or A_1, then we can compute A_2 from the formula $-\frac{1}{2}e^{A_0}c^{-4}$; and, by the formula (18), viz. :—

$$A_3 = \frac{1}{3.2.1}\left\{2.1A_2\left[A_1 - \frac{4}{c}\right]\right\},$$

A_3 may be computed.

Afterwards, A_4, A_5, &c., may be computed by successive applications of (19). This being so,

$$y = A_0 + A_1(x-c) + A_2(x-c)^2 + \ldots, \qquad \ldots \quad (20)$$

$$\frac{dy}{dx} = A_1 + 2A_2(x-c) + 3A_3(x-c)^2 + \ldots \; .$$

In these series x may have any value, provided the series converges adequately. The convergence may be much improved by an artifice, which, however, I unfortunately did not discover until most of the computations were completed. Let us add and subtract $\log 2x^2$ on the right-hand side of (20).

Now,

$$\log_e 2x^2 = \log_e 2c^2 + 2\log_e\left[1 + \frac{x-c}{c}\right]$$

$$= \log_e 2c^2 + 2\frac{x-c}{c} - \frac{2}{2}\frac{(x-c)^2}{c^2} + \frac{2}{3}\frac{(x-c)^3}{c^3} - \ldots \; .$$

If, then, we write

$$B_0 = A_0 - \log_e 2c^2, \qquad B_1 = A_1 - \frac{2}{c}, \qquad B_2 = A_2 + \frac{2}{2c^2}, \qquad B_3 = A_3 - \frac{2}{3c^3} \text{ \&c.},$$

we have

$$y = \log_e 2x^2 + B_0 + B_1(x-c) + B_2(x-c)^2 + \ldots, \qquad \ldots \quad (21)$$

a more convergent series than that with the A's.

The simplest way of computing the B's appears to be by first computing the A's.

The process for obtaining the numerical solution is then as follows :—

We have the values of y, dy/dx, $\frac{1}{2}dy^2/dx^2$ when $x = 8$, that is to say, of A_0, A_1, A_2 when $c = 8$. From these the successive A's and B's are computed, and the resulting series gives the values of y and dy/dx when x is 5 or $r = \cdot 2$. Starting from this point a new series gives the result when $r = \cdot 3$, another series gives the values for $r = \cdot 4$, and so on. Later in the calculation several values may be computed from one formula.*

When the computation has been carried out to $r = a$, we have reached the spherical envelope, but that envelope may be replaced by another at any more remote distance from the centre. Thus, the integration may be pursued for values of x less than unity, and when the lower limit is zero the envelope is at infinity.

If we write

$$\log u = B_0 + B_1(x-c) + B_2(x-c)^2 + \ldots,$$

* If the series be carried as far as B_8, several steps may be included in one series. For example, the first series, when $c = 8$, may be pushed even as far as $r = \cdot 4$ without serious error; for it gives $y = 2\cdot 960$ instead of the true value $2\cdot 965$, and $dy/dx = \cdot 957$, instead of the true value $\cdot 944$. I have not, however, been satisfied with this degree of accuracy.

we have

$$e^{y} = u \,.\, 2x^2,$$

and

$$\frac{w}{\rho} = \frac{2}{9}\frac{\beta^2 a^2}{r^2} \cdot u.$$

But it may easily be seen that $2\beta^2 a^2/9r^2$ is a particular solution of the problem; hence, u is a factor by which the particular solution is to be multiplied to obtain the general solution. The function u is given by

$$u = \tfrac{1}{2}\frac{e^y}{x^2} = -\tfrac{1}{2}x^2\frac{d^2y}{dx^2}. \qquad (22)$$

A table of the values of u is given below, showing how the general solution shades off into the particular solution. This function, u, is also tabulated by RITTER, and I made use of its value, when $x = 1$, to determine the value of η, with which the integration is to begin. I find, however (see Table I.), that, when $x = 1$, $u = 1{\cdot}0063$, in place of 1·031, as given by him.

The last row in Table I. gives the ratio of the central density ω to w, the density at the distance r; this ratio is equal to $e^{\eta - y_1}$.

The following Table gives the results of the computation, and the suffix 1 is reintroduced in the several symbols.

I.—Table of Results.

$\frac{1}{x_1} = \frac{r}{a_1} =$	0	·1	·125	·2	·3	·4	·5	·6	·7	·8	·9	1·0
$y_1 =$	5·328845	5·01656	4·8639	4·3317	3·6064	2·9653	2·4235	1·9672	1·5794	1·2458	·9553	·6995
$\frac{dy_1}{dx_1} =$	0·0	·05689	·10117	·2967	·6217	·9442	1·2404	1·5098	1·7569	1·9867	2·2030	2·4087
$\frac{d^2y_1}{dx_1^2} =$	0·0	−·01509	−·03162	−·1217	−·2984	−·4967	−·7053	−·9267	−1·1647	−1·4237	−1·7055	−2·0126
$u = -\frac{1}{2} x_1^2 \frac{d^2y_1}{dx_1^2} =$	0·0	·75445	1·0120	1·5215	1·6577	1·5521	1·4107	1·2871	1·1888	1·1123	1·0528	1·0063
$\frac{w}{w} = e^{\eta - y_1} =$	1·0000	1·3666	..	2·7105	5·598	10·63	18·29	28·84	42·50	59·32	79·33	102·45

$\frac{1}{x_1} = \frac{r}{a_1} =$	1·0	1·1	1·2	1·5	2·0	2·5	3·0	$\infty \left(\frac{1}{x_1}\right)$
$y_1 =$	·6995	·4717	·2672	−·241	−·864	−1·328	−1·699	$-\infty\ (\log 2x_1^2)$
$\frac{dy_1}{dx_1} =$	2·4087	2·6062	2·7971	3·342	4·202	5·139	6·038	$+\infty \left(\frac{2}{x_1}\right)$
$\frac{d^2y_1}{dx_1^2} =$	−2·0126	−2·3466	−2·7089	−3·976	−6·744	−10·35	−14·82	$-\infty \left(-\frac{2}{x_1^2}\right)$
$u = -\frac{1}{2} x_1^2 \frac{d^2y_1}{dx_1^2} =$	1·0063	·9697	·9406	·884	·843	·828	·823	1·0
$\frac{w}{w} = e^{\eta - y_1} =$	102·45	128·7	157·8	263	489	778	1127	$\infty \left(\frac{1}{2} \frac{e^{\eta}}{x_1^2}\right)$

It will be noticed that u rises from zero to a maximum of about 1·66, falls to a minimum of about ·82, and then rises to unity.

Since $\frac{1}{3}\beta_1^2\, dy_1/dx_1 = 1$ when $x_1 = 1$, we have $\frac{1}{3}\beta_1^2 = 1/2{\cdot}4087 = {\cdot}4152$.

M. RITTER has ·4143 for this constant, which he calls m.

It appears from the Table that the density at the centre is $102\frac{1}{2}$ times as great as that where $r = a_1$. M. RITTER's solution is intended to make that ratio exactly 100, but this solution shows that we ought to have started with a slightly different value of η to obtain that result.

In the general solution of the differential equation $d^2y/dx^2 = -e^y/x^4$ the two arbitrary constants may be taken to be the values of y and dy/dx when x is infinite. Now, we have taken arbitrarily $y = 5{\cdot}329$ when x is infinite, and the physical conditions of the problem imply that dy/dx is zero when x is infinite. For if dy/dx had any positive or negative value different from zero, it would mean that at the centre there was a nucleus of infinitely small dimensions, but of finite positive or negative mass. Now, a_1 is that distance from the centre at which the density is 1/102·45 of the central density; hence, we may regard a_1 as the arbitrary constant of the solution. Whatever be the elasticity of the gas, we may always take as our unit of length that distance from the centre of the nebula at which the density has fallen to 1/102·45 of its central value. Hence, the above table gives the general solution of the problem, subject, however, to the condition that there is no central nucleus.

If we view the nebula from a very great distance, a_1 appears very small, and thus the solution of the problem becomes $y = \log 2x^2$. It is easy to verify that this is a particular algebraic solution of the differential equation, as is pointed out by RITTER in his paper.* I found this solution very useful in a preliminary consideration of the problem treated in this paper.

The next point which we have to consider is the form which the solution will take, if, instead of taking a_1 as the unit of length, we take any other value.

The density at any distance and the elasticity are to remain unchanged, but are to be referred to new constants.

Thus, w, r, v^2 remain unchanged, but are to be referred to M, ρ, β^2, a, instead of to M_1, ρ_1, β_1^2, a_1.

Now, since w remains unchanged,

$$\tfrac{1}{9}\beta^2\rho\, e^y = \tfrac{1}{9}\beta_1^2\rho_1 e^{y_1},$$

and, since v^2 remains unchanged,

$$\beta^2\rho\, a^2 = \beta_1^2\rho_1 a_1^2.$$

Also

$$x = \frac{a}{r} = x_1\frac{a}{a_1}\,.$$

* I have made use of this solution in a paper in the 'Proceedings of the Royal Society,' Dec. 3, 1883, and it has also been referred to in a paper by Sir W. THOMSON, 'Phil. Mag.,' vol. 23, p. 287. Sir W. THOMSON's paper covers much the same ground as some of M. RITTER's earlier papers, but was written by him independently and in ignorance of them.

From these relations it is clear that

$$y = y_1 - 2\log\frac{a_1}{a},$$

and

$$M = \tfrac{1}{3}\beta_1^2 M_1 \frac{dy_1}{dx_1}\left(x_1 = \frac{a_1}{a}\right).$$

Then, since $\frac{1}{3}\beta^2\, dy/dx = 1$, when $x = 1$, and since $dy = dy_1$ and $dx = dx_1\, a/a_1$, it follows that

$$\tfrac{1}{3}\beta^2 = \frac{1}{x_1\, dy_1/dx_1}, \text{ when } x_1 = \frac{a_1}{a}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

This relationship has been already used for determining β_1^2.

It is obvious also that

$$\frac{\rho}{\rho_1} = \frac{x_1^3\, dy_1/dx_1, \text{ when } x_1 = a_1/a}{x_1^3\, dy_1/dx_1, \text{ when } x_1 = 1}.$$

Therefore,

$$\frac{w}{\rho} = \frac{\frac{1}{3}e^{y_1}}{x_1^3\, dy_1/dx_1, \text{ when } x_1 = a_1/a}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

If w_0 be the density when $r = a$, we have

$$\frac{w_0}{\rho} = \frac{\frac{1}{3}e^{y_1}, \text{ when } x_1 = a_1/a}{x_1^3\, dy_1/dx_1} = -\tfrac{1}{3}\,.\,\frac{x_1\, d^2y_1/dx_1^2}{dy_1/dx_1}, \text{ when } x_1 = a_1/a. \quad . \quad (25)$$

If p_0 be the pressure when $r = a$, we have

$$p_0 = \tfrac{1}{3}v^2 w_0 = \tfrac{4}{9}\pi\mu a^2\rho^2\,.\,\beta^2\frac{w_0}{\rho}.$$

If, therefore, we write $P = \frac{4}{3}\pi\mu a^2\rho^2$,

$$\frac{p_0}{P} = \tfrac{1}{3}\beta^2\,.\,\frac{w_0}{\rho} = -\frac{d^2y_1/dx_1^2}{(dy_1/dx_1)^2}, \text{ when } x_1 = a_1/a. \quad . \quad . \quad . \quad . \quad (26)$$

By (26) we are able to find how the pressure on an envelope of given radius a varies with the variation of the temperature of a given mass M of gas contained in it. By means of the formulæ (23), (25), (26), we are now able to obtain from the original solution any number of other ones; for, after the changes have been effected in the notation, we may proceed to magnify or diminish all the various values of a until they are of one size, and we shall thus obtain the solution for a gas at any temperature whatever.

I shall now proceed to give a table of results when the standard radius a, which may be conveniently called the boundary, is placed successively infinitely near the centre, where $r = 0 \times a_1$, at $r = \cdot 1 \times a_1$, $r = \cdot 2 \times a_1$, and so on. The first line of entries gives the various values of $\frac{1}{3}\beta^2$ (computed from (23)), on which the elasticity of the gas depends; the second line gives w_0/ρ (computed from (25)), or the ratio of the boundary density to the mean density of all inside of it; the third line gives p_0/P (computed from (26)), by which we trace the variations of pressure at the boundary.

TABLE II.

Value of a by reference to former solution $\frac{a}{a_1} =$	0	·1	·2	·3	·4	·5	·6	·6264	·7	·8
$\frac{\frac{1}{3}v^2}{\mu M/a} = \frac{1}{3}\beta^2 \left[= \frac{1}{x_1\, dy_1/dx_1} \right] =$	∞	1·7577	·6741	·4826	·4236	·4031	·3974	·3972	·3984	·4027
$\frac{w_0}{\rho} \left[= \frac{-\frac{1}{3}x_1\, d^2y_1/dx_1^2}{dy_1/dx_1} \right] =$	1·0000	·8841	·6838	·5333	·4383	·3791	·3410	$\frac{1}{3}$	·3158	·2986
$\frac{p_0}{P} \left[= \frac{-d^2y_1/dx_1^2}{[dy_1/dx_1]^2} \right] =$	∞	4·662	1·383	·772	·557	·458	·407	·397	·377	·361

Value of a by reference to former solution $\frac{a}{a_1} =$	·9	1·0	1·25	1·5	2·0	2·5	3·0	∞
$\frac{\frac{1}{3}v^2}{\mu M/a} = \frac{1}{3}\beta^2 \left[= \frac{1}{x_1\, dy_1/dx_1} \right] =$	·4085	·4152	·4325	·449	·476	·487	·497	$\frac{1}{2}$
$\frac{w_0}{\rho} \left[= \frac{-\frac{1}{3}x_1\, d^2y_1/dx_1^2}{dy_1/dx_1} \right] =$	·2867	·2785	·2676	·264	·267	·269	·273	$\frac{1}{3}$
$\frac{p_0}{P} \left[= \frac{-d^2y_1/dx_1^2}{[dy_1/dx_1]^2} \right] =$	·351	·347	·347	·356	·382	·392	·406	$\frac{1}{2}$

The minimum value of w_0/ρ occurs when $a/a_1 = 1·6$ very nearly, for, when $a/a_1 = 1·4$, $1·5$, $1·6$, I find $w_0/\rho = ·26521$, $·26437$, $·26425$ respectively.* When $r/a_1 = 1·6$, $y_1 = -·38435$ and $dy_1/dx_1 = 3·5180$. The minimum value of p_0/P occurs when $a/a_1 = 1·1$ very nearly, for, when $a/a_1 = 1·0$, $1·1$, $1·2$, I find $p_0/P = ·3469$ $·3455$, $·3462$ respectively.

When w_0/ρ is a minimum, the density at the centre is 381 times that at the boundary, and, when p_0/P is a minimum, the density at the centre is 129 times that at the boundary. M. RITTER finds the pressure to be a minimum when this ratio is 258, instead of 129. As this corresponds to $a/a_1 = 1·5$, this discrepancy between our solutions is not so large as might be expected from the great discrepancy between these results, and I cannot but think that my result is more accurate than his.

The minimum value of $\frac{1}{3}\beta^2$ occurs when $a/a_1 = ·6264$, and its value is $·39723$. This value makes the surface density exactly one-third of the mean density, for $\frac{1}{3}\beta^2$ is a minimum when $x_1\, dy_1/dx_1$ is a maximum, and this occurs when $x_1 d^2y_1/dx_1^2 + dy_1/dx_1 = 0$; and, when this relationship is satisfied, $w_0/\rho = \frac{1}{3}$.

It is interesting to note that in this case β^2 is very nearly equal to $\frac{6}{5}$, so that the

* Mr. HILL finds that the minimum value of w_0/ρ approximates to $\frac{4}{15}$, or $·2667$. The agreement between our results is satisfactory.

total internal kinetic energy of agitation of the sphere of gas at minimum temperature limited by the radius a is $\frac{1}{2}\left(\frac{6}{5}\mu M/a\right) M = \frac{3}{5}\mu M^2 a$ *very nearly.* Now, the energy lost in the concentration of a homogeneous sphere M from a condition of infinite dispersion is *exactly* $\frac{3}{5}\mu M^2/a$. It might, therefore, be suspected that ·39723 is only an approximation to $\frac{2}{5}$, which may be the rigorous value. But my numerical calculations were carried out with so much care that I find it almost impossible to believe that there is an error as large as 3 in the third place of decimals, or, indeed, any error at all in the third figure. Moreover, it would be expected that, if this very simple relationship is rigorously correct, it would be possible to prove it rigorously, just as it is rigorously shown above that $w_0/\rho = \frac{1}{3}$; but I am unable to find any analytical relationships by which the minimum value of $\frac{1}{3}\beta^2$ can be deduced. If my arithmetical process be correctly carried out, then we ought to find that, when $r =$ ·6264, dy_1/dx_1 should be equal to $- x_1 d^2y_1/dx_1^2$ or e^{y_1}/x_1^3. Now, I find that, when $r =$ ·6264, $dy_1/dx_1 = 1{\cdot}57703$ and $e^{y_1}/x_1^3 = 1{\cdot}5770$, so that the two agree to four places of decimals. I conclude, therefore, that the true minimum of $\frac{1}{3}\beta^2$ is ·3972.*

It will be observed that, as a/a_1 increases to infinity, $\frac{1}{3}\beta^2$ terminates by being equal to $\frac{1}{2}$. M. RITTER has found that it rises above $\frac{1}{2}$, and oscillates about that value an indefinite number of times with diminishing amplitude, gradually settling down to $\frac{1}{2}$ as a/a_1 becomes infinite. The values in the preceding table are not, however, carried far enough to exhibit these oscillations of $\frac{1}{3}\beta^2$. A consequence of this result is that there are a number of modes of equilibrium of a gas at a given temperature, provided that the temperature lies within certain narrow limits. This very remarkable conclusion is rendered more intelligible by Mr. HILL's treatment than by M. RITTER's. This point has, however, no bearing on the present investigation.

In any one of the solutions comprised in Table II. we may complete the table of densities by the formula (24), viz.,

$$\frac{w}{\rho} = \frac{\frac{1}{3}e^{y_1}}{x_1^3\, dy_1/dx_1\, (x_1 = a_1/a)},$$

and I shall later proceed to do this in the one case which has interest for our present problem, namely, where the temperature is a minimum, so that a/a_1 is ·6264. The full numerical results may be more conveniently given hereafter, and it will only be now necessary to indicate how they are to be computed.

When, for example, $r = {\cdot}1 \times a_1$, $r/a = {\cdot}1/{\cdot}6264 = {\cdot}1596$; thus, our equidistant values of the density and other functions will proceed by multiples of ·1596 a up to ·9578 a, and the limit of the isothermal sphere is where $r = a$.

When the temperature is a minimum $\frac{1}{3}\beta^2 =$ ·39723, and we have $w_0 = \frac{1}{3}\rho$; therefore, $w/w_0 = w/\frac{1}{3}\rho$, and, therefore, if $y_{1,0}$ be the value of y_1, when $r = {\cdot}6264\, a_1$,

* This is confirmed by Mr. HILL. His equation $s = z$ is equivalent to $x_1^2 d^2y_1/dx_1^2 + x_1 dy_1/dx_1 = 0$, and it appears from his tables that $s = z = 2{\cdot}517$. Now, $s = 3/\beta^2$, and the reciprocal of $2{\cdot}517$ is ·397.

$w/\frac{1}{3}\rho = e^{y_1 - y_{1,0}}$. Thus, for example, at the centre, $w/\frac{1}{3}\rho$ is 32·14, and when $r = \cdot 4789\, a$ it is 5·7417.

The proportion of the mass M which is included in radius a/x is $\frac{1}{3}\beta^2\, dy/dx = \frac{1}{3}\beta^2 a_1\, dy_1/a\, dx_1 = \frac{\cdot 39723}{\cdot 6264}\, dy_1/dx_1$. Hence, the masses may be computed.

At any part of the isothermal sphere gravity g is to be found from

$$g = \tfrac{1}{3}\beta^2 \mu M \frac{dy}{dx} \cdot \frac{x^2}{a^2};$$

or, expressing g in terms of G gravity at the surface, we have, since $G = \mu M/a^2$,

$$\frac{g}{G} = \tfrac{1}{3}\beta^2 x^2 \frac{dy}{dx}. \qquad (27)$$

The angular velocity of a body moving in a circular orbit at any part of the nebula, and its linear velocity v are also easily to be found.

§ 5. *On an Atmosphere in Convective Equilibrium.*

I shall now suppose that a sphere of gas of mass M at minimum temperature is bounded by an atmosphere in convective equilibrium, with continuity of temperature and density at the sphere of discontinuity of radius a. Let v_0^2 be the mean square of velocity of agitation in the isothermal sphere, and v^2 that at any other radius r. Then throughout the isothermal sphere $v^2 = v_0^2$, but in the layer outside v^2 gradually decreases to zero.

Let w_0 be the density and p_0 the pressure at radius a, and w, p the same things at radius r.

Then, if the ratio of the two specific heats be that deduced from the simple kinetic theory of gases, without any allowance for intra-molecular vibrations, we have that ratio equal to $\frac{5}{3}$.

Hence,

$$p = p_0 \left(\frac{w}{w_0}\right)^{\frac{5}{3}},$$

and

$$\tfrac{1}{3} v^2 = p_0 \frac{w^{\frac{2}{3}}}{w_0^{\frac{5}{3}}} = \tfrac{1}{3} v_0^2 \cdot \left(\frac{w}{w_0}\right)^{\frac{2}{3}},$$

also

$$\frac{dp}{w} = \tfrac{5}{3} \cdot \frac{p_0}{w_0^{\frac{5}{3}}} w^{-\frac{1}{3}}\, dw = \tfrac{5}{2} \cdot \tfrac{1}{3} v_0^2\, d\left(\frac{w}{w_0}\right)^{\frac{2}{3}}.$$

Now, the equation for the hydrostatic equilibrium of the layer is

$$\frac{r^2\, dp}{w\, dr} + \mu M + 4\pi\mu \int_a^r w r^2\, dr = 0. \qquad (28)$$

Let

$$x = \frac{a}{r}, \qquad z = \frac{v^2}{v_0^2} = \left(\frac{w}{w_0}\right)^{\frac{2}{3}},$$

and we have

$$\frac{r^2\,dp}{w\,dr} = -\tfrac{5}{2}\cdot\tfrac{1}{3}\,v_0^2\,a\,\frac{dz}{dx},$$

$$\mu M = \frac{v_0^2\,a}{\beta^2},$$

$$4\pi\mu a^3 = \frac{3\mu M}{\rho} = \frac{\mu M}{w_0}, \text{ since } w_0 = \tfrac{1}{3}\rho \text{ rigorously,}$$

$$= \frac{v_0^2 a}{\beta^2 w_0}.$$

Hence, our equation is

$$\mu M\left\{-\tfrac{5}{6}\beta^2\frac{dz}{dx} + 1 + \int_x^1 \frac{z^{\frac{3}{2}}}{x^4}\,dx\right\} = 0. \qquad (29)$$

It is obvious the $\frac{5}{6}\beta^2 M\,dz/dx$ is the whole mass (expressed in terms of the mass of the isothermal sphere) enclosed inside of radius a/x. The differential equation to be satisfied is

$$\tfrac{5}{6}\beta^2\frac{d^2z}{dx^2} + \frac{z^{\frac{3}{2}}}{x^4} = 0. \qquad (30)$$

We have seen in the last section that $\frac{1}{3}\beta^2 = \cdot 39723$, and, hence, $\frac{5}{6}\beta^2 = \cdot 99308$.

This equation is not so easy to solve as that in the last section, and I have not succeeded in finding the general law of the coefficients in an expansion. Nevertheless it is easy to find a series which will do all that is required.

Let c be any value of x for which we know z and dz/dx, and let

$$\xi = x - c.$$

Assume

$$z = z_0\{1 + A_1\xi + A_2\xi^2 + A_3\xi^3 + \ldots\}.$$

Then, if the suffix 0 indicates the value of a symbol when $x = c$ and $\xi = 0$, we have

$$z_0 = z_0,$$

$$\left(\frac{dz}{dx}\right)_0 = A_1 z_0,$$

$$\left(\frac{d^2z}{dx^2}\right)_0 = 2A_2 z_0.$$

But

$$\left(\frac{d^2z}{dx^2}\right)_0 = -\frac{6}{5\beta^2}\cdot\frac{z_0^{\frac{3}{2}}}{c^4},$$

and

$$2A_2 z_0 = -\frac{6}{5\beta^2} \cdot \frac{z_0^{\frac{3}{2}}}{c^4}, \qquad \text{or} \qquad A_2 = -\frac{3}{5\beta^2} \frac{z_0^{\frac{1}{2}}}{c^4},$$

so that, if z_0 and $(dz/dx)_0$ are known, A_2 is known.

The differential equation (30) which we have to satisfy is

$$\tfrac{5}{6}\beta^2 (\xi + c)^4 \frac{d^2 z}{d\xi^2} = -z^{\frac{3}{2}},$$

or

$$\frac{1}{A_2}\left(\frac{\xi}{c} + 1\right)^4 \frac{d^2(z/z_0)}{d\xi^2} = 2\left(\frac{z}{z_0}\right)^{\frac{3}{2}}.$$

Now, by expansion,

$$\begin{aligned}
2\left(\frac{z}{z_0}\right)^{\frac{3}{2}} = {} & 2 + 3A_1\xi + 3\left[A_2 + \tfrac{1}{4}A_1^2\right]\xi^2 + 3\left[A_3 + \tfrac{1}{2}A_1A_2 - \tfrac{1}{24}A_1^3\right]\xi^3 \\
& + 3\left[A_4 + \tfrac{1}{2}(A_1A_3 + \tfrac{1}{2}A_2^2) - \tfrac{1}{8}A_1^2A_2 + \tfrac{1}{64}A_1^4\right]\xi^4 \\
& + 3\left[A_5 + \tfrac{1}{2}(A_1A_4 + A_2A_3) - \tfrac{1}{8}(A_1^2A_3 + A_1A_2^2) + \tfrac{1}{16}A_1^3A_2 - \tfrac{1}{128}A_1^5\right]\xi^5 \\
& + 3\left[A_6 + \tfrac{1}{2}(A_1A_5 + A_2A_4 + \tfrac{1}{2}A_3^2) - \tfrac{1}{8}(A_1^2A_4 + 2A_1A_2A_3 + \tfrac{1}{3}A_2^3)\right. \\
& \qquad \left. + \tfrac{1}{16}(A_1^3A_3 + \tfrac{3}{2}A_1^2A_2^2) - \tfrac{5}{128}A_1^4A_2 + \tfrac{7}{1536}A_1^6\right]\xi^6 \\
& + 3\left[A_7 + \tfrac{1}{2}(A_1A_6 + A_2A_5 + A_3A_4) - \tfrac{1}{8}(A_1^2A_5 + 2A_1A_2A_4 + A_1A_3^2 + A_2^2A_3)\right. \\
& \qquad + \tfrac{1}{16}(A_1^3A_4 + 3A_1^2A_2A_3 + A_1A_2^3) - \tfrac{5}{128}(A_1^4A_3 + 2A_1^3A_2^2) \\
& \qquad \left. + \tfrac{1}{256}A_1^5A_2 - \tfrac{3}{1024}A_1^7\right]\xi^7 + \ldots \qquad (31)
\end{aligned}$$

And

$$\begin{aligned}
\frac{1}{A_2}\left(\frac{\xi}{c}+1\right)^4 \frac{d^2}{d\xi^2}(z/z_0) = {} & 2 + \left(3\,.\,2\,\frac{A_3}{A_2} + \frac{4}{c}\,.\,2\,.\,1\right)\xi + \left(4\,.\,3\,.\,\frac{A_4}{A_2} + \frac{4}{c}\,.\,3\,.\,2\,\frac{A_3}{A_2} + \frac{6}{c^2}\,.\,2\,.\,1\right)\xi^2 \\
& + \left(5\,.\,4\,\frac{A_5}{A_2} + \frac{4}{c}\,.\,4\,.\,3\,\frac{A_4}{A_2} + \frac{6}{c^2}\,.\,3\,.\,2\,\frac{A_3}{A_2} + \frac{4}{c^3}\,.\,2\,.\,1\right)\xi^3 \\
& + \left(6\,.\,5\,\frac{A_6}{A_2} + \frac{4}{c}\,.\,5\,.\,4\,\frac{A_5}{A_2} + \frac{6}{c^2}\,.\,4\,.\,3\,\frac{A_4}{A_2} + \frac{4}{c^3}\,.\,3\,.\,2\,\frac{A_3}{A_2} + \frac{1}{c^4}\,.\,2\,.\,1\right)\xi^4 \\
& + \left(7\,.\,6\,\frac{A_7}{A_2} + \frac{4}{c}\,.\,6\,.\,5\,\frac{A_6}{A_2} + \frac{6}{c^2}\,.\,5\,.\,4\,\frac{A_5}{A_2} + \frac{4}{c^3}\,.\,4\,.\,3\,\frac{A_4}{A_2} + \frac{1}{c^4}\,.\,3\,.\,2\,\frac{A_3}{A_2}\right)\xi^5 \\
& + \ldots \qquad (32)
\end{aligned}$$

By equating the coefficients in (31) and (32) we are able to determine the A's. The law of the series (32) is obvious, and sufficient of the series (31) is written down to enable us to find A_9. We can, however, obtain a good approximation to higher coefficients, because the coefficients in (31) become relatively unimportant.

We now begin the solution with

$$c = 1, \qquad z_0 = 1, \qquad \left(\frac{dz}{dx}\right)_0 = \frac{1}{\frac{5}{6}\beta^2} = 1\cdot0070, \qquad \left(\frac{d^2z}{dx^2}\right)_0 = -\frac{1}{\frac{5}{6}\beta^2} = -1\cdot0070.$$

Hence,

$$A_1 = 1\cdot0070, \qquad A_2 = -\cdot5035,$$

whence I compute

$$A_3 = +\cdot41782, \quad A_4 = -\cdot30068, \quad A_5 = +\cdot16175, \quad A_6 = -\cdot01306, \quad A_7 = -\cdot1333,$$
$$A_8 = +\cdot266, \quad A_9 = -\cdot378, \quad A_{10} = +\cdot48, \quad A_{11} = -\cdot6.$$

With these coefficients I find

$$\left.\begin{array}{llllll} \dfrac{r}{a} = \frac{12}{11}, & \frac{12}{10}, & \frac{12}{9}, & \frac{12}{8}, & \frac{12}{7}, & \frac{12}{6} \\ z = \cdot9123 & \cdot8160 & \cdot7089 & \cdot5887 & \cdot4525 & \cdot2982 \end{array}\right\} \quad . \quad . \quad (33)$$

Then, evaluating $x^{-4}z^{\frac{3}{2}}$, and combining the several values by the rules for integration of the calculus of finite differences, I find

$$\left.\begin{array}{llllll} \dfrac{r}{a} = \frac{12}{11}, & \frac{12}{10}, & \frac{12}{9}, & \frac{12}{8}, & \frac{12}{7}, & \frac{12}{6} \\ \frac{5}{6}\beta^2 \dfrac{dz}{dx} = \ldots & 1\cdot21 & 1\cdot35 & 1\cdot527 & 1\cdot729 & 1\cdot9513 \end{array}\right\} \quad . \quad . \quad (34)$$

When $r = 2$, we begin a new series with

$$c = \tfrac{1}{2}, \qquad z_0 = \cdot2982, \qquad A_1 = \left(\frac{1}{z}\frac{dz}{dx}\right)_0 = \frac{1\cdot9513}{\cdot9907 \times \cdot2982} = +6\cdot5894,$$
$$A_2 = \left(\frac{1}{2z}\frac{d^2z}{dx^2}\right)_0 = \frac{-z_0^{\frac{1}{2}}}{2(\frac{1}{2})^4} = -4\cdot3686.$$

From these I compute $A_3 = -2\cdot744$, $A_4 = +21\cdot365$, $A_5 = -45\cdot409$, $A_6 = +9\cdot932$, $A_7 = +\cdot319$.

It appears that z vanishes when $x - c = -\cdot141$ or $x = \cdot359$.

It follows, therefore, that four equidistant values of x lying between $r = 2a$ and $r = a/\cdot359 = 2\cdot786\,a$ correspond to $x - c = 0$, $x - c = -\cdot047$, $x - c = -\cdot094$, $x - c = -\cdot141$.

For the first of these, where $r = 2\,a$, we have $z = \cdot2982$, and for the last, where $r = 2\cdot786\,a$, $z = 0$; and, when $x - c = -\cdot047$, or $r = a/\cdot453 = 2\cdot208\,a$, I find $z = \cdot2031$; and, when $x - c = -\cdot094$, or $r = a/\cdot406 = 2\cdot463\,a$, I find $z = \cdot1033$.

Finding $x^{-4}z^{\frac{3}{2}}$ for these four values and combining them by the rules of integration, I find

$$\tfrac{5}{6}\beta^2 \frac{dz}{dx} = 2{\cdot}1767, \quad \text{when } r = 2{\cdot}786\, a. \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

We thus see that the mass of the whole system is 2·1767 times the mass of the isothermal nucleus, and its radius is 2·786 times the radius of the nucleus.

The mass of the isothermal nucleus is thus 46 per cent. of the whole. M. RITTER, taking the ratio of the specific heats as $\frac{7}{5}$ instead of $\frac{5}{3}$, says that the proportion is about 40 per cent.

§ 6. *On a Gaseous Sphere in "Isothermal-Adiabatic" Equilibrium.*

M. RITTER calls a sphere, with isothermal nucleus and a layer in convective equilibrium above it, a case of isothermal-adiabatic equilibrium. Since the height of an atmosphere in convective equilibrium depends only on the temperature at the base, and since the isothermal nucleus in our numerical example is at minimum temperature, the thickness of the adiabatic layer is a minimum, and the isothermal nucleus a maximum.

We are now in a position to collect together all the numerical results of the last two sections in a form appropriate for our subsequent investigation. It will be convenient to refer all the densities and masses to the mean density and mass of the isothermal nucleus. Gravity may also be referred to gravity G at the limit of the isothermal nucleus, and velocity to v_0^2, the mean square of velocity of agitation in the isothermal nucleus.

TABLE III.—Isothermal-Adiabatic Sphere.

Radius $\frac{r}{a} =$	0	·1596	·3193	·4789	·6385	·7982	·9578	1	1·0909	1·2	1·3333	1·5	1·7143	2·0	2·208	2·463	2·786
Square of velocity of agitation $\frac{v^2}{{v_0}^2} =$	1	1	1	1	1	1	1	1	·912	·816	·709	·589	·452	·298	·203	·103	0
Density $\frac{w}{\frac{1}{3}\rho} =$	32·14	23·52	11·86	5·742	3·024	1·759	1·115	1	·871	·737	·597	·452	·304	·163	·092	·033	0
Mass in terms of $M =$	0	·0361	·1881	·3942	·5988	·7866	·9574	1	..	1·207	1·349	1·527	1·729	1·951	..	..	2·177
Gravity $\frac{g}{G} =$	0	1·4156	1·8467	1·7188	1·4685	1·2346	1·0436	1	..	·8385	·7589	·6785	·5882	·4878	..	..	·28
Square of velocity of satellite $\frac{v^2}{{v_0}^2} =$	0	·1896	·4945	·6908	·7869	·8269	·8388	·8391	..	·844	·849	·854	·846	·818	..	..	·66
$\log \frac{\frac{1}{3}\rho}{w} =$	8·4929	8·6286	8·9260	9·2410	9·5194	9·7547	9·9529	·0000	·0598	·1325	·2241	·3452	·5166	·7882	1·0385	1·4791	∞
$\log \frac{1}{[w/\frac{1}{3}\rho]\,[v/v_0]} =$	8·4929	8·6286	8·9260	9·2410	9·5194	9·7547	9·9529	·0000	·0797	·1766	·2988	·4603	·6889	1·0510	1·3846	1·9722	∞
$\log F_1 = \log \left[\frac{3\pi}{8\beta^2} \frac{g/G}{[v^2/{v_0}^2]\,[w/\frac{1}{3}\rho]}\right] =$	$-\infty$	8·7745	9·1872	9·4712	9·6813	9·8413	9·9664	9·9950	..	·1393	·2487	·4019	·6256	·9970	..	..	∞
$\log F_2 = \log \left[\frac{1}{\pi} \frac{[g/G]^{\frac{1}{2}}\,x^{\frac{1}{2}}}{[v/v_0]\,[w/\frac{1}{3}\rho]}\right] =$	$-\infty$	8·6053	8·8098	9·0213	9·2031	9·3523	9·4744	9·5029	..	9·5967	9·6793	9·7909	9·9594	·2475	..	..	∞

§ 7. *On the Kinetic Energy of Agitation and its Distribution in an Isothermal-Adiabatic Sphere of Gas.*

We shall now consider what would be the distribution of kinetic energy in the nebula if each meteorite (or molecule) were to fall from infinity to the neighbourhood where we find it, and were to retain that energy afterwards. This will give the distribution of energy in a swarm of the supposed arrangement of density, if the rate of diffusion of kinetic energy were to be infinitely slow, and if there were no loss of energy through imperfect elasticity.

The square of the velocity of a satellite in a circular orbit is one half of the square of the velocity acquired by the fall from infinity to the distance of the satellite from the centre. If the concentration has proceeded as far as radius r, and if a meteorite falls from infinity to distance r, then, if U be its velocity, and v the velocity in a circular orbit at distance r,

$$\tfrac{1}{2} U^2 = v^2 = \tfrac{1}{3} \beta^2 \frac{\mu M}{a} . x \frac{dy}{dx} = \tfrac{1}{3} v_0^2 x \frac{dy}{dx}, \text{ in the isothermal sphere,}$$

$$= \tfrac{5}{6} \beta^2 \frac{\mu M}{a} . x \frac{dz}{dx} = \tfrac{5}{6} v_0^2 x \frac{dz}{dx}, \text{ in the adiabatic layer.}$$

In these formulæ, by the definitions of y and z,

$$y = \log_e \left(\frac{9w}{\beta^2 \rho}\right) \text{ in the first, and } z = \left(\frac{w}{w_0}\right)^{\frac{2}{5}} \text{ in the second.}$$

From these formulæ v^2 was computed in Table III. The value of v^2 or $\frac{1}{2}U^2$ gives what may be called the theoretical value of the kinetic energy, because it gives us a measure of the amount of redistribution of energy by diffusion and loss of energy by imperfect elasticity, which must take place before the whole system can assume the form of an isothermal adiabatic sphere.

We will now go on to consider the total potential energy lost in condensation.

We have seen that the potential energy lost by the fall of a single meteorite is $\frac{1}{3}v_0^2 x\, dy/dx$ in the isothermal part, and $\frac{5}{6}v_0^2 x\, dz/dx$ in the outer part.

Now, in the isothermal part a spherical element of mass is

$$-\tfrac{1}{3} M\beta^2 . \frac{d^2y}{dx^2}\, dx,$$

and the energy lost by its fall is

$$-\tfrac{1}{9} M\beta^2 v_0^2 . x \frac{dy}{dx} \frac{d^2y}{dx^2}\, dx.$$

Hence, the whole energy lost in the concentration of the isothermal nucleus is

$$\tfrac{1}{9}M\beta^2 v_0^{\,2} \,.\int_1^\infty x \frac{dy}{dx}\frac{d^2y}{dx^2}\,dx.$$

But

$$\begin{aligned}
-\int_1^\infty x \frac{dy}{dx}\frac{d^2y}{dx^2}\,dx &= \int_1^\infty \frac{e^y}{x^3}\frac{dy}{dx}\,dx = 3\int_1^\infty \frac{e^y}{x^4}\,dx - e^{y_0} \\
&= 3\left(\frac{dy}{dx}\right)_0 - e^{y_0} \\
&= \frac{9}{\beta^2} - \frac{9w_0}{\beta^2\rho}.
\end{aligned}$$

Hence, the energy lost is $Mv_0^{\,2}\left(1 - \dfrac{w_0}{\rho}\right)$. But in an isothermal sphere of minimum temperature $w_0 = \frac{1}{3}\rho$, and thus the total lost energy is $\frac{2}{3}Mv_0^{\,2}$.

Again, in the adiabatic layer an element of mass is

$$-\tfrac{5}{6}M\beta^2 \frac{d^2z}{dx^2}\,dx = +\,M\frac{z^{\frac{3}{2}}}{x^4}\,dx,$$

and, therefore, the energy lost by its fall from infinity is

$$\tfrac{5}{6}Mv_0^{\,2} \,.\, \frac{z^{\frac{3}{2}}}{x^3}\frac{dz}{dx}\,dx,$$

and the whole loss of energy is the integral of this from $x = 1$ to $x = \cdot 359$. When $x = 1$, $z = 1$, and when $x = \cdot 359$, $z = 0$. Hence

$$\int_{\cdot 359}^1 \frac{z^{\frac{3}{2}}}{x^3}\frac{dz}{dx}\,dx = \tfrac{2}{5} + \tfrac{6}{5}\int_{\cdot 359}^1 \frac{z^{\frac{5}{2}}}{x^4}\,dx.$$

Thus, the whole energy lost in the adiabatic layer is

$$Mv_0^{\,2}\left[\tfrac{1}{3} + \int_{\cdot 359}^1 \frac{z^{\frac{5}{2}}}{x^4}\,dx\right].$$

Add this to the energy found before for the isothermal part, and the whole lost energy of the system is found to be

$$Mv_0^{\,2}\left[1 + \int_{\cdot 359}^1 \frac{z^{\frac{5}{2}}}{x^4}\,dx\right]. \qquad \qquad (36)$$

Now let us evaluate the total kinetic energy existing in the form of agitation of molecules.

In the isothermal part it is clearly $\frac{1}{2}Mv_0^2$. In the adiabatic part it is half the element of mass into the square of velocity of agitation integrated through the layer, that is to say, $\frac{1}{2} . M \frac{z^{\frac{3}{2}}}{x^4} dx \times v^2$, and, since $z = \frac{v^2}{v_0^2}$, we have

$$\frac{1}{2}Mv_0^2 \left[1 + \int_{\cdot 359}^{1} \frac{z^{\frac{5}{2}}}{x^4} dx \right]$$

for the total internal kinetic energy of agitation. This is rigorously one-half of the energy lost in concentration.

Hence, if a meteor swarm concentrates into this arrangement of density, one half of the original energy is occupied in vaporising and heating parts of the meteorites on impact, and the other half is retained as kinetic energy of agitation.

I find by quadrature that $\int_{\cdot 359}^{1} \frac{z^{\frac{5}{2}}}{x^4} dx = \cdot 643$. Hence, the potential energy lost in concentration is $Mv_0^2 (1 \cdot 643)$, and that part of it which is retained as energy of agitation is $\frac{1}{2}Mv_0^2 (1 \cdot 643)$. The whole mass of the system is $2 \cdot 1767\, M$, and we may, therefore, write these

$$\cdot 7548\, (2 \cdot 1767\, M)\, v_0^2 \quad \text{and} \quad \tfrac{1}{2} \times \cdot 7548\, (2 \cdot 1767\, M)\, v_0^2.$$

It is clear then that the average mean square of velocity of agitation of the *whole* system is $\cdot 7548\, v_0^2$.* Or, shortly, the average temperature is very nearly $\frac{3}{4}$ of the temperature of the isothermal nucleus.

It follows from this whole investigation that for any given mass of matter, arranged in an isothermal-adiabatic sphere of given dimensions, the actual velocities of agitation are determinable throughout.

§ 8. *On the "Sphere of Action."*

When two meteorites pass near to one another, each will be deflected from its straight path by the attraction of the other. The question arises as to whether the amount of such deflection can be so great that the passage of two meteorites near to one another ought to be estimated as an encounter in the kinetic theory.

We shall now, therefore, find the deflection of two meteorites, moving with the mean relative velocity, when they pass so close as just to graze one another.

The mean square of relative velocity in the isothermal portion is $2v^2_0$, and this may be taken as the square of the velocity at infinity in the relative hyperbola described The angle between the asymptotes of the hyperbola is the deflection due to this sort of encounter.

Let a, ϵ be the semi-axis and eccentricity of the hyperbola. Then, if ϵ be large, the

* M. RITTER gives ·741 in place of ·755, but, as already remarked, he uses a different value for the ratio of the specific heats.

angle between the asymptotes is $1/\epsilon$; and, if $\frac{1}{2}s$ be the radius of either meteorite, the pericentral distance (when they graze) is s. Therefore,

$$s = a(\epsilon - 1).$$

By the law of central orbits

$$2v_0^2 = \frac{\mu m}{a}.$$

Therefore,

$$\epsilon = \frac{2v_0^2 s}{\mu m} + 1.$$

But, since $v_0^2 = \beta^2 \mu M/a$, we have

$$\epsilon = 2\beta^2 \frac{Ms}{ma} + 1.$$

The unity on the right-hand side is negligible, and, since $180/\pi\epsilon$ is the deflection in degrees, that deflection is

$$\frac{180}{2\pi\beta^2} \frac{ma}{Ms} \text{ degrees.}$$

Now, if δ be the density of the body of a meteorite, $m = \frac{1}{6}\pi\delta s^3$, and, therefore, this expression becomes

$$\frac{15^\circ}{\beta^2} \times \frac{\delta a s^2}{M}.$$

Let us find what s must be if the deflection is 10°; we have

$$s = \beta \sqrt{\frac{2M}{3a\delta}}.$$

We may, for a rough evaluation, take β as unity instead of $\sqrt{6/5}$, and suppose a to be equal to the distance of Neptune from the Sun (viz., $4{\cdot}5 \times 10^{14}$ cm.), and, as a very high estimate of the value of δ, let us suppose the density of a meteorite is 10. Then, since the Sun, $M_0 = 2 \times 10^{33}$ grammes, and M is about a half of the Sun's mass, we have

$$s = \left[\frac{2 \times 10^{33}}{3 \times 4{\cdot}5 \times 10^{14} \times 10}\right]^{\frac{1}{2}} = (15 \times 10^{16})^{\frac{1}{2}} = 4 \times 10^8.$$

Hence, $m = \frac{1}{6}\pi\delta s^3 = \frac{1}{6}\pi \times 10 \times 64 \times 10^{24} = 3 \times 10^{26}$ grammes, in round numbers.

But the Earth's mass is 6×10^{27} grammes, and therefore the meteorites are one-twentieth of the mass of the Earth.

It follows, therefore, that, with such small masses as those with which the present theory deals, the deflection due to gravity is insensible, and we need only estimate actual impacts as encounters.

Hence, the radius of the sphere of action of a meteorite is identical with the diameter of its body.

§ 9. *On the Criterion for the Applicability of Hydrodynamics to a Swarm of Meteorites.*

The question at issue is to determine within what limits the quasi-gas formed by a swarm of colliding meteorites may be treated as a plenum, subject to the laws of hydrodynamics. The doctrines of the nebular hypothesis depend on the stability of a rotating mass of fluid, and that stability depends on the frequencies of its gravitational oscillations. Now the works of POINCARÉ and others seem to show that instability, at least in a homogeneous fluid, first arises from one of the graver modes of oscillation, and the period of the gravest mode does not differ much from the period of a satellite grazing the surface of the mass of fluid. Then, in order that hydrodynamical treatment should be applicable for the discussion of such questions of stability, the mean free time between collisions must be small compared with the period of such a satellite. Another way of stating this is that the mean free path of a meteorite shall be but little curved, and that the velocity of a meteorite shall be but little changed by gravity in the interval between two collisions. This must be fulfilled not only at the limits of the swarm, but at every point of it. The condition above stated will be satisfied if the space through which a meteorite falls from rest, at any part of the swarm, in the mean interval between collisions is small compared with the mean free path. If this criterion is fulfilled, then, in most respects which we are likely to discuss, the swarm will behave like a gas, and we must at present confine the consideration of the matter to this general criterion.

It would be laborious to determine exactly the space fallen through from rest, because gravity varies as the meteorite falls, but a sufficiently close approximation may be found by taking gravity constant throughout the fall and equal to its value at the point from which the meteorite starts.

We have already denoted by g the value of gravity at any part of the swarm, and have tabulated it in Table III. in terms of G or $\mu M/a^2$.

Now the mean interval is $T = L/(v\sqrt{8/3\pi})$. Hence, if D be the distance fallen in this time,

$$D = \tfrac{1}{2} gT^2 = \tfrac{1}{2}\frac{gL^2}{v^2}\cdot\frac{3\pi}{8}.$$

But

$$L = l\left(\frac{M_0}{M}\right)\frac{\frac{1}{3}\rho}{w}\cdot\frac{m}{s^2} \quad \text{and} \quad \frac{G}{v_0^2} = \frac{1}{\beta^2 a}.$$

Therefore,

$$\frac{D}{L} = \frac{l}{2a}\cdot\frac{M_0}{M}\cdot\frac{m}{s^2}\cdot\left\{\frac{3\pi}{8\beta^2}\cdot\frac{[g/G]}{[v^2/v_0^2][w/\frac{1}{3}\rho]}\right\} = \frac{l}{2a}\cdot\frac{M_0}{M}\cdot\frac{m}{s^2}\cdot F_1. \quad . \quad . \quad . \quad (37)$$

The factor F_1 has been tabulated above in Table III., and it increases from the centre to the outside.

This criterion may be regarded from another point of view, for, if the meteorite be describing a circular orbit about the centre of the swarm, D is the deflection from the straight path in the mean interval between two collisions. Then the criterion is that the deflection shall be small compared with the mean free path.

We may consider the criterion from again another point of view, and state that the arc of circular orbit described in the mean interval shall be a small fraction of the whole circumference.

The linear velocity v in the circular orbit is given by

$$v^2 = g\frac{a}{x} = \frac{v_0^2}{\beta^2 x} \cdot \frac{g}{G}.$$

And the mean interval $T = L/[v\sqrt{8/3\pi}]$. Hence, if A be the arc described with velocity v in time T,

$$A^2 = \frac{3\pi}{8\beta^2} \cdot \frac{L^2}{v^2/v_0^2} \cdot \frac{g}{Gx} = \frac{L^2}{v^2/v_0^2} \cdot \frac{g}{Gx} \text{ nearly, since } \frac{3\pi}{8\beta^2} = \cdot 988.$$

But the whole arc of circumference C is $2\pi a/x$.

Therefore,

$$\frac{A}{C} = \frac{L}{2a} \cdot \frac{1}{\pi} \cdot \frac{[g/G]^{\frac{1}{2}} x^{\frac{1}{2}}}{v/v_0}$$

$$= \frac{l}{2a} \cdot \frac{M_0}{M} \cdot \frac{m}{s^2} \cdot \left\{ \frac{1}{\pi} \cdot \frac{[g/G]^{\frac{1}{2}} x^{\frac{1}{2}}}{[v/v_0]\,[w/\frac{1}{3}\rho]} \right\} = \frac{l}{2a} \cdot \frac{M_0}{M} \cdot \frac{m}{s^2} \cdot F_2. \quad \quad (38)$$

The factor F_2 has been tabulated above, in Table III.

§ 10. *On the Density of Meteorites and Numerical Application.*

It is necessary to make assumptions both as to the mass and the density of the meteorites. We have a right to assume, I think, that the density δ is a little less than that of iron, say about 6, and we may put $\frac{4}{3}\pi\delta$ equal to 25. Then we have

$$m = \tfrac{1}{6}\pi\delta s^3 = \tfrac{25}{8}s^3, \quad \text{and} \quad \frac{m}{s^2} = \tfrac{25}{8}s.$$

There is but little information about the average size of meteorites; but, if we retain the symbol s, it will be easy, by merely shifting the decimal point in the final results, to obtain results for all sizes. Thus, if $s = 1$ cm., $m = 3\frac{1}{8}$ grammes; if $s = 10$ cm., $m = 3\frac{1}{8}$ kilogrammes; if $s = 100$ cm., $m = 3\frac{1}{8}$ tonnes, and if $s = 1000$ cm., $m = 3125$ tonnes. I shall, therefore, keep s in the analytical formulæ, and put it equal to unity in the numerical results.

In the first place, making no assumptions as to the density or masses of the meteorites, we have

$$M_0 = 2{\cdot}1767 \times M, \quad \tfrac{1}{3}\beta^2 = \cdot 39723.$$

Then, by substitution in (10) and (11), we have

$$\left.\begin{aligned}
v_0 &= u \times 10^{9\cdot86918-10} \\
L &= l \times 10^{0\cdot33781} \frac{m/s^2}{w/\frac{1}{3}\rho} \\
T &= \tau \times 10^{0\cdot46863} \frac{m/s^2}{[v/v_0]\,[w/\frac{1}{3}\rho]} \\
\frac{D}{L} &= \frac{l}{2a} \times 10^{0\cdot33781} \times F_1 \times \frac{m}{s^2} \\
\frac{A}{C} &= \frac{l}{2a} \times 10^{0\cdot33781} \times F_2 \times \frac{m}{s^2}
\end{aligned}\right\} \quad \ldots\ldots\ldots \quad (39)$$

We will now apply this solution to a case which will put the theory to a severe test. Suppose that the limit of the sphere of uniformly distributed energy of agitation is nearly as far as the planet Uranus, so that, say $a = 16a_0$. Then the extreme limit of the swarm is at $44\frac{1}{2}a_0$; but the orbit of the planet Neptune is at $30a_0$, so that the limit is further beyond Neptune than Saturn is from the Sun.

Now, if $a/a_0 = 16$, I find

$$\left.\begin{aligned}
u &= 10^{5\cdot86117} \text{ cm. per sec.} \\
&= 10^{\cdot18795} a_0 \text{ per annum,} \\
\tau &= 10^{4\cdot46808} \text{ seconds} \\
&= 10^{6\cdot96899-10} \text{ years}
\end{aligned}\right\} \quad \ldots\ldots\ldots \quad (40)$$

Introducing these values in (39) and putting $\frac{25}{8}s$ for m/s^2, I find

$$\left.\begin{aligned}
v &= 1\cdot141a_0 \text{ per annum} = 5\cdot374 \text{ kilom. per sec.} \\
\frac{L}{a_0} &= 10^{7\cdot9540-10} \times \frac{s}{[w/\frac{1}{3}\rho]} \\
T &= 10^{7\cdot9325-10} \times \frac{s}{[v/v_0]\,[w/\frac{1}{3}\rho]} \\
\frac{D}{L} &= 10^{6\cdot4489-10} s F_1 \\
\frac{A}{C} &= 10^{6\cdot4489-10} s F_2
\end{aligned}\right\} \quad \ldots\ldots \quad (41)$$

Now we have in Table III. the logarithms of the several factors, which occur last in these formulæ (41), at various distances from the centre.

It will suffice for our purpose only to take every other value from Table III. The distances from the centre are expressed in terms of the astronomical unit distance, viz., the Earth's mean distance from the Sun. The mean free path is expressed both in the same unit and in kilometres; and the mean intervals between collisions in days. The criteria D/L and A/C are, of course, pure numbers. Table IV., as it stands, is applicable to meteorites weighing $3\frac{1}{8}$ grammes, but by shifting the decimal point one place to the right in the last four rows of entries it becomes kilogrammes, one more and it becomes tonnes, and another, thousands of tonnes, and so on.

IV.—Table of Results.

The meteorites weigh $3\frac{1}{8}$ grammes, and have the density of iron. The swarm extends to $44\frac{1}{2}a_0$, a_0 being Earth's distance from Sun.

	Sun.	Asteroids.	Saturn.			Uranus.		Neptune.	
Distance from centre $\frac{r}{a_0} =$	0	2·55	7·66	12·77	16	19·2	24	32	$44\frac{1}{2}$
Velocity of mean square in kilometres per sec. $v =$	5·37	5·37	5·37	5·37	5·37	4·85	4·12	2·93	0
Mean free path, $\frac{L}{a_0} =$	·00028	·00038	·00157	·00511	·00900	·0122	·0199	·0552	∞
L kilom. $=$	41,600	57,000	233,000	760,000	1,340,000	1,810,000	2,960,000	8,210,000	∞
Mean free time, in days $T =$	·097	·133	·545	1·78	3·13	4·70	9·02	35·17	∞
Criterion, $\frac{D}{L} =$	..	·0000167	·0000832	·000195	·000278	·000387	·000709	·00279	∞
Criterion, $\frac{A}{C} =$	..	·0000113	·0000295	·0000633	·0000895	·000111	·000174	·000497	∞

The incidence of the several planets in the scale of distance is roughly indicated by the names written above.

The criteria show that, if the meteorites weigh $3\frac{1}{8}$ kilogrammes, the collisions are frequent enough, even beyond the orbit of Neptune, to allow the kinetic theory of gases to be applicable for such problems as are in contemplation. For, when $r/a = 32$, the two criteria (with decimal point shifted one place to the right) are ·028 and ·005, both small fractions. But, if the meteorites weigh $3\frac{1}{8}$ tonnes, the criteria cease to be very small, about $r/a = 24$. If they weigh 3125 tonnes, the applicability will cease somewhat beyond where the asteroids now are.

I conclude, then, from this discussion that we are justified in applying hydrodynamical treatment to a swarm of meteorites from which the solar system originated, even in the earliest stages of the history of the swarm.

This discussion has, of course, no bearing on the fundamental hypothesis that meteorites *can* glance from one another on impact with a virtually high degree of elasticity; nor does it do anything to justify the assumption that a swarm will consist approximately of a quasi-isothermal nucleus with a quasi-adiabatic layer over it. This latter assumption I have been led to by the considerations to which we now pass.

§ 11. *On the Diffusion of Kinetic Energy and on the Viscosity.*

In order to discuss these questions, it will be well to begin with a simple case of fluid motion.

Consider two-dimensional motion, in which there are a number of streams of equal breadth moving parallel to y with velocity V, and, interpolated between them, let there be strata of quiescent fluid; suppose then that we wish to find the motion at any time after this initial state. Let the boundaries of the streams V be from $x = ml$ to $\frac{1}{2}(2m + 1)l$. Then, if u be the velocity at x, parallel to y at time t, and ν the kinetic modulus of viscosity, the equation of motion is

$$\frac{du}{dt} = \nu \frac{d^2u}{dx^2}.$$

The solution of this being of the form $e^{-p^2t} \cos px$, the complete solution satisfying the initial condition is—

$$u = \tfrac{1}{2} V + \frac{2V}{\pi}\left[e^{-\pi^2\nu t/l^2} \cos \frac{\pi x}{l} - \tfrac{1}{3} e^{-9\pi^2\nu t/l^2} \cos \frac{3\pi x}{l} + \tfrac{1}{5} e^{-25\pi^2\nu t/l^2} \cos \frac{5\pi x}{l} - \ldots\right].$$

Now, if we refer time to a period τ, where $\tau = l^2/\pi^2\nu$, then after a time $\theta\tau$, which is greater than τ, the solution is sensibly

$$u = \tfrac{1}{2} V\left[1 + \frac{4}{\pi e^\theta} \cos \frac{\pi x}{l}\right].$$

It is clear that the maximum of u occurs when $x = 0$, and the minimum when $x = l$, and that they are

$$\tfrac{1}{2} V\left[1 \pm \frac{4}{\pi e^\theta}\right].$$

Hence, the difference between the maximum and minimum is $4V/\pi e^\theta$. Therefore, the ratio of the greatest difference of velocities after time $\theta\tau$ to the initial difference of velocities is $4/\pi e^\theta$. When θ is 1, 2, 3, this ratio assumes the values 1/2·135, 1/5·804, 1/15·73 respectively. Thus, after three times the interval τ, the difference of velocities is small. The time τ may be therefore taken as a convenient measure of viscosity.

In our problem the streams must be taken of a width comparable with the linear dimensions of the solar system. I therefore take l, the width of the streams, as equal to a_0, the Earth's distance from the Sun, and we have

$$\tau = \frac{a_0^2}{\pi^2\nu}.$$

Now, according to the kinetic theory of gases, the kinetic modulus of viscosity is $1/\pi$ into the mean free path multiplied by the mean velocity. Hence,

$$\nu = \frac{1}{\pi} L \left(v \sqrt{\frac{8}{3\pi}} \right) . ^{*}$$

Hence, we have

$$\tau = \left(\frac{a_0}{L}\right)\left(\frac{a_0}{v}\right) \sqrt{\frac{3}{8\pi}} . \qquad (42)$$

If we apply this formula to the solution which has been already found in Table IV., we obtain the following results :—

$\frac{}{a_0}$	0,	2·55,	7·66,	12·77,	16,	19·2,	24,	32,
τ years	1082,	792,	193,	59·2,	33·7,	27·5,	19·8,	61·7.

These results are applicable to meteorites weighing $3\frac{1}{8}$ grammes in a swarm extending to $44\frac{1}{2}\ a_0$. If the meteorites weigh $3\frac{1}{8}$ kilogrammes, the values of τ would be one-tenth of the tabulated values. If the streams were ten times as broad, the periods would be a hundred times as long.

Now the periods τ in the above table, even if multiplied by a thousand, must be considered as short in the history of a stellar system. It thus appears that the quasi-viscosity must be such that a swarm of meteors must, if revolving, move nearly without relative motion of its parts, at least in the early stages of its evolution.

But let us consider the values of τ at different epochs in the history of the same system. If a be the radius of the isothermal sphere the formulæ (9) and (10) show that L/a_0 varies as a^3, whilst v/a_0 varies as $a^{-\frac{1}{2}}$. Hence τ varies inversely as $a^{\frac{5}{2}}$. Thus, as the swarm contracts, the periods τ increase rapidly.

Thus, later in the history, the viscosity will probably fall off so much that equalisation of angular velocity may be no longer attained, and we should then have the central portion rotating more rapidly than the outside, with a gradual transition from one angular velocity to the other.

The modulus ν gives, besides the viscosity, the rate of equalisation of the kinetic energy of agitation ; this corresponds in a true gas with the conduction of heat. The conclusion at which we thus arrive appears to justify the assumption that the whole of the central part of the swarm is endued with uniform kinetic energy of agitation, and that the mass of the quasi-isothermal nucleus is the greatest possible. With regard to the assumption that the nucleus is coated with a layer in adiabatic or convective equilibrium, it may be remarked that the velocity of agitation must decrease when we get to the outskirts of the swarm, and convective equilibrium will probably satisfy the conditions of the case better than any other. Further considerations will be adduced on this point in the Summary.

* MEYER, 'Kinetische Theorie der Gase,' p. 321. The $1/\pi$ is derived from a numerical quadrature which gives the value ·318, and it is apparently only accidentally equal to $1/\pi$. The $v\sqrt{(8/3\pi)}$ is the mean velocity denoted Ω by MEYER.

§ 12. *On the Rate of Loss of Kinetic Energy through Imperfect Elasticity, and on the Heat Generated.*

In a collision between two meteorites the loss of energy is probably proportional to their relative kinetic energy before impact. Therefore, the amount of heat generated by a single meteorite per unit time is proportional to the kinetic energy (say h) and to the frequency of collision. By (10) the frequency, or reciprocal of T, varies as vws^2/m; but $m^{\frac{1}{2}}v$ is equal to $(2h)^{\frac{1}{2}}$, and $s^2m^{-\frac{3}{2}}$ varies as $m^{-\frac{5}{6}}$. Hence, the frequency of collision varies as $h^{\frac{1}{2}}wm^{-\frac{5}{6}}$, and the amount of heat generated by a single meteorite per unit time varies as $h^{\frac{3}{2}}wm^{-\frac{5}{6}}$. But, if p be the quasi-hydrostatic pressure, p varies as hwm^{-1}, and, therefore, the heat generated by a single meteorite varies as $h^{\frac{1}{2}}pm^{\frac{1}{6}}$.

Then, to find the total heat generated per unit time and volume, we have to multiply this by the number of meteorites per unit volume, that is to say, by wm^{-1}, which is equal to $3ph^{-1}$.

Thus the amount of heat generated per unit time and volume is proportional to $p^2m^{\frac{1}{6}}h^{-\frac{1}{2}}$. With meteorites of uniform size, and with uniform kinetic energy of agitation, this becomes simply the square of the hydrostatic pressure.

The mean temperature of the gases volatilised by collisions must depend on a variety of considerations, but it would seem as if the temperature would follow, more or less closely, the variations of heat generated per unit time and volume.

§ 13. *On the Fringe of a Swarm of Meteorites.*

The law of distribution of meteorites found above depends on the frequency of collisions. But at some distance from the centre collisions must have become so rare that the statistical method is inapplicable. There must then be a sort of fringe to the swarm, which I attempt to represent by supposing that beyond a certain radius a (not the same as the former a) collisions never occur, and each meteorite describes an orbit under gravity.

Now, at any point gravity depends on the mass of all the matter lying inside a sphere whose radius is equal to the distance of that point from the centre of the swarm. Hence, the value of gravity depends on the law of density of distribution of the meteorites, which is the thing which we are seeking to discover.

We suppose, then, that from every point of a sphere of radius a a fountain of meteorites is shot up, at all inclinations to the vertical, and with velocities grouped about a mean velocity, according to the exponential law appropriate to the case. As many meteorites are supposed to fall back on to the surface as leave it, and this inward cannonade against the boundary of the sphere exactly balances the quasi-gaseous pressure on the inside of the sphere. Thus, the ideal surface may be annihilated. Since the falling half of the orbit of a meteorite is the facsimile of the rising half, we need only trace the body from projection to apocentre, and then double the distribution of density which is deduced on the hypothesis that all the meteorites are rising. Again, since every element of the sphere shoots out a similar fountain, and since collisions are precluded by hypothesis, we need only consider the velocity along the radius vector. As far as concerns the distribution of density, it is the same as if each element shot up a vertical fountain; but, of course, in determining the vertical velocity, we must pay attention to the inclination to the vertical at which the meteorite was shot out.

The mass of the matter inside the sphere, whose attraction affords the principal part of the force under which the meteorites move, is say M, and, for the sake of simplicity of notation, we shall take $2\mu M/a$ as being unit square of velocity.

Now, let $\frac{1}{2}\phi(r)$ be the potential at the point whose radius is r, and suppose that a meteorite is shot out from a point on the sphere with a velocity u, and at an inclination ϵ to the vertical; then, if r, θ be the radius vector and longitude of the meteorite at the time t, the equations of conservation of moment of momentum, and of energy are—

$$r^2 \frac{d\theta}{dt} = ua \sin \epsilon,$$

$$\left(\frac{dr}{dt}\right)^2 + \left(r\frac{d\theta}{dt}\right)^2 - \phi(r) = u^2 - \phi(a).$$

If we write $f(r) = \phi(a) - \phi(r)$, and eliminate $d\theta/dt$, we get

$$r^2 \frac{dr}{dt} = r\{r^2(u^2 - f(r)) - u^2a^2 \sin^2 \epsilon\}^{\frac{1}{2}}.$$

Now, we are to regard dr/dt as the vertical velocity in a fountain squirting up from a point on the sphere. Then, since $f(a) = 0$, it follows that at the foot of the fountain $r^2\, dr/dt$ is equal to $a^2 u \cos \epsilon$. If, therefore, δ be the density at the height r, and δ_0 at the foot, the equation of continuity is

$$\delta\, r^2 \frac{dr}{dt} = \delta_0\, a^2 u \cos \epsilon.$$

Therefore,

$$\frac{\delta}{\delta_0} = \frac{a^2 u \cos \epsilon}{r\{r^2(u^2 - f(r)) - u^2a^2 \sin^2 \epsilon\}^{\frac{1}{2}}}.$$

But now let us suppose that the meteorites are not only shot out at inclination ϵ, but at all possible inclinations from 0° and 90°. It is then clear that this expression must be multiplied by $\sin \epsilon\, d\epsilon$, and integrated. Hence, if δ now denotes the integral density,

$$\delta = C\int_0^{\epsilon_0} \frac{a^2 u \cos \epsilon \sin \epsilon\, d\epsilon}{r\{r^2(u^2 - f(r)) - u^2a^2 \sin^2 \epsilon\}^{\frac{1}{2}}},$$

where C is a constant which it will be unnecessary to determine, and where the limit ϵ_0 will be the subject of future consideration.

Effecting the integration, we have

$$\delta = -\frac{C}{ur}\{r^2(u^2-f(r)) - u^2a^2\sin^2\epsilon\}^{\frac{1}{2}}, \text{ between limits,}$$

$$= \frac{C}{ur}[\{r^2(u^2-f(r))\}^{\frac{1}{2}} - \{r^2(u^2-f(r)) - u^2a^2\sin^2\epsilon_0\}^{\frac{1}{2}}].$$

It is obvious that, if u^2 is greater than $r^2f(r)/(r^2-a^2)$, the square root involved in dr/dt does not vanish for any value of ϵ; and, hence, we must simply take $\epsilon_0 = 90°$. If, on the other hand, u^2 is less than this critical value, ϵ_0 is that value of ϵ which makes dr/dt vanish.

Thus, our formula divides into three, viz. :—

1st. u^2 greater than $\dfrac{f(r)}{1-a^2/r^2}$;

$$\delta = \frac{C}{u}\left[(u^2-f(r))^{\frac{1}{2}} - \left(1-\frac{a^2}{r^2}\right)^{\frac{1}{2}}\left(u^2-\frac{f(r)}{1-a^2/r^2}\right)^{\frac{1}{2}}\right].$$

2nd. u^2 less than $\dfrac{f(r)}{1-a^2/r^2}$;

$$\delta = \frac{C}{u}(u^2-f(r))^{\frac{1}{2}}.$$

3rd. u^2 less than $f(r)$; $\delta = 0$.

The physical meaning of this division is as follows: If we take a station near the surface of the sphere, meteorites shot out at all inclinations, even horizontally, reach the height of our station; and, when they are shot out horizontally, $\epsilon = 90°$. If we go, however, to a higher region, there is a certain inclination which just brings the meteorites at apocentre, where $dr/dt = 0$, to our height; but those shot out more nearly horizontally fail to reach us. Still higher, not even a meteorite shot up vertically can reach us, and the density vanishes.

These results only correspond to a single velocity u; but, if v^2 be the mean square of the velocity, the number of meteorites whose velocities range between u and $u+du$ is proportional to $u^2e^{-3u^2/2v^2}du$.* Hence, we have to multiply δ by this expression, and integrate from $u=\infty$ to $u=0$.

Now, the first term of the first form for δ is the same as the second form; and in the third form δ is zero; hence, this first term when multiplied by the exponential has to be integrated from $u^2=\infty$ to $f(r)$. The second term of the first form of δ has to be multiplied by the exponential, and integrated from $u^2=\infty$ to $f(r)/(1-a^2/r^2)$.

Now, for the first term put

$$\frac{3}{2v^2}(u^2-f(r)) = x^2;$$

* OSKAR MEYER, 'Die Kinetische Theorie der Gase,' 1877, pp. 271–2.

therefore,

$$u\,(u^2 - f(r))^{\frac{1}{2}}\,du = \left(\frac{2v^2}{3}\right)^{\frac{3}{2}} x^2\,dx,$$

and the limits of x are ∞ to 0.

Hence, the first term is

$$C\left(\frac{2v^2}{3}\right)^{\frac{3}{2}} e^{-3f(r)/2v^2}\int_0 x^2\,e^{-x^2}\,dx$$

Again, for the second term put

$$\frac{3}{2v^2}\left(u^2 - \frac{f(r)}{1 - a^2/r^2}\right) = x^2,$$

and similarly introduce it into the second term, and we have

$$-\,C\left(\frac{2v^2}{3}\right)^{\frac{3}{2}}\left(1 - \frac{a^2}{r^2}\right)^{\frac{1}{2}} e^{3f(r)/2v^2(1-a^2/r^2)}\int_0^\infty x^2\,e^{-x^2}\,dx.$$

From these expressions we may omit the constant factors; and, if w be the density at height r, whilst w_0 is the density at the sphere,

$$\frac{w}{w_0} = e^{-3f(r)/2v^2} - \left(1 - \frac{a^2}{r^2}\right)^{\frac{1}{2}} e^{-3f(r)/2v^2(1-a^2/r^2)}.$$

In this formula unit square of velocity is $2\mu M/a$; but we have elsewhere written $v^2 = \beta^2\mu M/a$; hence, if the special unit of velocity be given up, we may write β^2 in place of $2v^2$, and the result becomes

$$\frac{w}{w_0} = e^{-3f(r)/\beta^2} - \left(1 - \frac{a^2}{r^2}\right)^{\frac{1}{2}} e^{-3f(r)/\beta^2(1-a^2/r^2)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

It is interesting to observe the connection between this law of density and that which would have held if the gaseous law (due to collisions) had obtained. In that case, since $\frac{1}{2}\phi\,(r)$ is the potential, we should have had

$$\frac{1}{w}\frac{dp}{dr} - \frac{1}{2}\frac{d}{dr}\,\phi\,(r) = 0.$$

Now, $p = \frac{1}{3}v^2 w$, and, therefore,

$$\log w - \frac{3}{2v^2}\,\phi\,(r) = \text{const.}$$

$$= \log w_0 - \frac{3}{2v^2}\,\phi\,(a).$$

Thus,

$$\log\frac{w}{w_0} = -\frac{3}{2v^2}\left[\phi\,(a) - \phi\,(r)\right] = -\frac{3}{2v^2}\,f(r),$$

or

$$\frac{w}{w_0} = e^{-3f(r)/2v^2} = e^{-3f(r)/\beta^2}.$$

The first term of our result, then, is exactly that resulting from the gaseous law, and the second subtractive term represents the action of the diminished velocity with which the meteorites move in the higher regions, when they are liberated from the equalising effects of continual impacts.

By previous definition, $\mu M f(r)/a$ is the excess of the potential at radius a above its value at radius r; hence,

$$\frac{\mu M}{a} f(r) = \int_a^r \frac{4\pi\mu}{r^2} \int_0^r wr^2\, dr\,.\, dr.$$

Now, since $f(r)$ is only required for values of r greater than a, we may put w equal to its mean value ρ, between the limits 0 and a. Thus,

$$\int_0^r wr^2\, dr = \int_a^r wr^2\, dr + \tfrac{1}{3}\rho a^3.$$

Hence,

$$f(r) = \frac{4\pi a}{M}\left[\int_a^r \frac{1}{r^2}\int_a^r wr^2\, dr + \tfrac{1}{3}\rho a^3 \int_a^r \frac{dr}{r^2}\right] = \left(1 - \frac{a}{r}\right) + 3\int_1^{r/a} \frac{1}{z^2}\int_1^z \frac{w}{\rho}\, z^2\, dz.$$

If this form for $f(r)$ were substituted in (43), we should obtain a very complicated differential equation for w. We may, however, find two values of $f(r)$ within which the truth must lie.

First, if we neglect the attraction of all the matter lying outside of radius a, the second term vanishes, and we have,

$$f(r) = 1 - \frac{a}{r};$$

and the law of density is

$$\frac{w}{w_0} = e^{-3(1-a/r)\beta^2} - \left(1 - \frac{a^2}{r^2}\right)^{\frac{1}{2}} e^{3/(1+a/r)\beta^2}. \qquad \text{(44)}$$

Secondly, we may suppose the density to go on diminishing according to the inverse square of the distance. We have seen in the preceding solution and tables that this is roughly the law of diminution for a long way outside the isothermal nucleus. According to this assumption, $w = w_0\, a^2/r^2$. Hence, in the second term of $f(r)$ we put $w = w_0\, a^2/r^2 = w_0/z^2$.

Hence,

$$\int_1^z \frac{w}{\rho} z^2\, dz = \frac{w_0}{\rho}\int_1^z dz = \frac{w_0}{\rho}(z-1),$$

and

$$f(r) = 1 - \frac{a}{r} + \frac{w_0}{\frac{1}{3}\rho}\int_1^{r/a} \frac{z-1}{z^2}\, dz = \left(1 - \frac{w_0}{\frac{1}{3}\rho}\right)\left(1 - \frac{a}{r}\right) + \frac{w_0}{\frac{1}{3}\rho}\log\frac{r}{a}. \qquad \text{(45)}$$

The substitution of this value in (43) gives the law of density.

In order to see the kind of results to which these formulæ lead, let us suppose that, when we have reached radius 2 in the adiabatic layer, collisions have become so rare as to be negligible. Then the symbols in the formulæ of this section have numerical values; and, in order to distinguish them, let them be accented, so that, for example, we write a', β'^2, ρ', &c., in (43), (44), and (45), in place of a, β^2, ρ.

Now Table III. shows that, when $a' = 2a$, $M' = 1{\cdot}95\,M = 2M$ nearly. Hence, $M'/a' = M/a$ nearly. But, at radius $2a$ in Table III., $v^2/v_0{}^2 = {\cdot}298 = {\cdot}3$, and this v^2 is what we now write v'^2 or $\beta'^2\mu M'/a'$, whilst $v_0{}^2 = \beta^2\mu M/a$.

But $\beta^2 = \frac{6}{5}$ very nearly; hence, $\beta'^2/\beta^2 = {\cdot}3$, or $\beta'^2 = {\cdot}36$.

Thus, $3/\beta'^2 = 8{\cdot}333$.

Then, substituting $2a$ for a', and noticing that in Table III., $w/\frac{1}{3}\rho = {\cdot}163$, when $r = 2a$, the first law of density (44) becomes

$$\frac{w}{\frac{1}{3}\rho} = {\cdot}163\left[e^{-\frac{25}{3}(1-2a/r)} - \left(1 - 4\frac{a^2}{r^2}\right)^{\frac{1}{2}} e^{-\frac{25}{3}/(1+2a/r)}\right]. \quad . \quad . \quad . \quad (46)$$

Again, since $M' = 2M$, and $a' = 2a$, $\rho' = \frac{1}{4}\rho$, $\dfrac{w_0{}'}{\frac{1}{3}\rho'} = 4 \times \dfrac{w_0{}'}{\frac{1}{3}\rho} = 4 \times {\cdot}163$ by Table III., and $\dfrac{w_0{}'}{\frac{1}{3}\rho'} = {\cdot}65 = \frac{2}{3}$ nearly.

Thus, according to the second assumption, we have by (45)

$$3f(r) = \left(1 - \frac{2a}{r}\right) + \log\left(\frac{r}{2a}\right)^2, \quad \text{and, since } \frac{1}{\beta'^2} = 2{\cdot}78,$$

$$\left.\begin{aligned} \frac{3f(r)}{\beta'^2} &= 2{\cdot}78\left(1 - \frac{2a}{r}\right) + 2{\cdot}78\log\left(\frac{r}{2a}\right)^2, \\ \frac{3f(r)}{\beta'^2(1 - 4a^2/r^2)} &= \frac{2{\cdot}78}{1 + 2a/r} + \frac{2{\cdot}78}{1 - 4a^2/r^2}\log\left(\frac{r}{2a}\right)^2; \end{aligned}\right\}$$

and the law of density is

$$\frac{w}{\frac{1}{3}\rho} = {\cdot}163\left[e^{-3f(r)/\beta'^2} - \left(1 - \frac{4a^2}{r^2}\right)^{\frac{1}{2}} e^{-3f(r)/\beta'^2(1-4a^2/r^2)}\right]. \quad . \quad . \quad . \quad (47)$$

The values computed from these alternative formulæ (46) and (47) will be comparable with those in Table III.

In Table III. we have the value of $w/\frac{1}{3}\rho$ computed at distances $r/a = 2{\cdot}208$, $2{\cdot}463$, $2{\cdot}786$. The following short table gives the result extracted from Table III. for comparison with the values computed from (46) and (47):—

$r/a =$	2·0,	2·208,	2·463,	2·786.
Convective equilib., $\frac{w}{\frac{1}{3}\rho} =$	·163,	·092,	·033,	0.
First hypoth. (46), $\frac{w}{\frac{1}{3}\rho} =$	·163,	·074,	·033,	·015.
Second hypoth. (47), $\frac{w}{\frac{1}{3}\rho} =$	·163,	·071,	·029,	·011.

It appears, therefore, that the results from the two hypotheses differ but little for some distance outside the region of collisions, and either line may be taken as near enough to the correct result. We see then that the effect of annulling collisions and allowing each body to describe an orbit is that the density at first falls off more rapidly than if the medium were in convective equilibrium, and that further away the density falls off less rapidly. At more remote distances the density would be found to tend to vary as the inverse square of this distance. Thus, the formulæ would make the mass of the system infinite. In other words, the existence of meteorites with nearly parabolic and hyperbolic orbits necessitates an infinite number, if the loss to the system is constantly made good by the supply.

The subject of this section is considered further, from a physical point of view, in the Summary at the end.

§ 14. *On the Kinetic Theory where the Meteorites are of all sizes.*

In an actual swarm of meteorites all sizes occur, for, even if this were not the case initially, inequality of size would soon arise through fractures. Hence, it becomes of interest to examine the kinetic theory on the hypothesis that the colliding bodies are of all possible sizes, grouped about some mean value according to some law of frequency.

If there be two sets of elastic spheres in such numbers that there are respectively A and B in unit volume, and if the mean squares of the velocities of the two are α^2 and β^2 respectively, and if a and b are the radii of the spheres of the two sets, then it is proved that the number of collisions between them per unit time and volume is

$$2AB\,(a+b)^2\left[\tfrac{2}{3}\pi\,(\alpha^2+\beta^2)\right]^{\frac{1}{2}}\,*$$

We shall now change the notation, and for a and b write s_1 and s_2, and for α and β write u_1 and u_2.

Then, if δ be the density of the spheres, their masses are $\frac{4}{3}\pi\delta s_1^3$ and $\frac{4}{3}\pi\delta s_2^3$.

The condition for the permanence of condition is that the spheres of all masses shall have the same mean kinetic energy. Hence, we refer the mass to a mean sphere of radius $\mathfrak{s}$, and the velocity to a square of velocity V^2.

Then

$$s_1^3 u_1^2 = s_2^3 u_2^2 = \mathfrak{s}^3 V^2.$$

Thus, our formula may be written

$$2AB\,(s_1+s_2)^2\left[\left(\frac{\mathfrak{s}}{s_1}\right)^3+\left(\frac{\mathfrak{s}}{s_2}\right)^3\right]^{\frac{1}{2}}\left(\tfrac{2}{3}\pi V^2\right)^{\frac{1}{2}}.$$

* 'The Kinetic Theory of Gases,' by H. W. WATSON, p. 11.

But now suppose that there are spheres of all possible sizes, and that in unit volume the number whose radius lies between s and $s + ds$ is

$$\frac{4n}{\sigma^3\sqrt{\pi}} s^2 e^{-s^2/\sigma^2} ds.*$$

Since the integral of this from ∞ to 0 is n, it follows that n is the number of spheres of all sizes in unit volume.

If ρ be the total mass in unit volume, or the density of distribution,

$$\begin{aligned}\rho &= \frac{4n}{\sqrt{\pi}} \cdot \int_0^\infty \tfrac{4}{3}\pi\delta s^3 \,.\, \frac{s^2}{\sigma^3} e^{-s^2/\sigma^2} ds \\ &= \frac{4n}{\sqrt{\pi}} \cdot \tfrac{4}{3}\pi\delta\sigma^3 \int_0^\infty x^5 e^{-x^2} dx \\ &= \frac{4n}{\sqrt{\pi}} \cdot \tfrac{4}{3}\pi\delta\sigma^3 .\end{aligned}$$

If m be the mean mass, $m = \rho/n$; but $m = \frac{4}{3}\pi\delta\varsigma^3$; hence,

$$\varsigma^3 = \frac{4}{\sqrt{\pi}}\sigma^3,$$

and

$$\left(\frac{\varsigma}{s_1}\right)^3 + \left(\frac{\varsigma}{s_2}\right)^3 = \frac{4}{\sqrt{\pi}}\left[\left(\frac{\sigma}{s_1}\right)^3 + \left(\frac{\sigma}{s_2}\right)^3\right].$$

If the A spheres of radii s_1 are those whose radii lie between s_1 and $s_1 + ds_1$, and the B spheres of radii s_2 are those whose radii lie between s_2 and $s_2 + ds_2$,

$$A = \frac{4n}{\sqrt{\pi}}\left(\frac{s_1}{\sigma}\right)^2 e^{-s_1^2/\sigma^2}\frac{ds_1}{\sigma},$$

$$B = \frac{4n}{\sqrt{\pi}}\left(\frac{s_2}{\sigma}\right)^2 e^{-s_2^2/\sigma^2}\frac{ds_2}{\sigma}.$$

Hence, the formula for collisions between the A's and B's is

$$\frac{64n^2}{\pi^{\frac{5}{4}}} \cdot (\tfrac{2}{3}\pi V^2)^{\frac{1}{2}} . (s_1 + s_2)^2 \left[\left(\frac{\sigma}{s_1}\right)^3 + \left(\frac{\sigma}{s_2}\right)^3\right]^{\frac{1}{2}} \frac{s_1{}^2 s_2{}^2}{\sigma^4} e^{-(s_1^2 + s_2^2)/\sigma^2} \frac{ds_1}{\sigma}\frac{ds_2}{\sigma},$$

or, if we write $x = s_1/\sigma$, $y = s_2/\sigma$, it is

$$\frac{64n^2}{\pi^{\frac{5}{4}}} (\tfrac{2}{3}\pi V^2)^{\frac{1}{2}} \sigma^2 (x + y)^2 (x^3 + y^3)^{\frac{1}{2}} (xy)^{\frac{1}{2}} e^{-x^2-y^2} dx\, dy . \quad . \quad . \quad . \quad (48)$$

* If the spheres are grouped about a mean mass, instead of about a mean radius, according to a law of this kind, the subsequent integrals become very troublesome. Any law of the kind suffices for the discussion. If, however, I had foreseen the investigation of § 16, I should not have taken this law of frequency.

But

$$\sigma^2 = \frac{\pi^{\frac{1}{3}}}{2^{\frac{4}{3}}} \varsigma^2, \quad \text{and} \quad \frac{64}{\pi^{\frac{5}{4}}} \left(\tfrac{2}{3}\pi\right)^{\frac{1}{2}} \frac{\pi^{\frac{1}{3}}}{2^{\frac{4}{3}}} = \frac{32}{\pi^{\frac{5}{12}}} \cdot \frac{2^{\frac{1}{6}}}{3^{\frac{1}{2}}} \cdot$$

Hence, the number of collisions per unit time and volume between spheres whose radii range between s_1 and $s_1 + ds_1$, and others with radii between s_2 and $s_2 + ds_2$, is

$$\frac{32}{\pi^{\frac{5}{12}}} \frac{2^{\frac{1}{6}}}{3^{\frac{1}{2}}} \cdot V\varsigma^2 n^2 \,.\, (x+y)^2 (x^3+y^3)^{\frac{1}{2}} (xy)^{\frac{1}{2}} e^{-x^2-y^2} \,dx\,dy\,.$$

The number of collisions of a single sphere per unit time is $1/n$ of this, and, since $n = \rho/m$, we have for the collisions of a single sphere the factor $\dfrac{V\varsigma^2}{m/\rho}$ instead of $V\varsigma^2 n^2$.

Then the total number of collisions of all kinds in unit time, or the reciprocal of the mean free time, is the double integral of this from ∞ to 0.

For the purpose of carrying out the integration, we may conveniently, as an algebraic artifice, change from the rectangular axes x, y to the polar coordinates r, θ. Thus,

$$\int_0^\infty \int_0^\infty (x+y)^2 (x^3+y^3)^{\frac{1}{2}} (xy)^{\frac{1}{2}} e^{-x^2-y^2} \,dx\,dy$$
$$= \int_0^\infty r^{\frac{11}{2}} e^{-r^2} dr \int_0^{\frac{1}{2}\pi} (\sin\theta + \cos\theta)^{\frac{5}{2}} (1 - \sin\theta\cos\theta)^{\frac{1}{2}} (\sin\theta\cos\theta)^{\frac{1}{2}} \,d\theta.$$

Now, if we put $r = z^2$,

$$\int_0^\infty r^{\frac{11}{2}} e^{-r^2} dr = 2\int_0^\infty z^{12} e^{-z^4} dz = 2 \cdot \frac{9\,.\,5\,.\,1}{4\,.\,4\,.\,4} \int_0^\infty e^{-z^4} dz.$$

For the transformation of the second integral, put

$$z = \cos\theta - \sin\theta,$$

and we find

$$\int_0^{\frac{1}{2}\pi} (\sin\theta + \cos\theta)^{\frac{5}{2}} (1 - \sin\theta\cos\theta)^{\frac{1}{2}} (\sin\theta\cos\theta)^{\frac{1}{2}} \,d\theta = \int_{-1}^{+1} \tfrac{1}{2} (2 - z^2)^{\frac{3}{4}} (1 - z^4)^{\frac{1}{2}} \,dz$$
$$= \int_0^{+1} (2 - z^2)^{\frac{3}{4}} (1 - z^4)^{\frac{1}{2}} \,dz.$$

Hence, the whole integral is

$$\tfrac{45}{32} \int_0^\infty e^{-z^4} dz \int_0^1 (2 - z^2)^{\frac{3}{4}} (1 - z^4)^{\frac{1}{2}} \,dz,$$

and the mean frequency of collision of a single ball per unit time is

$$\tfrac{15}{4} \cdot \frac{3^{\frac{1}{2}}\,.\,2^{\frac{1}{6}}}{\pi^{\frac{5}{12}}} \frac{V(2\varsigma)^2}{m/\rho} \int_0^\infty e^{-z^4} dz \int_0^1 (2 - z^2)^{\frac{3}{4}} (1 - z^4)^{\frac{1}{2}} \,dz.$$

The second of these two integrals cannot, I think, be evaluated algebraically, but its value is easily found by quadratures. I find, then,

$$\int_0^1 (2 - z^2)^{\frac{3}{4}} (1 - z^4)^{\frac{1}{2}} \, dz = 1{\cdot}2999.$$

The former of the two integrals may be evaluated as follows :—

Let

$$I = \int_0^\infty e^{-x^4} \, dx,$$

then,

$$\begin{aligned}
4I^2 &= 4 \int_0^\infty \int_0^\infty e^{-x^4 - y^4} \, dx \, dy \\
&= 4 \int_0^\infty \int_0^{\frac{1}{2}\pi} e^{-r^4 (1 - \frac{1}{2} \sin^2 2\theta)} \, r \, dr \, d\theta \\
&= \int_0^\infty \int_0^\pi e^{-z^2 (1 - \frac{1}{2} \sin^2 \phi)} \, dz \, d\phi \\
&= 2 \int_0^{\frac{1}{2}\pi} \int_0^\infty \frac{e^{-t^2}}{(1 - \frac{1}{2} \sin^2 \phi)} \, dt \, d\phi \\
&= \pi^{\frac{1}{2}} \int_0^{\frac{1}{2}\pi} \frac{d\phi}{(1 - \frac{1}{2} \sin^2 \phi)^{\frac{1}{2}}} \\
&= \pi^{\frac{1}{2}} F(45^\circ),
\end{aligned}$$

where F is the complete elliptic integral with modulus sin 45°.

Hence,

$$I = \tfrac{1}{2} \pi^{\frac{1}{4}} F^{\frac{1}{2}}.^*$$

We thus have the frequency of collision given by

$$\tfrac{15}{8} \cdot \frac{3^{\frac{1}{2}} . 2^{\frac{1}{6}}}{\pi^{\frac{1}{6}}} \cdot F^{\frac{1}{2}} \cdot 1{\cdot}2999 \cdot \frac{V (2\varsigma)^2}{m/\rho} \, .$$

Now, LEGENDRE's Tables give

$$\log F = {\cdot}2681272,$$

with which value we easily find for T the mean free time, or $1/T$ the frequency,

$$\frac{1}{T} = 5{\cdot}3318 \, \frac{V (2\varsigma)^2}{m/\rho} = \tfrac{16}{3} \frac{V (2\varsigma)^2}{m/\rho} \text{ nearly.} \quad \ldots \ldots \quad (49)$$

If $1/T_0$ be the frequency of collision when the spheres are all of the same size and mass ς and m, and are agitated with mean square of velocity V^2, we have, by the ordinary theory,

$$\frac{1}{T_0} = 4 \sqrt{\frac{\pi}{3}} \cdot \frac{V (2\varsigma)^2}{m/\rho} = 4{\cdot}0935 \, \frac{V (2\varsigma)^2}{m/\rho} \, . \quad \ldots \ldots \quad (50)$$

* I owe this to Mr. FORSYTH, and the result verifies an evaluation by quadratures which I had made.

It follows, therefore, that in our case collisions are more frequent than if the balls were all of the same size in about the proportion of 4 to 3.

In order to find the mean free path, we require to find the mean velocity.

If u^2 be the mean square of the velocity for any size s, the proportion of all the spheres of that size which move with velocities lying between v and $v + dv$ is

$$\frac{4}{\sqrt{\pi}}\, y^2 e^{-y^2}\, dy,$$

where $y^2 = 3v^2/2u^2$.

But the number of spheres of size between s and $s + ds$, in unit volume, is

$$\frac{4n}{\sqrt{\pi}}\, x^2 e^{-x^2}\, dx,$$

where $x = s/\sigma$.

Hence, the mean velocity U is given by

$$U = \frac{16}{\pi}\int_0^\infty\int_0^\infty v x^2 y^2 e^{-x^2-y^2}\, dx\, dy.$$

Now,

$$v = \sqrt{\tfrac{2}{3}}\,.\, uy, \text{ and } s^3u^2 = \varsigma^3 V^2, \text{ or } x^3u^2 = \left(\frac{\varsigma}{\sigma}\right)^3 V^2 = \frac{4}{\sqrt{\pi}}\, V^2,$$

so that

$$u = \frac{2}{\pi^{\frac{1}{4}}}\, x^{-\frac{3}{2}} V, \text{ and } v = \frac{2\sqrt{2}}{\pi^{\frac{1}{4}}\sqrt{3}}\, x^{-\frac{3}{2}} y V.$$

Therefore,

$$U = \frac{32\sqrt{2}}{\pi^{\frac{5}{4}}\sqrt{3}}\, V\int_0^\infty\int_0^\infty x^{\frac{1}{2}} y^3 e^{-x^2-y^2}\, dx\, dy.$$

But

$$\int_0^\infty y^3 e^{-y^2}\, dy = \tfrac{1}{2}, \text{ and } \int_0^\infty x^{\frac{1}{2}} e^{-x^2}\, dx = 2\int_0^\infty z^2 e^{-z^4}\, dz\,;$$

therefore,

$$U = \frac{32\sqrt{2}}{\pi^{\frac{5}{4}}\sqrt{3}}\, V\int_0^\infty z^2 e^{-z^4}\, dz.$$

This integral may be evaluated as follows:—

Let

$$J = \int_0^\infty x^2 e^{-x^4}\, dx,$$

$$\begin{aligned}
4J^2 &= 4\int_0^\infty\int_0^\infty x^2 y^2 e^{-x^4-y^4}\, dx\, dy \\
&= \int_0^\infty\int_0^{\frac{1}{2}\pi} r^4 \sin^2 2\theta\, e^{-r^4(1-\frac{1}{2}\sin^2 2\theta)}\, r\, dr\, d\theta \\
&= \tfrac{1}{4}\int_0^\infty\int_0^\pi z^2 \sin^2\phi\, e^{-z^2(1-\frac{1}{2}\sin^2\phi)}\, dz\, d\phi \\
&= \tfrac{1}{2}\int_0^\infty\int_0^{\frac{1}{2}\pi} \frac{\sin^2\phi}{(1-\frac{1}{2}\sin^2\phi)^{\frac{3}{2}}}\, t^2 e^{-t^2}\, dt\, d\phi \\
&= \tfrac{1}{4}\pi^{\frac{1}{2}}\int_0^{\frac{1}{2}\pi} \frac{\frac{1}{2}\sin^2\phi}{(1-\frac{1}{2}\sin^2\phi)^{\frac{3}{2}}}\, d\phi \\
&= \tfrac{1}{4}\pi^{\frac{1}{2}}\left[\int_0^{\frac{1}{2}\pi} \frac{d\phi}{(1-\frac{1}{2}\sin^2\phi)^{\frac{3}{2}}} - F\right].
\end{aligned}$$

Now,

$$\int_0^{\frac{1}{2}\pi} \frac{d\phi}{(1 - k^2 \sin^2 \phi)^{\frac{3}{2}}} = \frac{E}{k'^2},$$

and in the present case $k^2 = k'^2 = \frac{1}{2}$.

Hence,

$$J = \tfrac{1}{4}\pi^{\frac{1}{2}} [2E - F]^{\frac{1}{2}} *,$$

where E and F are the complete elliptic integrals with modulus sin 45°.

In LEGENDRE'S Tables, we find

$$E = 1{\cdot}350644, \quad F = 1{\cdot}854075, \quad \text{and } 2E - F = {\cdot}847213.$$

Then,

$$\frac{U}{V} = \frac{8}{\pi} \sqrt{\tfrac{2}{3}} \sqrt{(2E - F)} = 1{\cdot}91377.$$

The mean free path

$$L = UT = 1{\cdot}9138\ VT = \frac{1{\cdot}9138}{5{\cdot}3318} \frac{m/\rho}{(2\varsigma)^2},$$

and thus

$$L = \frac{1}{2{\cdot}786} \frac{m/\rho}{(2\varsigma)^2}. \qquad (51)$$

If the spheres had all been of the same size, we should have had

$$L_0 = \frac{m/\rho}{\pi (2\varsigma)^2 \sqrt{2}} = \frac{1}{4{\cdot}44} \frac{m/\rho}{(2\varsigma)^2}. \qquad (52)$$

Hence, finally from (49) to (52), if there be a number of spherical meteorites, of uniform density, of all sizes with radii grouped about a mean radius according to the law of error, and if S be the *diameter* of the meteorite of mean mass m, and ρ be the density of the distribution of meteorites in space, and $\frac{1}{2} m V^2$ their mean kinetic energy of agitation, then the mean free path L, mean free time T, and mean velocity U are given by

$$\left.\begin{aligned} L &= \frac{1}{2{\cdot}786} \frac{m/\rho}{S^2} = \tfrac{5}{14} \frac{m/\rho}{S^2} \text{ nearly,} \\ T &= \frac{1}{5{\cdot}332} \frac{m/\rho}{VS^2} = \tfrac{3}{16} \frac{m/\rho}{VS^2} \text{ nearly,} \\ U &= 1{\cdot}9138\ V = 2V \text{ nearly.} \end{aligned}\right\} \qquad (53)$$

Also the mean free path is about $\frac{7}{11}$ths, and the mean free time about $\frac{3}{4}$ of that which would have held if the meteorites had all been of the same size m and had had the same mean kinetic energy $\frac{1}{2} m V^2$.

* I owe this to Mr. FORSYTH.

§ 15. *On the Variation of Mean Frequency of Collision and Mean Free Path for the several sizes of balls.*

Each size of ball has its own mean frequency of collision and mean free path, and it is well to trace how the total means evaluated in the last section are made up.

We have already seen in (48) that (substituting for σ its value in terms of ς) the number of collisions per unit time and volume between balls of sizes s to $s + ds$ and balls of sizes s' to $s' + ds'$ is

$$\frac{64n^2}{\pi^{\frac{5}{4}}}\left(\tfrac{2}{3}\pi V^2\right)^{\frac{1}{2}}\cdot\frac{\pi^{\frac{1}{3}}}{2^{\frac{4}{3}}}\,\varsigma^2 (x+y)^2 (x^3+y^3)^{\frac{1}{2}} (xy)^{\frac{1}{2}} e^{-x^2-y^2}\,dx\,dy,$$

where $x = s/\sigma$, $y = s'/\sigma$.

But the number of balls of size s to $s + ds$ in unit volume is

$$\frac{4n}{\sqrt{\pi}}\,x^2 e^{-x}\,dx.$$

Hence, the mean frequency of collision for a ball of size s with all others is

$$\frac{\pi^{\frac{1}{2}}}{4n}\cdot\frac{64n^2}{\pi^{\frac{5}{4}}}\left(\tfrac{2}{3}\pi V^2\right)^{\frac{1}{2}}\frac{\pi^{\frac{1}{3}}}{2^{\frac{4}{3}}}\,\varsigma^2\int_0^\infty (x+y)^2 (x^3+y^3)^{\frac{1}{2}} x^{-\frac{3}{2}} y^{\frac{1}{2}} e^{-y^2}\,dy.$$

Now,

$$x^{-\frac{3}{2}} = \left(\frac{\sigma}{s}\right)^{\frac{3}{2}} = \tfrac{1}{2}\pi^{\frac{1}{4}}\cdot\left(\frac{\varsigma}{s}\right)^{\frac{3}{2}}.$$

Therefore, if we write $1/\tau$ for the frequency of collision of a ball of size s with all others, we have

$$\frac{1}{\tau} = \frac{2^{\frac{1}{6}}\cdot\pi^{\frac{1}{3}}}{3^{\frac{1}{2}}}\cdot\frac{V(2\varsigma)^2}{m/\rho}\left(\frac{\varsigma}{s}\right)^{\frac{3}{2}}\int_0^\infty (x+y)^2 (x^3+y^3)^{\frac{1}{2}} y^{\frac{1}{2}} e^{-y^2}\,dy.$$

Now, the mean frequency for all sizes is given by

$$\frac{1}{T} = 5{\cdot}3318\cdot\frac{V(2\varsigma)^2}{m/\rho}\,.$$

Hence,

$$\frac{T}{\tau} = \frac{1}{5{\cdot}3318}\cdot\frac{2^{\frac{1}{6}}\cdot\pi^{\frac{1}{3}}}{3^{\frac{1}{2}}}\cdot\left(\frac{\varsigma}{s}\right)^{\frac{3}{2}}\int_0^\infty (x+y)^2 (x^3+y^3)^{\frac{1}{2}} y^{\frac{1}{2}} e^{-y^2}\,dy.$$

$$= {\cdot}1780\cdot\left(\frac{\varsigma}{s}\right)^{\frac{3}{2}}\int_0^\infty (x+y)^2 (x^3+y^3)^{\frac{1}{2}} y^{\frac{1}{2}} e^{-y^2}\,dy. \quad\ldots\ldots \quad (54)$$

The integral involved here cannot in general be determined algebraically; but, if x be very small, or very great, we can find an approximate value for it.

If x be very small, the integral becomes

$$\int_0^\infty y^4 e^{-y^2}\,dy = \tfrac{3}{8}\sqrt{\pi}, \qquad \text{and} \qquad \frac{T}{\tau} = {\cdot}118\left(\frac{\varsigma}{s}\right)^{\frac{3}{2}}.$$

If x be very large, the integral becomes

$$x^{\frac{7}{2}} \int_0^\infty y^{\frac{1}{2}} e^{-y^2} dy = 2x^{\frac{7}{2}} \int_0^\infty z^2 e^{-z^4} dz.$$

Now,

$$x^{\frac{7}{2}} = \left(\frac{s}{\sigma}\right)^{\frac{7}{4}} = \frac{2^{\frac{7}{3}}}{\pi^{\frac{7}{12}}} \left(\frac{s}{\varsigma}\right)^{\frac{7}{2}}, \qquad \text{and} \qquad \int_0^\infty z^2 e^{-z^4} dz = \tfrac{1}{4}\pi^{\frac{1}{4}} (2E - F)^{\frac{1}{2}}.$$

Therefore, the integral becomes

$$\frac{2^{\frac{4}{3}}}{\pi^{\frac{4}{3}}} (2E - F)^{\frac{1}{2}} \left(\frac{s}{\varsigma}\right)^{\frac{1}{2}},$$

and with the known values of E and F this gives us

$$\frac{T}{\tau} = \cdot 282 \left(\frac{s}{\varsigma}\right)^2 .$$

For intermediate values of s recourse must be had to quadratures for evaluating the integral. I have therefore determined, by a rough numerical process, sufficient values of the integral to render possible the drawing of a curve for the values of T/τ for all values of s. The following table gives the results for the integral $\int_0^\infty (x + y)^2 (x^3 + y^3)^{\frac{1}{2}} y^{\frac{1}{2}} e^{-y^2} dy$, which may be denoted by K:—

	K
$s = \frac{1}{2}\varsigma$	1·71 ,
$s = \frac{3}{4}\varsigma$	2·90 ,
$s = \varsigma$	4·94 ,
$s = \frac{3}{2}\varsigma$	12·97 ,
$s = 2\varsigma$	28·75 .

If these values be introduced in the formula

$$\frac{T}{\tau} = \cdot 1780\, K \left(\frac{\varsigma}{s}\right)^{\frac{3}{2}},$$

we obtain

	T/τ
$s = \frac{1}{2}\varsigma$	·86 ,
$s = \frac{3}{4}\varsigma$	·80 ,
$s = \varsigma$	·88 ,
$s = \frac{3}{2}\varsigma$	1·26 ,
$s = 2\varsigma$	1·81 .

These values are used for forming the curve, entitled "frequency of collision," in fig. 1 below, and they are supplemented by the values found above for T/τ, in the case where s/ς is either very small or very large.

The frequency becomes infinite when the balls are infinitely small, because of the infinite velocity with which they move, and again infinite for infinitely large balls, because of their infinite size. But it must be remembered that there are infinitely few balls of these two limiting sizes.

We have now to consider the mean free path, say λ, for the several sizes.

If u^2 be the mean square of velocity for the size s, the mean velocity for that size is $u\sqrt{(8/3\pi)}$, by the ordinary kinetic theory.

From the constancy of mean kinetic energy for all sizes, we have

$$s^3 u^2 = \varsigma^3 V^2,$$

so that the mean velocity for size s is

$$V(\varsigma/s)^{\frac{3}{2}}\sqrt{(8/3\pi)}.$$

But, if U be the mean velocity, and L the mean free path, and T the mean free time for all sizes together, we have

$$V = \frac{U}{1{\cdot}9138} = \frac{1}{1{\cdot}9138}\frac{L}{T}.$$

Therefore, the mean velocity for size s is

$$\frac{\sqrt{(8/3\pi)}}{1{\cdot}9138}\left(\frac{\varsigma}{s}\right)^{\frac{3}{2}}\frac{L}{T} = {\cdot}4815\left(\frac{\varsigma}{s}\right)^{\frac{3}{2}}\frac{L}{T}.$$

But the mean velocity for size s is λ/τ; hence,

$$\frac{\lambda}{\mathrm{L}} = {\cdot}4815\left(\frac{\varsigma}{s}\right)^{\frac{3}{2}}\frac{\tau}{\mathrm{T}} = \frac{4815}{1780}\cdot\frac{1}{\mathrm{K}}$$

$$= \frac{2{\cdot}705}{\mathrm{K}}.$$

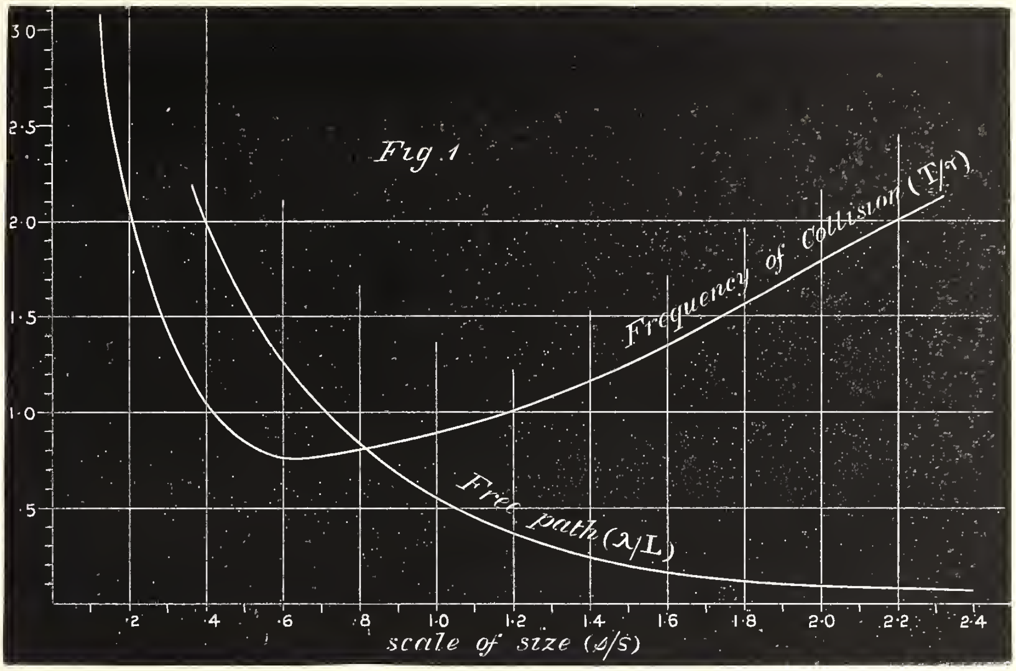
When s is very small, we find $\lambda/\mathrm{L} = 4$, and, when s is very large, $\lambda/\mathrm{L} = 1{\cdot}7\,(\varsigma/s)^{\frac{7}{2}}$. Thus, for small values of s, the mean free path reaches a constant limit 4, and for large values it becomes infinitely small.

The intermediate values, sufficient for drawing a curve, are given in the following short table :—

	λ/L.
$s = \frac{1}{2}\varsigma$	1·58.
$s = \frac{3}{4}\varsigma$	·93.
$s = \varsigma$	·55.
$s = \frac{3}{2}\varsigma$	·21.
$s = 2\varsigma$	·09.

These values are set out in the annexed figure in the curve marked "free path," and are supplemented by the values found above for small and large values of s. The constant limit 4 falls outside the figure. The horizontal portion of the curve is asymptotic to the s-axis.

Fig. 1.



No immediate use is made of these conclusions, but it was proper to examine this point in the theory.

§ 16. *On the Sorting of Meteorites according to size and its Results.*

It is a well-known result of the kinetic theory of gases, that if a number of different gases co-exist, each gas has the same density as though it alone existed, and was subject to the resultant forces of the system; also the mean kinetic energy of agitation of each gas is the same. From this it follows that the elasticity of each gas is inversely proportional to the mass of its molecule.

Carrying on this conclusion to meteorites, we see that the elasticity of the gas formed by large meteorites is less than that for small; and, hence, there is a greater concentration of large meteorites towards the centre, and there will be a sorting according to size. The object of this section is to investigate this point.

In §§ 14 and 15, the laws of a kinetic theory were investigated when the gas consisted of molecules of all masses, grouped, according to a law of frequency, about a certain mean radius, and molecules of infinite mass were considered to be admissible, with, of course, infinite rarity. Now, if we were to continue to use that law of frequency of masses in the present investigation, we should find, as an analytical result, that the mean mass in the centre of the swarm becomes infinite. The existence of very large meteorites in sufficient numbers to give statistical constancy in a volume which is not a considerable fraction of the volume of the whole swarm is physically improbable. We shall, therefore, treat the case best by absolutely excluding very large masses. When such masses occur, they must not be treated statistically; this is a question which I hope to consider in a future paper. Had I foreseen this conclusion when the investigations of the last two sections were carried out, a different law of frequency of mass would have been assumed. But the results of those sections are amply sufficient to indicate the conclusions which would have been reached with another law of frequency, and, therefore, it does not seem worth while to recompute the results by means of a fresh series of laborious quadratures.

Any law of frequency would suffice for our purpose which excludes masses greater than a certain limit and rises to a maximum for a certain mean mass. For the present, I do not specify that law precisely, but merely assume that at some radius, which may conveniently be taken as that of the isothermal sphere, where $r = a$, the number of meteorites whose masses lie between x and $x + \delta x$ is $f(x)\,\delta x$; it is also assumed that x may range from M* to zero.

The meteorites whose masses range from x to $x + \delta x$ may be deemed to constitute a gas. Suppose that at radius r the number of its molecules per unit volume is δn, its density δw, its pressure δp, and let the same symbols, with suffix 0, denote the same things at radius a. Since all the partial gases are in the permanent state, they all have the same mean kinetic energy of agitation, equal to $\frac{1}{2}h$, suppose. Throughout the isothermal sphere, this h is constant, and equal, say, to h_0, but varies with the radius in the adiabatic layer over it. It follows, therefore, that the mean square of the velocity of the particular partial gas x to $x + \delta x$ is equal to h/x, and the relation between δp and δw is

$$\delta p = \tfrac{1}{3}\frac{h}{x}\,\delta w.$$

Let $-\chi$ be the excess of the gravitation potential of the *whole* swarm at radius r above its value at radius a.

Then, since each partial gas behaves as though it existed by itself, the equation of hydrostatic equilibrium of the partial gas x to $x + \delta x$ is

$$\frac{1}{\delta w}\frac{d\,\delta p}{dr} + \frac{d\chi}{dr} = 0.$$

The investigation must now divide into two, according as whether we are considering the isothermal sphere or the adiabatic layer.

The Isothermal Sphere.

Here we have h a constant and equal to h_0, and δp varies as δw, so that

* This M is not to be confused with M, the mass of the isothermal sphere.

$$\tfrac{1}{3}\frac{h_0}{x}\log\frac{\delta w}{\delta w_0} = -\chi,$$

or

$$\frac{\delta w}{\delta w_0} = e^{-3\chi x/h_0}.$$

Now it is obvious that $\delta n/\delta n_0 = \delta w/\delta w_0$; and, therefore,

$$\delta n = e^{-3\chi x/h_0}\,\delta n_0.$$

But, by the definition of $f(x)$,

$$\delta n_0 = f(x)\,\delta x\,;$$

hence,

$$\delta n = e^{-3\chi x/h_0} f(x)\,\delta x. \qquad (55)$$

This is the law of frequency of mass x to $x + \delta x$ at radius r.

Now, if m, m_0 be the mean masses at radii r and a respectively,

$$m = \frac{\int_0^M x\,e^{-3\chi x/h_0} f(x)\,dx}{\int_0^M e^{-3\chi x/h_0} f(x)\,dx}; \qquad (56)$$

and, if we put $\chi = 0$, we obtain m_0 from the same formula.

It is also clear that, if w be the total density of the swarm at radius r,

$$w = \int x\,dn = \int_0^M x\,e^{-3\chi x/h_0} f(x)\,dx. \qquad (57)$$

By the definition of χ, and in consequence of the supposed spherical arrangement of matter, we have

$$\chi = \int_a^r \frac{1}{r^2}\left(\int_o^r 4\pi\mu w r^2 dr\right) dr.$$

If this value were substituted in (57), we should obtain a very complicated differential equation to determine w, the solution of which is hopelessly difficult. We may, however, assume without much error that the w in the integral expressing χ is the density of meteorites, all of which are of the same size m', and which are agitated with mean kinetic energy $\tfrac{1}{2}h_0$. If this density be written w, we then clearly have

$$\chi = -\frac{h_0}{3m'}\log\frac{\mathrm{w}}{\mathrm{w}_0}.$$

The values of w and w_0 may be extracted from Table III. of solutions in § 6.

Then we have

$$-\frac{3\chi x}{h_0} = \frac{x}{m'}\log\frac{\text{w}}{\text{w}_0} = qx, \text{ suppose,}$$

where q is rigorously equal to $-3\chi/h_0$; but for computing the approximate value $(1/m')\log(\text{w}/\text{w}_0)$ is to be employed.

In order to proceed to the evaluation of the mean mass at various distances, we must assume some form for $f(x)$.

I assume, then, that

$$f(x) = \frac{6n_0}{\text{M}^3}x(\text{M}-x).$$

It is easy to show that

$$\int_0^{\text{M}} f(x)\,dx = n_0, \quad \text{and} \quad \frac{1}{n_0}\int_0^{\text{M}} xf(x)\,dx = \tfrac{1}{2}\text{M}.$$

Hence, the mean mass $m_0 = \frac{1}{2}\text{M}$, and the maximum frequency is for masses equal to m_0.

Then, by (56), we have for the mean mass at radius r

$$m = \frac{\int_0^{\text{M}} x^2(\text{M}-x)\,e^{qx}dx}{\int_0^{\text{M}} x(\text{M}-x)\,e^{qx}dx}.$$

But

$$\left.\begin{aligned}\int_0^{\text{M}} x^2(\text{M}-x)\,e^{qx}dx &= \frac{1}{q^4}\left[e^{\text{M}q}(\text{M}^2q^2-4\text{M}q+6)-2(\text{M}q+3)\right],\\ \int_0^{\text{M}} x(\text{M}-x)\,e^{qx}dx &= \frac{1}{q^3}\left[e^{\text{M}q}(\text{M}q-2)+(\text{M}q+2)\right].\end{aligned}\right\} \quad . \quad . \quad (58)$$

It may be remarked that, if $\text{M}q$ be treated as small, we have the first of these integrals equal to $\frac{1}{12}\text{M}^4(1+\frac{3}{5}\text{M}q)$, and the second equal to $\frac{1}{6}\text{M}^3(1+\frac{1}{2}\text{M}q)$, and the ratio of the first to the second is $\frac{1}{2}\text{M}(1+\frac{1}{10}\text{M}q)$.

In order to evaluate m, we proceed to introduce the approximate value for q.

Now,

$$q = \frac{1}{m'}\log\frac{\text{w}}{\text{w}_0}, \quad \text{and} \quad e^{\text{M}q} = \left(\frac{\text{w}}{\text{w}_0}\right)^{\text{M}/m'};$$

then, writing for brevity,

$$P = \log\left(\frac{\text{w}}{\text{w}_0}\right)^{\text{M}/m'},$$

we have

$$\frac{m}{\frac{1}{2}M} = \frac{2}{P}\cdot\frac{\left(\frac{\text{w}}{\text{w}_0}\right)^{\text{M}/m'}(P^2-4P+6)-2(P+3)}{\left(\frac{\text{w}}{\text{w}_0}\right)^{\text{M}/m'}(P-2)+(P+2)}. \quad . \quad . \quad . \quad . \quad . \quad (59)$$

Also, if P be small, the approximate result is

$$\frac{m}{\frac{1}{2}\mathrm{M}} = 1 + \tfrac{1}{10}P.$$

Before proceeding to give numerical values for the fall of mean mass as we proceed outwards from the centre of the isothermal sphere, we must consider

The Adiabatic Layer.

In this case we assume, as before, that the ratio of the two specific heats is $1\frac{2}{3}$, and we therefore have for the relationship between δp and δw at radius r,

$$\frac{\delta p}{\delta p_0} = \left(\frac{\delta w}{\delta w_0}\right)^{\frac{5}{3}}.$$

Hence,

$$\frac{1}{\delta w}\frac{d\,\delta p}{dr} = \tfrac{5}{6}\left[\frac{3\,\delta p_0}{\delta w_0}\right]\frac{d}{dr}\left(\frac{\delta w}{\delta w_0}\right)^{\frac{2}{3}}.$$

But, since δp_0, δw_0 apply to the radius a where $h = h_0$, a constant,

$$\frac{3\,\delta p_0}{\delta w_0} = \frac{h_0}{x}.$$

Thus, in the adiabatic layer the equation of hydrostatic equilibrium is

$$\tfrac{5}{6}\frac{h_0}{x}\frac{d}{dr}\left(\frac{\delta w}{\delta w_0}\right)^{\frac{2}{3}} + \frac{d\chi}{dr} = 0,$$

whence,

$$\chi = \tfrac{5}{6}\frac{h_0}{x}\left(1 - \left(\frac{\delta w}{\delta w_0}\right)^{\frac{2}{3}}\right), \qquad (60)$$

or

$$\delta w = \delta w_0\left[1 - \frac{6\chi x}{5h_0}\right]^{\frac{3}{2}}.$$

The investigation now follows a line parallel to that taken before.

We have $\delta n/\delta n_0 = \delta w/\delta w_0$, and $\delta n_0 = f(x)\,dx$, so that

$$\delta n = \left(1 - \frac{6\chi x}{5h_0}\right)^{\frac{3}{2}} f(x)\,\delta x.$$

This is the law of frequency of masses lying between x and $x + \delta x$ at radius r.

As δn can never be negative, we see that there can be no mass greater than $\frac{5}{6}h_0/\chi$; and, if M be the greatest positive value of the expression $f(x)$, there can be no mass greater than the smaller of $\frac{5}{6}h_0/\chi$ or M.

Thus, if m be the mean mass at radius r,

$$m = \frac{\int_0^a x\left(1 - \frac{6\chi x}{5h_0}\right)^{\frac{3}{2}} f(x)\,dx}{\int_0^a \left(1 - \frac{6\chi x}{5h_0}\right)^{\frac{3}{2}} f(x)\,dx}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (61)$$

where a is the smaller of $\frac{5}{6} h_0/\chi$ and M.

If we put $\chi = 0$ in (61), we obtain m_0, the mean mass at radius a.

It is clear also that, if w be the total density of the swarm at radius r,

$$w = \int x\,dn = \int_0^a x\left(1 - \frac{6\chi x}{5h_0}\right)^{\frac{3}{2}} f(x)\,dx. \quad . \quad . \quad . \quad . \quad . \quad (62)$$

By definition of χ, and in consequence of the supposed spherical arrangement of matter, we have

$$\chi = \int_a^r \frac{1}{r^2}\left(\int_0^r 4\pi\mu w r^2\,dr\right) dr.$$

If this value were substituted in (62), we might obtain a complicated differential equation for w. It is clear, however, that an adequate approximation may be obtained by assuming that the w in the integral expressing χ is the density of meteorites, all of which are of the same size m', arranged in a layer in convective equilibrium, and with kinetic energy of agitation at the limit $r = a$ equal to $\frac{1}{2} h_0$.

If this density be written w, and if v^2 be the mean square of velocity of agitation at radius r, we have, by (60), and in consequence of the relationship $(\text{w}/\text{w}_0)^{\frac{2}{3}} = (\text{v}/\text{v}_0)^2$,

$$\chi = \frac{5}{6}\frac{h_0}{m'}\left(1 - \left(\frac{\text{w}}{\text{w}_0}\right)^{\frac{2}{3}}\right) = \frac{5}{6}\text{v}_0^2\left(1 - \frac{\text{v}^2}{\text{v}_0^2}\right),$$

and

$$\frac{6}{5}\frac{\chi x}{h_0} = \frac{x}{m'}\left(1 - \frac{\text{v}^2}{\text{v}_0^2}\right).$$

Let

$$\frac{1}{\beta} = \frac{1}{m'}\left(1 - \frac{\text{v}^2}{\text{v}_0^2}\right)$$

for brevity ; then, adopting the law of frequency $f(x) = \frac{6\,n_0}{\text{M}^3} x\,(\text{M} - x)$, as before, we have for the mean mass at radius r

$$m = \frac{\int_0^a x^2(\text{M} - x)\left(1 - \frac{x}{\beta}\right)^{\frac{3}{2}} dx}{\int_0^a x(\text{M} - x)\left(1 - \frac{x}{\beta}\right)^{\frac{3}{2}} dx}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (63)$$

where a is equal to the smaller of M and β.

The solution now becomes different according as M or β is the smaller.

First, suppose M is the smaller. Then the limits of integration are M and 0.

If we put

$$z = 1 - \frac{x}{\beta}.$$

$$x^n \left(1 - \frac{x}{\beta}\right)^{\frac{3}{2}} dx = -\beta^{n+1} z^{\frac{3}{2}} \left(1 - nz + \frac{n \,.\, n-1}{1 \,.\, 2} z^2 - \ldots\right) dz,$$

so that the numerator and denominator of m are easily integrable.

If now we write

$$Q = 1 - \frac{\mathrm{M}}{\beta},$$

$$\int_0^{\mathrm{M}} z^2 (\mathrm{M} - x) \left(1 - \frac{x}{\beta}\right)^{\frac{3}{2}} dx = 2\beta^4 \left[\tfrac{1}{5}\left(\frac{\mathrm{M}}{\beta} - 1\right)(1 - Q^{\frac{5}{2}}) - \tfrac{1}{7}\left(2\frac{\mathrm{M}}{\beta} - 3\right)(1 - Q^{\frac{7}{2}}) + \tfrac{1}{9}\left(\frac{\mathrm{M}}{\beta} - 3\right)(1 - Q^{\frac{9}{2}}) + \tfrac{1}{11}(1 - Q^{\frac{11}{2}})\right]$$

$$= 2\beta^4 \left[\frac{8}{7 \,.\, 9 \,.\, 11} - \frac{8}{5 \,.\, 7 \,.\, 9} Q + \frac{2}{5 \,.\, 7} Q^{\frac{7}{2}} - \frac{4}{7 \,.\, 9} Q^{\frac{9}{2}} + \frac{2}{9 \,.\, 11} Q^{\frac{11}{2}}\right],$$

$$\int_0^{\mathrm{M}} x (\mathrm{M} - x) \left(1 - \frac{x}{\beta}\right)^{\frac{3}{2}} dx = 2\beta^3 \left[\tfrac{1}{5}\left(\frac{\mathrm{M}}{\beta} - 1\right)(1 - Q^{\frac{5}{2}}) - \tfrac{1}{7}\left(\frac{\mathrm{M}}{\beta} - 2\right)(1 - Q^{\frac{7}{2}}) - \tfrac{1}{9}(1 - Q^{\frac{9}{2}})\right]$$

$$= 2\beta^3 \left[\frac{2}{7 \,.\, 9} - \frac{2}{5 \,.\, 7} Q + \frac{2}{5 \,.\, 7} Q^{\frac{7}{2}} - \frac{2}{7 \,.\, 9} Q^{\frac{9}{2}}\right].$$

Then, since $\beta = (1 - Q)/M$, we have

$$\frac{m}{M} = \frac{\frac{4}{11} - \frac{4}{5} Q + \frac{9}{5} Q^{\frac{7}{2}} - 2 Q^{\frac{9}{2}} + \frac{7}{11} Q^{\frac{11}{2}}}{(1 - Q)(1 - \frac{9}{5} Q + \frac{9}{5} Q^{\frac{7}{2}} - Q^{\frac{9}{2}})} \,. \qquad \ldots \ldots \quad (64)$$

This expression has a high order of indeterminateness when $Q = 1$, but I find that when Q is nearly equal to unity

$$\frac{m}{\mathrm{M}} = \tfrac{1}{2}\left[1 - \tfrac{3}{10}(1 - Q^{\frac{1}{2}})\right] \text{ nearly.} \qquad \ldots \ldots \quad (65)$$

Thus, the mean mass is $\frac{1}{2}$M where $r = a$, which we know to be correct.

Secondly, suppose that β is smaller than M. Then effecting the integrations in the same manner as before, we have

$$\int_0^{\beta} x^2 (\mathrm{M} - x)\left(1 - \frac{x}{\beta}\right)^{\frac{3}{2}} dx = 2\beta^4 \left[\tfrac{1}{5}\left(\frac{\mathrm{M}}{\beta} - 1\right) - \tfrac{1}{7}\left(\frac{2\mathrm{M}}{\beta} - 3\right) + \tfrac{1}{9}\left(\frac{\mathrm{M}}{\beta} - 3\right) + \tfrac{1}{11}\right]$$

$$= \frac{2 \,.\, 8}{5 \,.\, 7 \,.\, 9} \beta^4 \left(\frac{\mathrm{M}}{\beta} - \tfrac{6}{11}\right),$$

$$\int_0^\beta x\,(\mathrm{M} - x)\left(1 - \frac{x}{\beta}\right)^{\frac{3}{2}} dx = 2\beta^3\left[\tfrac{1}{5}\left(\frac{\mathrm{M}}{\beta} - 1\right) - \tfrac{1}{7}\left(\frac{\mathrm{M}}{\beta} - 2\right) - \tfrac{1}{9}\right]$$

$$= \frac{2\,.\,2}{5\,.\,7}\beta^3\left[\frac{\mathrm{M}}{\beta} - \tfrac{4}{9}\right].$$

Therefore,

$$m = \tfrac{4}{9}\beta\,.\,\frac{\dfrac{\mathrm{M}}{\beta} - \tfrac{6}{11}}{\dfrac{\mathrm{M}}{\beta} - \tfrac{4}{9}},$$

or

$$\frac{m}{\mathrm{M}} = \tfrac{4}{9}\,.\,\frac{\dfrac{m'}{\mathrm{M}}}{1 - \dfrac{\mathrm{v}^2}{\mathrm{v}_0^{\,2}}}\,.\,\frac{\dfrac{\mathrm{M}}{m'}\left(1 - \dfrac{\mathrm{v}^2}{\mathrm{v}_0^{\,2}}\right) - \tfrac{6}{11}}{\dfrac{\mathrm{M}}{m'}\left(1 - \dfrac{\mathrm{v}^2}{\mathrm{v}_0^{\,2}}\right) - \tfrac{4}{9}}. \quad \ldots \ldots \quad (66)$$

In order to compute from the formulæ, (59), (64), (66), it is necessary to make an assumption as to the value of m' the mass of the meteorites of uniform size whose arrangement of density is supposed to be the same as that of the heterogeneous meteorites.

We have supposed that the law of frequency of masses is known at radius a, and that the mean mass is there equal to $\frac{1}{2}M$. Now, inside of that radius the larger masses are more frequent, and outside of it the smaller masses. I suppose, then, that throughout the isothermal sphere m' lies half way between m_0 or $\frac{1}{2}$M and the maximum mass M, and in the adiabatic layer that it lies half way between m_0 or $\frac{1}{2}$M and the minimum mass 0.

Thus, inside I take $m' = \frac{3}{4}$M, and outside $m' = \frac{1}{4}M$.

As we only want to consider the general nature of the sorting process, these assumptions will suffice. It may also be remarked that a large variation of m' is required to make any considerable difference in the numerical results.

We now have—

In the isothermal sphere (where $\mathrm{w}_0 = \frac{1}{3}\rho$),

$$\frac{\mathrm{M}}{m'} = \tfrac{4}{3}, \qquad \mathrm{P} = \log_e\left(\frac{\mathrm{w}}{\frac{1}{3}\rho}\right)^{\frac{4}{3}}, \qquad \tfrac{1}{2}\mathrm{M} = m_0\,;$$

In the adiabatic layer,

$$\frac{\mathrm{M}}{m'} = 4\,, \qquad Q = 1 - \frac{\mathrm{M}}{m'}\left(1 - \frac{\mathrm{v}^2}{\mathrm{v}_0^{\,2}}\right) = 4\frac{\mathrm{v}^2}{\mathrm{v}_0^{\,2}} - 3\,.$$

Thus, our formulæ are :—

In the isothermal sphere, from (59),

$$\frac{m}{m_0} = \frac{2}{P}\,.\,\frac{\left(\dfrac{\mathrm{w}}{\frac{1}{3}\rho}\right)^{\frac{4}{3}}(P^2 - 4P + 6) - 2\,(P + 3)}{\left(\dfrac{\mathrm{w}}{\frac{1}{3}\rho}\right)^{\frac{4}{3}}(P - 2) + (P + 2)}\,; \quad \ldots \ldots \quad (67)$$

in the adiabatic layer,

when $\dfrac{v^2}{v_0^2} > \frac{3}{4}$, from (64),

$$\frac{m}{m_0} = \frac{\frac{4}{11} - \frac{4}{5}Q + \frac{9}{5}Q^{\frac{7}{2}} - 2Q^{\frac{9}{2}} + \frac{7}{11}Q^{\frac{11}{2}}}{\frac{1}{2}(1-Q)(1 - \frac{9}{5}Q + \frac{9}{5}Q^{\frac{7}{2}} - Q^{\frac{9}{2}})}, \qquad \ldots \ldots \quad (68)$$

when $\dfrac{v^2}{v_0^2} < \frac{3}{4}$, from (66),

$$\frac{m}{m_0} = \frac{2}{9(1 - v^2/v_0^2)} \cdot \frac{\frac{19}{22} - v^2/v_0^2}{\frac{8}{9} - v^2/v_0^2}. \qquad \ldots \ldots \quad (69)$$

The values of $w/\frac{1}{3}\rho$ and of v^2/v_0^2 are tabulated in Table III., and from these I compute—

	isothermal.					$\frac{v^2}{v_0^2} > \frac{3}{4}$		$\frac{v^2}{v_0^2} < \frac{3}{4}$						
$\frac{r}{a} =$	0	·16	·48	·80	1·0	1·09	1·2	1·33	1·5	1·71	2·00	2·21	2·46	2·79
$\frac{m}{m_0} =$	1·41	1·38	1·22	1·11	1·0	·92	·83	·66	·49	·38	·30	·27	·24	·22

These values (together with two others in the isothermal part) are set out in fig. 2, and show the law of diminution of mean mass from centre to outside.

Fig. 2.

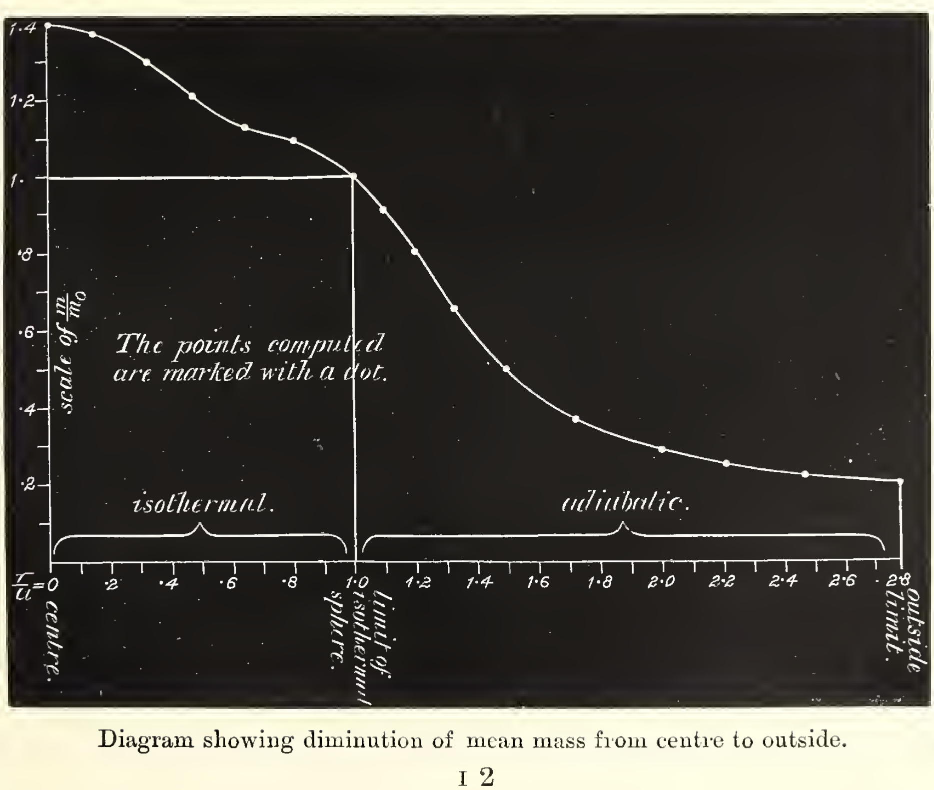


Diagram showing diminution of mean mass from centre to outside.

The evaluation of mean mass in the fringe (see § 13), where collisions are supposed to be non-existent, is not very difficult, although it involves some troublesome algebra. I do not give the investigation, merely remarking that it leads to almost exactly the same kind of law of diminution of mean mass as we have found in the adiabatic layer.

§ 17. *Summary.*

The first and second sections only involved arguments of a general character in which mathematical analysis was unnecessary. The reader who does not wish to concern himself with details may therefore be supposed to have passed from §§ 1 and 2 to this Summary.

In order to submit the theory to an adequate test, it is necessary to discuss some definite case of the aggregation of a swarm of meteorites, and it is obvious that the only system of which we possess any knowledge is our own. It is accordingly supposed that a number of meteorites have fallen together from a condition of wide dispersion, and have ultimately coalesced so as to leave the Sun and planets as their progeny. The object of this paper is to consider the mechanical condition of the system after the cessation of any considerable supply of meteorites from outside, and before the coalescence of the swarm into a star with attendant planets.

For the sake of simplicity, the meteorites are considered to be spherical, and are treated, at least in the first instance, as being of uniform size.

It is assumed provisionally that the kinetic theory of gases may be applied for the determination of the distribution of the meteorites in space. No account being taken of the rotation of the system, the meteorites will be arranged in concentric spherical layers of equal density of distribution, and the quasi-gas, whose molecules are meteorites, being compressible, the density will be greater towards the centre of the swarm.

The elasticity of a gas depends on the kinetic energy of agitation of its molecules; and, therefore, in order to determine the law of density in the swarm, we must know the distribution of kinetic energy of agitation. It is assumed that, when the swarm comes under our notice, uniformity of distribution of energy has been attained throughout a central sphere, which is surrounded by a layer of meteorites with that distribution of kinetic energy which in a gas corresponds to convective equilibrium. In other words, we have a quasi-isothermal sphere surrounded by what may be called an atmosphere in convective equilibrium, and with continuity of density and velocity of agitation at the sphere of separation. Since in a gas in convective equilibrium the law connecting pressure and density is that which holds when the gas is contained in a vessel impermeable to heat, such an arrangement of gas has been called by M. Ritter "an isothermal-adiabatic sphere," and the same term is adopted here as applicable to a swarm of meteorites. The justifiability of these assumptions will be considered later.

The first problem which presents itself, then, is the equilibrium of an isothermal sphere of gas under its own gravitation. The law of density is determined in § 4; but it will here suffice to remark that, if a given mass be enclosed in an envelope of given radius, there is a minimum temperature (or energy of agitation) at which isothermal equilibrium is possible. The minimum energy of agitation is found to be such that the mean square of velocity of the meteorites is almost exactly $\frac{6}{5}$ (viz. 1·1917) of the square of the velocity of a satellite grazing the surface of the sphere in a circular orbit.

As indicated above, it is supposed that in the meteor-swarm the rigid envelope bounding the isothermal sphere is replaced by a layer of meteorites in convective equilibrium. The law of density in the adiabatic layer is determined in § 5, and it appears that, when the isothermal sphere has minimum temperature, the mass of the adiabatic atmosphere is a minimum relatively to that of the isothermal sphere. Numerical calculation shows, in fact, that the isothermal sphere cannot amount in mass to more than 46 per cent. of the mass of the whole isothermal-adiabatic sphere, and that the limit of the adiabatic atmosphere is at a distance equal to 2·786 times the radius of the isothermal sphere.* A table of various quantities in such a system, at various distances from the centre, is given in Table III., § 6.

It is next proved, in § 7, that the total energy, existing in the form of energy of agitation in an isothermal-adiabatic sphere, is exactly one-half of the potential energy lost in the concentration of the matter from a condition of infinite dispersion. This result is brought about by a continual transfer of energy from a molar to a molecular form, for a portion of the kinetic energy of a meteorite is constantly being transferred into the form of thermal energy in the volatilised gases generated on collision. The thermal energy is then lost by radiation.

It is impossible as yet to sum up all the considerations which go to justify the assumption of the isothermal-adiabatic arrangement; but it is clear that uniformity of kinetic energy of agitation in the isothermal sphere must be principally brought about by a process of diffusion. It is, therefore, interesting to consider what amount of inequality in the kinetic energy would have to be smoothed away.

The arrangement of density in the isothermal-adiabatic sphere being given, it is easy to compute what the kinetic energy would be at any part of the swarm, if each meteorite fell from infinity to the neighbourhood where we find it, and there retained all the velocity due to such fall. The variation of the square of this velocity gives an indication of the amount of inequality of kinetic energy which has to be degraded by conversion into heat and redistributed by diffusion in the attainment of uniformity. This may be called "the theoretical value of the kinetic energy"; it is tabulated in Table III., on the line called "square of velocity of satellite." It rises from zero at the centre of the sphere to a maximum, which is attained nearly half way through the adiabatic layer, and then falls again. If the radius of the isothermal sphere be unity, then from $\frac{1}{2}$ to 2 the variations of this theoretical value of the kinetic energy

* These results had been previously discovered by M. RITTER.

are small. Since this "theoretical value of the kinetic energy" is zero at the centre, there must have been diffusion of energy from without inwards, and considerations of the same kind show that when a planet consolidates there must be a cooling of the middle strata both outwards and inwards.

We must now consider the nature of the criterion which determines whether the hydrodynamical treatment of a swarm of meteorites is permissible.

The hydrodynamical treatment of an ideal plenum of gas leads to the same result as the kinetic theory with regard to any phenomenon involving purely a mass, when that mass is a large multiple of the mass of a molecule ; to any phenomenon involving purely a length, when the cube of that length contains a large number of molecules ; and to any phenomenon involving purely a time, when that time is a large multiple of the mean interval between collisions. Again, any velocity to be justly deduced from hydrodynamical principles must be expressible as the edge of a cube containing many molecules passed over in a time containing many collisions of a single molecule ; and a similar statement must hold of any other function of mass, length, and time.

Beyond these limits, we must go back to the kinetic theory itself, and in using it care must be taken that enough molecules are considered at once to impart statistical constancy to their properties.

There are limits, then, to the hydrodynamical treatment of gases, and the like must hold of the parallel treatment of meteorites.

The principal question involved in the nebular hypothesis seems to be the stability of a rotating mass of gas ; but, unfortunately, this has remained up to now an untouched field of mathematical research. We can only judge of probable results from the investigations which have been made concerning the stability of a rotating mass of liquid. Now, it appears that the instability of a rotating mass of liquid first enters through the graver modes of gravitational oscillation. In the case of a rotating spheroid of revolution the gravest mode of oscillation is an elliptic deformation, and its period does not differ much from that of a satellite which revolves round the spheroid so as to graze its surface. Hence, assuming for the moment that a kinetic theory of liquids had been formulated, we should not be justified in applying the hydrodynamical method to this discussion of stability unless the periodic time of such a satellite were a large multiple of the analogue of the mean free time of a molecule of liquid.*

Carrying, then, this conclusion on to the kinetic theory of meteorites, it seems probable that hydrodynamical treatment must be inapplicable for the discussion of such a theory as the meteoric-nebular hypothesis, unless a similar relation holds good.

These considerations, although of a very general character, will afford a criterion of the applicability of hydrodynamics to the discussion of the mechanical conditions of a swarm of meteorites in the kind of problem suggested by the nebular hypothesis.

* If the molecules of liquid describe orbits about one another, the analogue would probably be the mean periodic time of one molecule about another.

In § 9 two criteria, suggested by this line of thought, are found. They measure, roughly speaking, the degree of curvature of the average path pursued by a meteorite between two collisions. These two criteria, denoted D/L and A/C, will afford a measure of the applicability of hydrodynamics in the sense above indicated.

After these preliminary investigations, we have to consider what kind of meeting of two meteorites will amount to an "encounter" within the meaning of the kinetic theory. Is it possible, in fact, that two meteorites can considerably bend their paths under the influence of gravitation when they pass near one another? This question is answered in § 8, where a formula is found for the deflection of the path of each of a pair of meteorites, when, moving with their mean relative velocity, they graze past one another without striking. It appears from the formula that, unless they have the dimensions of small planets, the mutual gravitational influence is practically insensible. Hence, nothing short of absolute impact is to be considered an encounter in the kinetic theory; and what is called the radius of "the sphere of action" is simply the distance between the centres of a pair when they graze, and is, therefore, the sum of their radii, or, if of uniform size, the diameter of one of them.

The next point to consider is the mass and size which must be attributed to the meteorites.

The few samples which have been found on the earth prove that no great error can be committed if the average density of a meteorite be taken as a little less than that of iron, and I accordingly suppose their density to be six times that of water.

Undoubtedly, in a swarm of meteorites all sizes exist (a supposition considered hereafter); for, even if originally of one uniform size, they would, by subsequent fracture, be rendered diverse. But in the first consideration of the problem they have been treated as of uniform size, and, as actual average sizes are nearly unknown, results are given in the numerical table for meteorites weighing $3\frac{1}{8}$ grammes. By merely shifting the decimal point one, two, or three places to the right the results become applicable to meteorites weighing $3\frac{1}{8}$ kilogrammes, $3\frac{1}{8}$ tonnes, 3125 tonnes, and so on.

It is known that meteorites are actually of irregular shapes, but certainly no material error can be incurred when we treat them as being spheres.

The object of all these investigations is to apply the formulæ to a concrete example. The mass of the system is therefore taken as equal to that of the Sun, and the limit of the swarm at any arbitrary distance from the present Sun's centre. The theory is, of course, most severely tested the wider the dispersion of the swarm; and, accordingly, in the numerical example the outside limit of the Solar swarm is taken at $44\frac{1}{2}$ times the Earth's distance from the Sun, or further beyond the planet Neptune than Saturn is from the Sun. This assumption makes the limit of the isothermal sphere at distance 16, about half way between Saturn and Uranus.

The results, applicable to meteorites of $3\frac{1}{8}$ grammes, are exhibited in Table IV., § 10.

The velocity of mean square in the isothermal sphere is $\surd(6/5)$ of the linear velocity of a planet at distance 16, revolving about a central body with a mass equal to 46 per cent. of that of the Sun, viz., $5\frac{1}{3}$ kilometres per second; and in the adiabatic layer it diminishes down to zero at distance $44\frac{1}{2}$. This velocity is independent of the size of the meteorites.

The mean free path between collisions ranges from 42,000 kilometres at the centre, to 1,300,000 kilometres at radius 16, and to infinity at radius $44\frac{1}{2}$. The mean interval between collisions ranges from a tenth of a day at the centre, to three days at radius 16, and to infinity at radius $44\frac{1}{2}$. The criterion D/L ranges from $\frac{1}{60,000}$ at the distance of the asteroids, to $\frac{1}{3600}$ at radius 16, and to infinity at radius $44\frac{1}{2}$. The criterion A/C is somewhat smaller than D/L. All these quantities are ten times as great for meteorites of $3\frac{1}{8}$ kilogrammes, and a hundred times as great for meteorites of $3\frac{1}{8}$ tonnes.

From a consideration of the table it appears that, with meteorites of $3\frac{1}{8}$ kilogrammes, the collisions are sufficiently frequent, even beyond the orbit of Neptune, to allow the kinetic theory to be applicable in the sense explained. But, if the meteorites weigh $3\frac{1}{8}$ tonnes, the criteria cease to be very small about distance 24; and, if they weigh 3125 tonnes, they cease to be very small at about the orbit of Jupiter.

It may be concluded, then, that, as far as frequency of collision is concerned, the hydrodynamical treatment of a swarm of meteorites is justifiable.

Although these numerical results are necessarily affected by the conjectural values of the mass and density of the meteorites, yet it was impossible to arrive at any conclusion whatever as to the validity of the theory without numerical values, and such a discussion as the above was therefore necessary. If the particular values used are not such as to commend themselves to the judgment of the reader, it is easy to substitute others in the formulæ, and so submit the theory to another test.

I now pass on to consider some results of this view of a swarm of meteorites, and to consider the justifiability of the assumption of an isothermal-adiabatic arrangement of density.

With regard to the uniformity of distribution of kinetic energy in the isothermal sphere, it is important to ask whether or not sufficient time can have elapsed in the history of the system to allow of the equalisation by diffusion.

In § 11 the rate of diffusion of the kinetic energy of agitation is considered, and it is shown that, in the case of our numerical example, primitive inequalities of distribution would, in a few thousand years, be sensibly equalised over a distance some ten times as great as our distance from the Sun. This result, then, goes to show that we are justified in assuming an isothermal sphere as the centre of the swarm. As, however, the swarm contracts, the rate of diffusion diminishes as the inverse $\frac{5}{2}$ power of its linear dimensions, whilst the rate of generation of inequalities of distribution of kinetic energy, through the imperfect elasticity of the meteorites, increases. Hence, in a late stage of the swarm inequalities of kinetic energy would be set up; thus, there would be a tendency to the production of convective currents, and the whole swarm would probably settle down to the condition of convective equilibrium throughout.

It may be conjectured, then, that the best hypothesis in the early stages of the swarm is the isothermal adiabatic arrangement, and later an adiabatic sphere. It has not seemed to me worth while to discuss the latter hypothesis in detail at present.

The investigation of § 11 also gives the coefficient of viscosity of the quasi-gas, and shows that it is so great that the meteor-swarm must, if rotating, revolve nearly without relative motion of its parts, other than the motion of agitation. But, as the viscosity diminishes when the swarm contracts, this would probably not be true in the later stages of its history, and the central portion would probably rotate more rapidly than the outside. It forms, however, no part of the scope of this paper to consider the rotation of the system.

In § 12 the rate of loss of kinetic energy through imperfect elasticity is considered, and it appears that the rate estimated per unit time and volume must vary directly as the square of the quasi-pressure and inversely as the mean velocity of agitation. Since the kinetic energy lost is taken up in volatilising solid matter, it follows that the heat generated must follow the same law. The mean temperature of the gases generated in any part of the swarm depends on a great variety of circumstances, but it seems probable that its variation would be according to some law of the same kind. Thus, if the spectroscope enables us to form an idea of the temperature in various parts of a nebula, we shall at the same time obtain some idea of the distribution of density.

It has been assumed that the outer portion of the swarm is in convective equilibrium, and therefore there is a definite limit beyond which it cannot extend. Now, a medium can only be said to be in convective equilibrium when it obeys the laws of gases, and the applicability of those laws depends on the frequency of collisions. But at the boundary of the adiabatic layer the velocity of agitation vanishes, and collisions become infinitely rare. These two propositions are mutually destructive of one another, and it is impossible to push the conception of convective equilibrium to its logical conclusion. There must, in fact, be some degree of rarity of density, and of collisions, at which the statistical treatment of the medium breaks down.

I have sought to obtain some representation of the state of things by supposing that collisions never occur beyond a certain distance from the centre of the swarm. Then, from every point of the surface of the sphere, which limits the regions of collisions, a fountain of meteorites is shot out, in all azimuths and inclinations to the vertical, and with velocities grouped about a mean according to the law of error. These meteorites ascend to various heights without collision, and, in falling back on to the limiting sphere, cannonade its surface, so as to counterbalance the hydrostatic pressure at the limiting sphere.

The distribution of meteorites, thus shot out, is investigated in § 13, and it is found that near the limiting sphere the decrease in density is somewhat more rapid than the decrease corresponding to convective equilibrium. But at more remote distances the decrease is less rapid, and the density ultimately tends to vary inversely as the square of the distance from the centre.

It is clear, then, that, according to this hypothesis, the mass of the system is infinite in a mathematical sense, for the existence of meteorites with nearly parabolic and hyperbolic orbits necessitates an infinite number, if the loss of the system shall be made good by the supply.*

But, if we consider the subject from a physical point of view, this conclusion appears unobjectionable. The ejection of molecules with exceptionally high velocities from the surface of a liquid is called evaporation, and the absorption of others is called condensation. The general history of a swarm, as stated in § 2, may then be put in different words, for we may say that at first a swarm gains by condensation, that condensation and evaporation balance, and, finally, that evaporation gains the day.

If the hypothesis of convective equilibrium be pushed to its logical conclusion, we reach a definite limit to the swarm, whereas, if collisions be entirely annulled, the density goes on decreasing inversely as the square of the distance. The truth must clearly lie between these two hypotheses. It is thus certain that even the very small amount of evaporation shown by the formulæ derived from the hypothesis of no collisions must be in excess of the truth; and it may be that there are enough waifs and strays in space, ejected from other systems, to make up for the loss. Whether or not the compensation is perfect, a swarm of meteorites would pursue its evolution without being sensibly affected by a slow evaporation.

Up to this point the meteorites have been considered as of uniform size, but it is well to examine the more truthful hypothesis, that they are of all sizes, grouped about a mean according to a law of error.

It appears, from the investigation in § 14, that the larger stones move slower, the smaller ones faster; and the law is that the mean kinetic energy is the same for all sizes.

It is proved that the mean path between collisions is shorter in the proportion of 7 to 11, and the mean frequency greater in the proportion of 4 to 3, than if the meteorites were of uniform mass, equal to their mean. Hence, the previous numerical results for uniform size are applicable to non-uniform meteorites of mean mass about a third greater than the uniform mass; for example, the results for uniform meteorites of $3\frac{1}{8}$ tonnes apply to non-uniform ones of mean mass, a little over 4 tonnes.

The means here spoken of refer to all sizes grouped together, but there are a separate mean free path and a mean frequency appropriate to each size. These are investigated

[* It must also be borne in mind that the very high velocities, which occur occasionally in a medium with perfectly elastic molecules, must happen with great rarity amongst meteorites. An impact of such violence that it *ought* to generate a hyperbolic velocity will probably merely cause fracture.—Added Nov. 23, 1888.]

in § 15, and their various values are illustrated in fig. 1. The horizontal scale in that figure gives the ratio of the radius of each size to the radius of the meteorite of mean mass. The vertical scales are the ratio of the mean free path of any size to that of all sizes together, and the ratio of the mean frequency for any size to that of all sizes together. The figure shows that collisions become infinitely frequent for the infinitely small ones, because of their infinite velocity; and again infinitely frequent for the infinitely large ones, because of their infinite size. There is a minimum frequency of collision for a certain size, a little less in radius than the mean, and considerably less in mass than the mean mass.

For infinitely small meteorites, the mean free path reaches a finite limit, equal to about four times the grand mean free path; but this could not be shown in the figure without a considerable extension of it upwards. For infinitely large ones, the mean free path becomes infinitely short. It must be borne in mind that there are infinitely few of the infinitely large and small meteorites.

Variety of size does not, then, so far, materially affect the results.

But a difference arises when we come to consider the different parts of the swarm. The larger meteorites, moving with smaller velocities, form a quasi-gas of less elasticity than do the smaller ones. Hence, the larger meteorites are more condensed towards the centre than are the smaller ones, or the large ones have a tendency to sink down, whilst the small ones have a tendency to rise. Accordingly, the various kinds are to some extent sorted according to size.

In § 16, an investigation is made of the mean mass of the meteorites at various distances from the centre, both inside and outside of the isothermal sphere, and fig. 2 is drawn to illustrate the law of diminution of mean mass.

It is also clear that the loss of the system through evaporation must fall more heavily on the small meteorites than on the large ones.

After the foregoing summary, it will be well to briefly recapitulate the principal conclusions which seem to be legitimately deducible from the whole investigation; and, in this recapitulation, qualifications must necessarily be omitted, or stated with great brevity.

When two meteorites are in collision, they are virtually highly elastic, although ordinary elasticity must be nearly inoperative.

A swarm of meteorites is analogous with a gas, and the laws governing gases may be applied to the discussion of its mechanical properties. This is true of the swarm from which the Solar system was formed, when it extended beyond the orbit of the planet Neptune.

When the swarm was very widely dispersed, the arrangement of density and of velocity of agitation of the meteorites was that of an isothermal-adiabatic sphere. Later in its history, when the swarm had contracted, it was probably throughout in convective equilibrium.

The actual mean velocity of the meteorites is determinable in a swarm of given mass, when expanded to a given extent.

The total energy of agitation in an isothermal-adiabatic sphere is half the potential energy lost in the concentration from a condition of infinite dispersion.

The half of the potential energy lost, which does not reappear as kinetic energy of agitation, is expended in volatilising solid matter and heating the gases produced on the impact of meteorites. The heat so generated is gradually lost by radiation.

The amount of heat generated per unit time and volume varies as the square of the quasi-hydrostatic pressure, and inversely as the mean velocity of agitation. The temperature of the gases volatilised probably varies by some law of the same nature.

The path of the meteorites is approximately straight, except when abruptly deflected by a collision with another. This ceases to be true at the outskirts of the swarm, where the collisions have become rare. The meteorites here describe orbits, under gravity, which are approximately elliptic, parabolic, and hyperbolic.

In this fringe to the swarm the distribution of density ceases to be that of a gas under gravity, and, as we recede from the centre, the density at first decreases more rapidly, and afterwards less rapidly, than if the medium were a gas.

Throughout all stages of the history of a swarm there is a sort of evaporation, by which the swarm very slowly loses in mass, but this loss is more or less counterbalanced by condensation. In the early stages, the gain by condensation outbalances the loss by evaporation; they then equilibrate; and, finally, the evaporation may be greater than the condensation.

Throughout the swarm the meteorites are partially sorted, according to size. As we recede from the centre, the number of small ones preponderates more and more and, thus, the mean mass continually diminishes with increasing distance. The loss to the system by evaporation falls principally on the smaller meteorites.

A meteor-swarm is subject to gaseous viscosity, which is greater the more widely diffused is the swarm. In consequence of this, a widely extended swarm, if in rotation, will revolve like a rigid body, without relative movement of its parts. Later in its history, the viscosity will, probably, not suffice to secure uniformity of rotation, and the central portion will revolve more rapidly than the outside.

[The kinetic theory of meteorites may be held to present a fair approximation to the truth in the earlier stages of the evolution of the system. But ultimately the majority of the meteors must have been absorbed by the central Sun and its attendant planets, and amongst the meteors which remain free the relative motion of agitation must have been largely diminished. These free meteorites—the dust and refuse of the system—probably move in clouds, but with so little remaining motion of agitation that (except, perhaps, near the perihelion of very eccentric orbits) it would scarcely be permissible to treat the cloud as in any respects possessing the mechanical properties of a gas.*]

The value of this whole investigation will appear very different to different minds. To some it will stand condemned, as altogether too speculative; others may think that

* Added Nov. 23, 1888.

it is better to risk error on the chance of winning truth. To me, at least, it appears that the line of thought flows in a true channel; that it may help to give a meaning to the observations of the spectroscopist; and that many interesting problems, here barely alluded to, may, perhaps, be solved with sufficient completeness to throw light on the evolution of nebulæ and of planetary systems.

II. *A Class of Functional Invariants.*

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The investigations herein contained are indirectly connected with some results in an earlier memoir.* In that memoir functions called quotient-derivatives are obtained in the form of certain combinations of differential coefficients of a quantity y dependent on a single independent variable x; and they are there shown to possess the property of invariance for isolated homographic transformations of the dependent and the independent variables. It is evident, however, from their form that they do not constitute the complete aggregate of irreducible invariants for the case of a single independent variable; and the deduction of this aggregate and an investigation of the relation in which they stand to a particular class of reciprocants were made in a subsequent paper.† The present memoir is a continuation of the theory of functional invariants, the invariants herein considered being constituted by combinations of the differential coefficients of a function of more than one independent variable which are such that, when the independent variables are transformed, each combination is reproduced save as to a factor depending on the transformations to which the variables are subjected. The transformations, in the case of which any detailed results are given, are of the general homographic type; and the investigations are limited to invariantive derivatives of a function of two independent variables only, a limitation introduced partly for the sake of conciseness. The characteristic properties, such as the symmetry of the invariants and the forms of the simultaneous linear partial differential equations satisfied by them, can in the case of more than two independent variables be inferred from the properties actually given; but many of the deductions made are necessarily proper to functions of only two independent variables.

In the matter of notation it is convenient here to state that the independent variables are denoted by x and y, and the dependent variable by z. The general differential coefficient $\partial^{m+n}z/\partial x^m\, \partial y^n$ is represented by $z_{m,n}$; but frequently the following modifications for the notation of particular coefficients are made, viz.:

$$\begin{aligned}
&p,\ q \text{ replace } z_{10},\ z_{01}:\\
&r,\ s,\ t \text{ replace } z_{20},\ z_{11},\ z_{02}:\\
&a,\ b,\ c,\ d \ \ldots\ z_{30},\ z_{21},\ z_{12},\ z_{03}:\\
&e,\ f,\ g,\ h,\ i \ \ldots\ z_{40},\ z_{31},\ z_{22},\ z_{13},\ z_{04}:
\end{aligned}$$

* "Invariants, Covariants, and Quotient-Derivatives associated with Linear Differential Equations," 'Phil. Trans.,' A, 1888, pp. 377–489.

† "Homographic Invariants and Quotient-Derivatives," 'Mess. of Math.,' vol. 17 (1888), pp. 154–192.

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respectively. The transformed independent variables are denoted by X and Y; and quantities bearing to them the same relation as the foregoing bear to x and y are denoted by $Z_{m,n}$, P, Q, ... And in three different instances it has been necessary, for the sake of uniformity of notation for similar successions of quantities, to use different symbols for the same quantity occurring in different successions; these are $u_4 = A_0$ (§§ 3, 12), $u_8 = -A_1$ (§§ 12, 13), $u_{13} = A_2$ (§§ 16, 17).

The general results of the memoir may be stated as follows:—

Every invariant is explicitly free from the variables themselves, viz., the dependent and the two [m] independent variables; it is homogeneous in the differential coefficients of the dependent variable; it is of uniform grade in differentiations with regard to each of the dependent variables, and it is either symmetric or skew symmetric with regard to such differentiations.

It satisfies six [$m^2 + m$] linear partial differential equations, all of the first order, of which four [m^2] are characteristic equations and determine the form of the invariant, and the remaining two [m] are index equations and are identically satisfied when the form is known and the index is derived by inspection from the form.

Every invariant involves the two [m] differential coefficients of the first order.

The following results relative to irreducible invariants derived from a single dependent variable z are given:—The invariants can be ranged in sets, each set being proper * to a particular rank. There is no invariant proper to the rank 1; there is one proper to the rank 2; there are three invariants proper to the rank 3; and, for a value of n greater than 3, there are $n + 1$ invariants proper to the rank n, which can be chosen so as to be linear in the differential coefficients of order n. Every invariant can be expressed in terms of these irreducible invariants; and the expression involves invariants of rank no higher than the order of the highest differential coefficient which occurs in that invariant.

In the case of irreducible invariants, involving differential coefficients of two dependent variables, it is shown that there is a single one proper to the rank 1, and that there are four proper to the rank 2.

Some eductive operators are given; and in one case the educts are discussed so as to select those of the invariants thus obtained which are evidently reducible. Some general results analogous to reversor operations are derived.

Finally, it is shown how the theory of binary forms can be partly connected with the theory of functional invariants; for functional invariants are expressible in terms of the simultaneous concomitants of a certain set of quantities, viewed as binary quantics of successive orders in q and $-p$ as variables.

[Note added December 5, 1888.†—The invariants in the present memoir are distinct

* An invariant is said to be *proper* to the rank n when the highest differential coefficient of z occurring in it is of order n.

† This addition is due to a desire which has been expressed that some indication should be given of the difference between the functions considered in the present memoir and invariantive functions of

in character from the differential invariants of M. HALPHEN and the ternary reciprocants of Mr. ELLIOTT.

The earliest record of M. HALPHEN's investigations is his well-known thesis,* wherein he considers the invariance of a differential equation $f(x, y, y', y'', \ldots) = 0$, when the single independent variable x and the single dependent variable y are (p. 20, *loc. cit.*) subjected to the transformation

$$\frac{X}{ax + by + c} = \frac{Y}{a'x + b'y + c'} = \frac{1}{a''x + b''y + c''}.$$

The only reference in the thesis to the case of three variables is (p. 60) in the concluding paragraph, where it is said that the theory can be extended to the case of one dependent variable z and two dependent variables x and y, the transformation suggested, but not explicitly stated, being

$$\frac{X}{\alpha x + \beta y + \gamma z + \delta} = \frac{Y}{\alpha' x + \beta' y + \gamma' z + \delta'} = \frac{Z}{\alpha'' x + \beta'' y + \gamma'' z + \delta''} = \frac{1}{\alpha''' x + \beta''' y + \gamma''' z + \delta'''}.$$

M. HALPHEN, again,† considers differential invariants, in which the last transformation is effected on functions of the three variables; but in this investigation y and z are taken to be two dependent variables of the single independent variable x.

Mr. ELLIOTT's theory ‡ of ternary reciprocants is closely connected with the concluding paragraph of M. HALPHEN's thesis; the functions are invariantive for interchanges of z, x, y, where z is a variable dependent on x and y; and the pure reciprocants are invariantive for the above-suggested transformations.

The theory in this memoir deals almost entirely with the case of three variables, z, x, y, where z is a dependent variable, and x and y are independent variables. The transformations, through which the invariance is maintained, refer to the independent variables only; they are—

$$\frac{x}{\alpha_1 + \beta_1 X + \gamma_1 Y} = \frac{y}{\alpha_2 + \beta_2 X + \gamma_2 Y} = \frac{1}{\alpha_3 + \beta_3 X + \gamma_3 Y}.$$

The dependent variable is left untransformed; it does not enter into the equations of transformation.

It follows, from the difference between the transformation in the theory here

other classes, such as the differential invariants of M. HALPHEN and the ternary reciprocants of Mr. ELLIOTT.

* 'Sur les Invariants Différentiels,' Paris, 1878.

† "Sur les Invariants Différentiels des Courbes gauches," 'Journ. de l'École Polytechnique,' vol. 28, 1880, pp. 1-102.

‡ "On Ternary and n-ary Reciprocants," 'London Math. Soc. Proc.' vol. 17 (1886), pp. 171–196; "On the Linear Partial Differential Equations satisfied by pure Ternary Reciprocants," *ibid.*, vol. 18 (1887), pp. 142–164; "On pure Ternary Reciprocants and Functions allied to them," *ibid.*, vol. 19 (1888), pp. 6–23.

exposed in which the dependent variable does not enter into the equations of transformation, and the transformations above indicated in which the occurrence of the dependent variable in the equations of transformation is essential, that different results will be obtained. Two examples will suffice. First, a comparison of the characteristic equations of ELLIOTT's reciprocants and of those characteristic of the present functional invariants may be made from the forms expressed in the notations of this memoir :—

Annihilators of ELLIOTT's reciprocants.

$$\Omega_1 = r\frac{\partial}{\partial s} + 2s\frac{\partial}{\partial t} + a\frac{\partial}{\partial b} + 2b\frac{\partial}{\partial c} + 3c\frac{\partial}{\partial d} + \dots$$

$$\Omega_2 = t\frac{\partial}{\partial s} + 2s\frac{\partial}{\partial r} + d\frac{\partial}{\partial c} + 2c\frac{\partial}{\partial b} + 3c\frac{\partial}{\partial a} + \dots$$

$$-\mathrm{E}_1 = -\mu + 3r\frac{\partial}{\partial r} + 2s\frac{\partial}{\partial s} + t\frac{\partial}{\partial t} + 4a\frac{\partial}{\partial a} + 3b\frac{\partial}{\partial b} + 2c\frac{\partial}{\partial c} + d\frac{\partial}{\partial d} + \dots$$

$$-\mathrm{E}_2 = -\mu + r\frac{\partial}{\partial r} + 2s\frac{\partial}{\partial s} + 3t\frac{\partial}{\partial t} + a\frac{\partial}{\partial a} + 2b\frac{\partial}{\partial b} + 3c\frac{\partial}{\partial c} + 4d\frac{\partial}{\partial d} + \dots$$

$$\mathrm{V}_1 = 3r^2\frac{\partial}{\partial a} + 3rs\frac{\partial}{\partial b} + (rt + 2s^2)\frac{\partial}{\partial c} + 3st\frac{\partial}{\partial d} + \dots$$

$$\mathrm{V}_2 = 3rs\frac{\partial}{\partial a} + (rt + 2s^2)\frac{\partial}{\partial b} + 3st\frac{\partial}{\partial c} + 3t^2\frac{\partial}{\partial d} + \dots$$

Annihilators of Invariants in this memoir.

$$\Delta_4 = p\frac{\partial}{\partial q} + r\frac{\partial}{\partial s} + 2s\frac{\partial}{\partial t} + a\frac{\partial}{\partial b} + 2b\frac{\partial}{\partial c} + 3c\frac{\partial}{\partial d} + \dots$$

$$\Delta_3 = q\frac{\partial}{\partial p} + t\frac{\partial}{\partial s} + 2s\frac{\partial}{\partial r} + d\frac{\partial}{\partial c} + 2c\frac{\partial}{\partial b} + 3b\frac{\partial}{\partial a} + \dots$$

$$\Omega_0 = -3\lambda + 2p\frac{\partial}{\partial p} + q\frac{\partial}{\partial q} + 4r\frac{\partial}{\partial r} + 3s\frac{\partial}{\partial s} + 2t\frac{\partial}{\partial t} + 6a\frac{\partial}{\partial a} + 5b\frac{\partial}{\partial b} + 4c\frac{\partial}{\partial c} + 3d\frac{\partial}{\partial d} + \dots$$

$$\Omega_1 = -3\lambda + p\frac{\partial}{\partial p} + 2q\frac{\partial}{\partial q} + 2r\frac{\partial}{\partial r} + 3s\frac{\partial}{\partial s} + 4t\frac{\partial}{\partial t} + 3a\frac{\partial}{\partial a} + 4b\frac{\partial}{\partial b} + 5c\frac{\partial}{\partial c} + 6d\frac{\partial}{\partial d} + \dots$$

$$\Delta_1 = 1\left(q\frac{\partial}{\partial s} + 2p\frac{\partial}{\partial r}\right) + 2\left(t\frac{\partial}{\partial c} + 2s\frac{\partial}{\partial b} + 3r\frac{\partial}{\partial a}\right) + \dots$$

$$\Delta_2 = 1\left(p\frac{\partial}{\partial s} + 2q\frac{\partial}{\partial t}\right) + 2\left(r\frac{\partial}{\partial b} + 2s\frac{\partial}{\partial c} + 3t\frac{\partial}{\partial d}\right) + \dots$$

Second, as an inference from the equations $\Omega_1 = 0$, $\Omega_2 = 0$, in ELLIOTT's theory, it follows that all pure reciprocants are invariants of the binary quantics $(r, s, t \between \xi, \eta)^2$, $(a, b, c, d \between \xi, \eta)^3, \ldots$ —but all invariants are not reciprocants—and that there are no covariants among these reciprocants. From the equations $\Delta_4 = 0$, $\Delta_3 = 0$, in the present theory it follows that all the functional invariants are algebraical covariants of the binary quantics $(r, s, t \between q, -p)^2$, $(a, b, c, d \between q, -p)^3, \ldots$ —but not all algebraical covariants are functional invariants; and, from the other equations, that no algebraical invariants of these quantics are functional invariants. In particular, $rt - s^2$ is a reciprocant, but not a functional invariant; $q^2r - 2pqs + p^2t$ is a functional invariant, but not a reciprocant.]

Isolated Transformations.

1. We may briefly consider functions which are *invariantive for merely isolated changes of the independent variables*, that is, for changes which are effected by one relation between x and X only, and one relation between y and Y only. For such transformations we have

$$P = p\frac{dx}{dX}, \qquad Q = q\frac{dy}{dY}, \qquad S = s\frac{dx}{dX}\frac{dy}{dY},$$

so that $s \div pq$ is an absolute invariant. Again,

$$\frac{\partial}{\partial X} = \frac{dx}{dX}\frac{\partial}{\partial x}, \qquad \frac{\partial}{\partial Y} = \frac{dy}{dY}\frac{\partial}{\partial y},$$

so that $1/p\ \partial/\partial x$ and $1/q\ \partial/\partial y$ are absolute invariantive operators, which, when applied to absolute invariants, will produce absolute invariants. We therefore have the series

$$\frac{1}{p}\frac{\partial}{\partial x}\left(\frac{s}{pq}\right), \qquad \frac{1}{q}\frac{\partial}{\partial y}\left(\frac{s}{pq}\right);$$

$$\left(\frac{1}{p}\frac{\partial}{\partial x}\right)^2\frac{s}{pq}, \qquad \left(\frac{1}{p}\frac{\partial}{\partial x}\frac{1}{q}\frac{\partial}{\partial y}\right)\frac{s}{pq}, \qquad \left(\frac{1}{q}\frac{\partial}{\partial y}\frac{1}{p}\frac{\partial}{\partial x}\right)\frac{s}{pq}, \qquad \left(\frac{1}{q}\frac{\partial}{\partial y}\right)^2\frac{s}{pq},$$

and so on. The operators $1/p\ \partial/\partial x$ and $1/q\ \partial/\partial y$ may be applied, any number of times in any order, to the absolute invariant s/pq (or any other invariant which is absolute), and the result will be an absolute invariant.

These invariants possess their property for any general isolated transformations of x and y; but, if special isolated transformations are effected on x and on y, *e.g.*, the homographic transformations of the form

$$x = \frac{aX + b}{cX + d}, \qquad y = \frac{a'Y + b'}{c'Y + d'},$$

additional invariants will be introduced. For instance, we then have

$$\mathrm{A}(z) = \frac{z_{30}z_{10} - \frac{3}{2}z_{20}^2}{z_{10}^4}, \qquad \mathrm{B}(z) = \frac{z_{03}z_{01} - \frac{3}{2}z_{02}^2}{z_{01}^4},$$

both absolute invariants; in the former the variation of y, and in the latter the variation of x, do not come into consideration. From these we can derive a series of educts by the application of combinations of the absolute invariantive operators $1/p\ \partial/\partial x$ and $1/q\ \partial/\partial y$. When any such educt is invariantive, say

$$\mathrm{A}_m = \left(\frac{1}{p}\frac{\partial}{\partial x}\right)^m \mathrm{A}(z),$$

we may obtain from it other invariants by taking as the function, the differential coefficients of which are to enter, not z, but any educt which is an absolute invariant. All such invariants, however, thus obtained are expressible in terms of the educts obtained from $\mathrm{A}(z)$ and $\mathrm{B}(z)$ by repeated application of $1/p\ \partial/\partial x$ and $1/q\ \partial/\partial y$ in all possible combinations. Thus, it is easy to verify that if I be any absolute invariant, and I_1, I_2, I_3 its first, second, and third educts due to successive operations on I by $1/p\ \partial/\partial x$, the equation

$$\mathrm{A}(\mathrm{I}) = \frac{\mathrm{I}_1\mathrm{I}_3 - \frac{3}{2}\mathrm{I}_2^2}{\mathrm{I}_1^4} - \frac{\mathrm{A}(z)}{\mathrm{I}_1^2}$$

is satisfied; and the law is general.

2. Nor is it necessary to consider in any detail functions of the differential coefficients of z, which are *invariantive for isolated transformation of the dependent variable;* that is, for a transformation which connects z with a new variable ζ, without regard to the dependent variables. Such a transformation can be effected by means of an equation,

$$\phi(z, \zeta) = 0;$$

and we then have

$$z_{01} = \zeta_{01}\frac{dz}{d\zeta}, \qquad z_{10} = \zeta_{10}\frac{dz}{d\zeta}.$$

Since $dz/d\zeta$ is determinate from the transforming equation, it follows that

$$\frac{z_{01}}{z_{10}}$$

is an absolute invariant for the transformations at present under consideration. Moreover, in the present case $\partial/\partial x$ and $\partial/\partial y$ are absolute invariantive operators; and therefore

$$\frac{\partial^{m+n}}{\partial x^m\, \partial y^n}\left(\frac{z_{01}}{z_{10}}\right)$$

will, for all values of m and n, be an absolute invariant. Thus, taking in succession $m = 1$ and $n = 0$, and $m = 0$ and $n = 1$, we have

$$\frac{z_{10}z_{11} - z_{01}z_{20}}{z_{10}^2} \quad \text{and} \quad \frac{z_{10}z_{02} - z_{01}z_{11}}{z_{10}^2},$$

the former of which, multiplied by z_{01}/z_{10} and subtracted from the latter, gives

$$\frac{z_{01}^2 z_{20} - 2z_{01}z_{10}z_{11} + z_{10}^2 z_{02}}{z_{10}^3}$$

as an absolute invariant, or $z_{10}^2 z_{20} - 2z_{01} z_{10} z_{11} + z_{10}^2 z_{02}$ as a relative invariant, for the present transformation.

General Transformation.

3. We now proceed to the consideration of functions which are invariantive for the general simultaneous homographic transformation of the independent variables represented by

$$\frac{x}{\alpha_1 + \beta_1 X + \gamma_1 Y} = \frac{y}{\alpha_2 + \beta_2 X + \gamma_2 Y} = \frac{1}{\alpha_3 + \beta_3 X + \gamma_3 Y}.$$

As it will be convenient to have some one invariant at least, a relative invariant for these transformations can be obtained as follows. An integral relation given by

$$z = \frac{a + bx + cy}{a' + b'x + c'y} = \frac{u}{v}$$

reproduces itself in form when the independent variables are subjected to the above transformation ; and the differential equation which is the equivalent of this integral relation will, therefore, also reproduce itself, and so will furnish an invariant.

Now, both u and v satisfy the three equations

$$\frac{\partial^2}{\partial x^2} = 0, \qquad \frac{\partial^2}{\partial x \, \partial y} = 0, \qquad \frac{\partial^2}{\partial y^2} = 0 ;$$

and therefore, substituting vz as the value of u in these, we have

$$\begin{aligned} 0 &= z_{20}v + 2z_{10}v_{10}, \\ 0 &= z_{11}v + z_{10}v_{01} + z_{01}v_{10}, \\ 0 &= z_{02}v + 2z_{01}v_{01}. \end{aligned}$$

The elimination of v, v_{10}, v_{01} between these leads to the result

$$0 = \begin{vmatrix} z_{20}, & 2z_{10}, & 0 \\ z_{11}, & z_{01}, & z_{10} \\ z_{02}, & 0, & 2z_{01} \end{vmatrix} = 2\left(z_{01}{}^2 z_{20} - 2z_{10}z_{01}z_{11} + z_{10}{}^2 z_{02}\right);$$

and, therefore,

$$A_0 = z_{01}{}^2 z_{20} - 2z_{10}z_{01}z_{11} + z_{10}{}^2 z_{02} = (z_{20}, z_{11}, z_{02} \between z_{01}, -z_{10})^2$$

is an invariant.

The integral of a partial differential equation of the second order, which is most general so far as concerns the number of arbitrary constants, contains five such independent arbitrary constants; and, therefore, a general integral of

$$A_0 = 0$$

is

$$z = \frac{a + bx + cy}{a' + b'x + c'y}.$$

It has already appeared that A_0 is an invariant for arbitrary change of z; and therefore, an immediate corollary is that

$$z = \phi\left(\frac{a + bx + cy}{a' + b'x + c'y}\right),$$

where ϕ is arbitrary, is a general integral of the equation $A_0 = 0$.

4. As an invariant is self-reproductive after transformations have been effected, save as to a factor, it is necessary to obtain the form of this factor. For this purpose it will be sufficient to consider a simple case.

Let z_1 and z_2 be two functions, and suppose the transformations of the variables to be any whatever, say of the form

$$x = \phi(X, Y), \qquad y = \psi(X, Y).$$

Then we have

$$P_1 = p_1 \frac{\partial x}{\partial X} + q_1 \frac{\partial y}{\partial X}, \qquad P_2 = p_2 \frac{\partial x}{\partial X} + q_2 \frac{\partial y}{\partial X},$$

$$Q_1 = p_1 \frac{\partial x}{\partial Y} + q_1 \frac{\partial y}{\partial Y}, \qquad Q_2 = p_2 \frac{\partial x}{\partial Y} + q_2 \frac{\partial y}{\partial Y};$$

and therefore

$$\begin{vmatrix} P_1, & Q_1 \\ P_2, & Q_2 \end{vmatrix} = \begin{vmatrix} p_1, & q_1 \\ p_2, & q_2 \end{vmatrix} \frac{\partial(x, y)}{\partial(X, Y)}.$$

Hence, in the present case, the factor is $\partial(x, y)/\partial(X, Y) = J$; and, by the analogy of all invariants, the factor for any one will be some power of J.

The invariants at present under consideration may, therefore, be defined as follows:—

A function ϕ of the partial differential coefficients of z with regard to x and to y is called an invariant if, when the independent variables are changed to X and Y and the same function Φ of the new variables is formed, the equation

$$\Phi = J^m \phi$$

is satisfied, where

$$J = \frac{\partial(x, y)}{\partial(X, Y)}.$$

5. The following properties of *irreducible* invariants are easily obtained :—

(i.) An invariant does not contain the dependent variable, nor either of the independent variables.

(ii.) An invariant is homogeneous in the differential coefficients.

(iii.) An invariant is of uniform grade,* equal to its index m, in differentiation with regard to x; and of uniform grade, also equal to its index m, in differentiation with regard to y.

(iv.) An invariant is either symmetric or skew symmetric in differentiation with regard to the independent variables.

All these properties hold of A_0, the index of which is easily seen to be 2; it is a symmetric invariant, that is, it is unchanged if x and y be interchanged.

The index of a symmetric invariant is an even integer; the index of a skew symmetric invariant is an odd integer.

These properties hold for functions which are invariants for any general transformation, and not merely for the homographic transformations to be adopted; but the forms of possible functions, as well as the value of J, will be determined by the character of the transformation. And, in particular, for the homographic transformation it is easy to prove that

$$J = \begin{vmatrix} \alpha_1, & \alpha_2, & \alpha_3 \\ \beta_1, & \beta_2, & \beta_3 \\ \gamma_1, & \gamma_2, & \gamma_3 \end{vmatrix} (\alpha_3 + \beta_3 X + \gamma_3 Y)^{-3}.$$

6. The method adopted for the determination of the forms of invariants will be to obtain the partial differential equations satisfied by them; these equations can be obtained, as in a similar case,† by using the principle of complete infinitesimal variation. For this purpose it will be necessary to have the formulæ expressing the relations between differential coefficients of z when the variables are transformed. This relation is given in the following proposition, the transformations being supposed any whatever. The special application to the homographic transformation will afterwards be made.

* The grade of a term is the sum of the orders of differentiation with regard to one variable of the factors; thus, the x-grade of A_0 is 2; the y-grade is 2.

† "Homographic Invariants and Quotient-Derivatives," 'Mess. of Math.,' vol. 17 (1888), pp. 154-192.

Let $z = \theta(x, y)$, and let the variables be transformed by the equations

$$x = \phi(\mathrm{X}, \mathrm{Y}), \qquad y = \psi(\mathrm{X}, \mathrm{Y}).$$

Let

$$\Phi = \phi(\mathrm{X} + \rho, \ \mathrm{Y} + \sigma) - x,$$
$$\Psi = \psi(\mathrm{X} + \rho, \ \mathrm{Y} + \sigma) - y,$$

so that Φ and Ψ vanish with ρ and σ. Then, by the generalised form of TAYLOR's Theorem,

$$\frac{1}{m!\,n!}\frac{\partial^{m+n} z}{\partial \mathrm{X}^m\,\partial \mathrm{Y}^n}$$

is the coefficient of $\rho^m\sigma^n$ in the expansion in ascending powers of

$$\begin{aligned}&\theta\{\phi(\mathrm{X} + \rho, \ \mathrm{Y} + \sigma), \ \psi(\mathrm{X} + \rho, \ \mathrm{Y} + \sigma)\}\\ &= \theta(x + \Phi, \ y + \Psi),\end{aligned}$$

where ρ and σ occur only in Φ and Ψ. Now,

$$\theta(x + \Phi, \ y + \Psi) = \sum_{m'=0}\sum_{n'=0}\frac{1}{m'!\,n'!}\frac{\partial^{m'+n'} z}{\partial x^{m'}\,\partial y^{n'}}\Phi^{m'}\Psi^{n'};$$

and therefore

$$\frac{1}{m!\,n!}\frac{\partial^{m+n} z}{\partial \mathrm{X}^m\,\partial \mathrm{Y}^n} = \sum_{m'=0}\sum_{n'=0}\frac{1}{m'!\,n'!}\frac{\partial^{m'+n'} z}{\partial x^{m'}\,\partial y^{n'}}\mathrm{C}_{m,n}(\Phi^{m'}\Psi^{n'}),$$

where $\mathrm{C}_{m,n}(\Phi^{m'}\Psi^{n'})$ denotes the coefficient of $\rho^m\sigma^n$ in the expansion of $\Phi^{m'}\Psi^{n'}$ in ascending powers of ρ and σ. When m' and n' both vanish, or when $m' + n' > m + n$, the coefficient $\mathrm{C}_{m,n}(\Phi^{m'}\Psi^{n'})$ is zero.

The form of the corresponding theorem for the case of any number of independent variables is evident.

Homographic Transformation: Characteristic Equations.

7. When we consider the general homographic transformation, we may take α_1 and α_2 to be zero, for the invariants do not explicitly contain x and y, but only differential coefficients with regard to them, and so they may be modified by the subtraction of the respective constants α_1/α_3, α_2/α_3; and then the general forms are equivalent to

$$\frac{x}{\mathrm{X} + \alpha\mathrm{Y}} = \frac{y}{\mathrm{Y} + \beta\mathrm{X}} = \frac{1}{\alpha_3 + \beta_3\mathrm{X} + \gamma_3\mathrm{Y}}.$$

In order to apply the method of infinitesimal variation, it is sufficient to make the factor J nearly equal to unity, or, what is the same thing, to make x nearly equal to

X and y nearly equal to Y. Hence, we take α_3 to be unity, and β_3 and γ_3 small, say, $-\epsilon$ and $-\theta$ respectively; and α and β are to be considered small, quantities of the first order being retained. Thus, we have

$$\mathrm{J} = \begin{vmatrix} 0, & 0, & 1 \\ 1, & \beta, & -\epsilon \\ \alpha, & 1, & -\theta \end{vmatrix} (1 - \epsilon\mathrm{X} - \theta\mathrm{Y})^{-3},$$
$$= 1 + 3\epsilon\mathrm{X} + 3\theta\mathrm{Y},$$

so far as quantities of the first order. Also

$$x = \frac{\mathrm{X} + \alpha\mathrm{Y}}{1 - \epsilon\mathrm{X} - \theta\mathrm{Y}} = \mathrm{X} + \alpha\mathrm{Y} + \epsilon\mathrm{X}^2 + \theta\mathrm{XY} = \phi(\mathrm{X}, \mathrm{Y}),$$
$$y = \frac{\mathrm{Y} + \beta\mathrm{X}}{1 - \epsilon\mathrm{X} - \theta\mathrm{Y}} = \beta\mathrm{X} + \mathrm{Y} + \epsilon\mathrm{XY} + \theta\mathrm{Y}^2 = \psi(\mathrm{X}, \mathrm{Y}),$$

to the same order; and therefore

$$\begin{aligned} \Phi &= \phi(\mathrm{X} + \rho, \mathrm{Y} + \sigma) - \phi(\mathrm{X}, \mathrm{Y}) \\ &= \rho + \alpha\sigma + \epsilon(\rho^2 + 2\mathrm{X}\rho) + \theta(\rho\sigma + \rho\mathrm{Y} + \sigma\mathrm{X}), \\ \Psi &= \psi(\mathrm{X} + \rho, \mathrm{Y} + \sigma) - \psi(\mathrm{X}, \mathrm{Y}) \\ &= \sigma + \beta\rho + \epsilon(\rho\sigma + \rho\mathrm{Y} + \sigma\mathrm{X}) + \theta(\sigma^2 + 2\mathrm{Y}\sigma). \end{aligned}$$

Hence, to the first order inclusive, we have

$$\begin{aligned} \Phi^{m'}\Psi^{n'} = \rho^{m'}\sigma^{n'} &+ m'\rho^{m'-1}\sigma^{n'}\{\alpha\sigma + \epsilon(\rho^2 + 2\mathrm{X}\rho) + \theta(\rho\sigma + \rho\mathrm{Y} + \sigma\mathrm{X})\} \\ &+ n'\rho^{m'}\sigma^{n'-1}\{\beta\rho + \epsilon(\rho\sigma + \rho\mathrm{Y} + \sigma\mathrm{X}) + \theta(\sigma^3 + 2\mathrm{Y}\sigma)\}, \end{aligned}$$

and therefore

$$\mathrm{C}_{m', n'} = 1 + m'(2\epsilon\mathrm{X} + \theta\mathrm{Y}) + n'(\epsilon\mathrm{X} + 2\theta\mathrm{Y}),$$
$$\mathrm{C}_{m'-1, n'+1} = m'(\alpha + \theta\mathrm{X}), \quad \mathrm{C}_{m'+1, n'-1} = n'(\beta + \epsilon\mathrm{Y}),$$
$$\mathrm{C}_{m'+1, n'} = (m' + n')\epsilon, \qquad \mathrm{C}_{m', n'+1} = (m' + n')\theta.$$

All other coefficients are negligible, being of a higher order of small quantities or zero (non-occurring); and these give all the combinations of values of m and n for $\mathrm{C}_{m, n}(\Phi^{m'}\Psi^{n'})$. Therefore, for all values of m and n, we have

$$\begin{aligned} \frac{\partial^{m+n} z}{\partial \mathrm{X}^m \partial \mathrm{Y}^n} = \frac{\partial^{m+n} z}{\partial x^m \partial y^n}&\{1 + m(2\epsilon\mathrm{X} + \theta\mathrm{Y}) + n(\epsilon\mathrm{X} + 2\theta\mathrm{Y})\} \\ &+ \frac{\partial^{m+n} z}{\partial x^{m+1} \partial y^{n-1}} n(\alpha + \theta\mathrm{X}) + \frac{\partial^{m+n} z}{\partial x^{m-1} \partial y^{n+1}} n(\beta + \epsilon\mathrm{Y}) \\ &+ \frac{\partial^{m+n-1} z}{\partial x^{m-1} \partial y^n} m(m + n - 1)\epsilon + \frac{\partial^{m+n-1} z}{\partial x^m \partial y^{n-1}} n(m + n - 1)\theta. \end{aligned}$$

8. If, then, we have an invariant f of index λ, such that

$$F(\ldots, Z_{m,n}, \ldots) = J^{\lambda} f(\ldots, z_{m,n}, \ldots),$$

and we substitute for J and for all differential coefficients $Z_{m,n}$, and then expand, retaining all small quantities of the first order, we have the following equations derived from a comparison of corresponding terms.

From the terms which are multiplied by ϵX

$$\Sigma\Sigma\,(2m+n)\,z_{m,n}\frac{\partial f}{\partial z_{m,n}} = 3\lambda f \quad \text{(i.)};$$

from the terms in θY

$$\Sigma\Sigma\,(2n+m)\,z_{m,n}\frac{\partial f}{\partial z_{m,n}} = 3\lambda f \quad \text{(ii.)};$$

from the terms in ϵ

$$\Delta_1 f = \Sigma\Sigma m\,(m+n-1)\,z_{m-1,n}\frac{\partial f}{\partial z_{m,n}} = 0 \quad \text{(iii.)};$$

from the terms in θ

$$\Delta_2 f = \Sigma\Sigma n\,(m+n-1)\,z_{m,n-1}\frac{\partial f}{\partial z_{m,n}} = 0 \quad \text{(iv.)};$$

from the terms in $\beta + \epsilon Y$

$$\Delta_3 f = \Sigma\Sigma m z_{m-1,n+1}\frac{\partial f}{\partial z_{m,n}} = 0 \quad \text{(v.)};$$

and from the terms in $\alpha + \theta X$

$$\Delta_4 f = \Sigma\Sigma n z_{m+1,n-1}\frac{\partial f}{\partial z_{m,n}} = 0 \quad \text{(vi.)}.$$

Equations (iii.)–(vi.) determine the form of the function f; when the form is obtained, the index is derivable by inspection, and equations (i.) and (ii.) are then identically satisfied.

9. Before considering these equations, characteristic of the invariants, one remark should be made. If the quantities ϵ and θ are absolutely zero so that the transformations are

$$x = X + \alpha Y, \qquad y = \beta X + Y,$$

that is, transformations to which a binary form is subject, the terms which, in what precedes, give rise to equations (i.)–(iv.) do not exist, and, therefore, these equations do not exist; but there are terms in β and α, and, therefore, equations (v.) and (vi.) survive, being in fact the partial differential equations determining those covariants which can be expressed in terms of partial differential coefficients of the form with regard to the variables.

Invariants in the Second Order.

10. First, let us consider invariants which involve no partial differential coefficients of order higher than the second. The differential equations to be satisfied are, in the non-subscript notation,

$$\text{(iii.)} \quad q\frac{\partial f}{\partial s} + 2p\frac{\partial f}{\partial r} = 0,$$

$$\text{(iv.)} \quad p\frac{\partial f}{\partial s} + 2q\frac{\partial f}{\partial t} = 0,$$

$$\text{(v.)} \quad q\frac{\partial f}{\partial p} + 2s\frac{\partial f}{\partial r} + t\frac{\partial f}{\partial s} = 0,$$

$$\text{(vi.)} \quad p\frac{\partial f}{\partial q} + 2s\frac{\partial f}{\partial t} + r\frac{\partial f}{\partial s} = 0,$$

so far as concerns the form of the function. From these equations we have

$$\frac{\dfrac{\partial f}{\partial r}}{q^2} = \frac{\dfrac{\partial f}{\partial s}}{-2pq} = \frac{\dfrac{\partial f}{\partial t}}{p^2} = \frac{\dfrac{\partial f}{\partial p}}{2(pt - qs)} = \frac{\dfrac{\partial f}{\partial q}}{2(qr - sp)}.$$

When Θ is taken to be the common value of these fractions, it follows that

$$\begin{aligned} df &= \frac{\partial f}{\partial r}dr + \frac{\partial f}{\partial s}ds + \frac{\partial f}{\partial t}dt + \frac{\partial f}{\partial p}dp + \frac{\partial f}{\partial q}dq \\ &= \Theta\, d\,(q^2r - 2pqs + p^2t). \end{aligned}$$

Now, df is a perfect differential, and therefore Θ is some function of $q^2r - 2pqs + p^2t$; hence, f also is some function of $q^2r - 2pqs + p^2t$, and therefore the *only irreducible invariant which contains differential coefficients of order not higher than the second is*

$$q^2r - 2pqs + p^2t.$$

This is the function A_0, already (§ 3) considered; the integral equation corresponding to the vanishing of this invariant is known.

Invariants in the Third Order.

11. When we come to consider invariants which involve differential coefficients of higher order, the method just used is no longer available, because the four differential equations are not sufficient to determine the ratios of the differential coefficients which

enter. If the determination of the invariants be made from the point of view that they are simultaneous solutions of the four equations, one method of proceeding will be to adopt JACOBI's process.

Any function f, which satisfies (iii.)–(vi.), must also satisfy each of the equations

$$[\Delta_r, \Delta_s] = 0$$

for $r, s = 1, 2, 3, 4$. Forming these, it is easy to show that

$$\begin{aligned}[\Delta_4, \Delta_3] &= \Sigma\,\Sigma\left\{\frac{\partial(\Delta_4 f)}{\partial\left(\dfrac{\partial f}{\partial z_{m,n}}\right)}\frac{\partial(\Delta_3 f)}{\partial z_{m,n}} - \frac{\partial(\Delta_4 f)}{\partial z_{m,n}}\frac{\partial(\Delta_3 f)}{\partial\left(\dfrac{\partial f}{\partial z_{m,n}}\right)}\right\} \\ &= \Sigma\Sigma\,(m-n)\,z_{m,n}\frac{\partial f}{\partial z_{m,n}}\end{aligned}$$

after substitution and collection. Since this does not vanish in virtue of any one of the given equations, we must have a new equation

$$\Delta_5 f = \Sigma\Sigma\,(m-n)\,z_{m,n}\frac{\partial f}{\partial z_{m,n}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(vii.)},$$

to be associated with the rest. But this is the only additional equation; for

$$\begin{aligned}&[\Delta_4, \Delta_2] = 0\,; \quad [\Delta_4, \Delta_1] = -\Delta_2 f = 0\,; \quad [\Delta_3, \Delta_2] = -\Delta_1 f = 0\,; \\ &[\Delta_3, \Delta_1] = 0\,; \quad [\Delta_2, \Delta_1] = 0\,; \quad [\Delta_5, \Delta_4] = 2\Delta_4 f = 0\,; \\ &[\Delta_5, \Delta_3] = -2\Delta_3 f = 0\,; \quad [\Delta_5, \Delta_2] = 0\,; \quad [\Delta_5, \Delta_1] = 0.\end{aligned}$$

It is easy to verify that $f = A_0$ satisfies (vii.). Every invariant will be a simultaneous solution of (iii.)–(vii.).

It may be noticed that the equation (vii.) can be otherwise obtained; it arises by equating the left-hand sides of equations (i.) and (ii.) to one another, for, on re-arrangement of this, we have

$$\Sigma\Sigma\,(m-n)\,z_{m,n}\frac{\partial f}{\partial z_{m,n}} = 0.$$

Hence, equation (vii.) may be considered as replacing either (i.) or (ii.); and any function, which is a simultaneous solution of (iii.)–(vi.) and for the same value of λ satisfies (i.) and (ii.), will also satisfy (vii.).

One deduction as to the character of the invariants can at once be made from the form of the equations.

Every Irreducible Invariant must involve z_{01} *and* z_{10}.

For every irreducible invariant f satisfies the five equations ; if then it be independent of z_{01}, we have $\partial f/\partial z_{01} = 0$. Since there is no term in it which involves z_{01}, and since there is a single term involving z_{01} in $\Delta_3 f = 0$, viz., $z_{01}(\partial f/\partial z_{10})$, we must therefore have $\partial f/\partial z_{10}$, *i.e.*, the function must be independent of z_{10}. From $\Delta_1 f = 0$, it then follows that $\partial f/\partial z_{20}$ and $\partial f/\partial z_{11}$ both vanish; from $\Delta_2 f = 0$, it then follows that $\partial f/\partial z_{11}$ and $\partial f/\partial z_{02}$ both vanish, and, therefore, that f involves no differential coefficients of the second order. Proceeding in this way to the successive orders, it appears that f involves no differential coefficients whatever ; so that it cannot be an invariant, other than a constant or z.

12. Proceeding now to the consideration of invariants which involve differential coefficients of the third order as the highest, and denoting them for convenience by a, b, c, d ($= z_{30}, z_{12}, z_{21}, z_{30}$, respectively), we have, as the subsidiary equations necessary for the construction of the general solution of $\Delta_3 f = 0$, the set

$$\frac{dp}{q} = \frac{dq}{0} = \frac{dr}{2s} = \frac{ds}{t} = \frac{dt}{0} = \frac{da}{3b} = \frac{db}{2c} = \frac{dc}{d} = \frac{dd}{0}.$$

To deduce that general solution, eight independent integrals of the subsidiary set must be obtained ; bearing in mind the character of the invariants (§ 11) ultimately to be arrived at, we take these integrals in the form

$$\begin{aligned}
u_1 &= q, \\
u_2 &= t, \\
u_3 &= qs - pt, \\
u_4 &= q^2 r - 2pqs + p^2 t, \\
u_5 &= d, \\
u_6 &= pd - qc, \\
u_7 &= p^2 d - 2pqc + q^2 b, \\
u_8 &= p^3 d - 3p^2 qc + 3pq^2 b - q^3 a.
\end{aligned}$$

Any solution of the equation $\Delta_3 f = 0$ can be expressed as a functional combination of $u_1, u_2, \ldots, u_8$; thus

$$rt - s^2 = \frac{u_2 u_4 - u_3^2}{u_1^2},$$

$$bd - c^2 = \frac{u_5 u_7 - u_6^2}{u_1^2},$$

$$(ad - bc)^2 - 4(ac - b^2)(bd - c^2) = \frac{(u_5 u_8 - u_6 u_7)^2 - 4(u_5 u_7 - u_6^2)(u_6 u_8 - u_7^2)}{u_1^6},$$

and so on.

In order to obtain the most general solution which simultaneously satisfies $\Delta_3 f = 0$ and $\Delta_1 f = 0$, it will be sufficient to obtain the irreducible functional combinations of $u_1, u_2, \ldots, u_8$, which satisfy $\Delta_1 f = 0$. Now,

$$\Delta_1 u_1 = 0, \qquad \Delta_1 u_2 = 0, \qquad \Delta_1 u_4 = 0, \qquad \Delta_1 u_5 = 0\,;$$

and

$$\begin{aligned} \Delta_1 u_3 &= u_1^2, \\ \Delta_1 u_6 &= -\,2u_1u_2, \\ \Delta_1 u_7 &= 4u_1u_3, \\ \Delta_1 u_8 &= -\,6u_1u_4\,; \end{aligned}$$

so that

$$\begin{aligned} \Delta_1\,(u_1u_6 + 2u_2u_3) &= 0, \\ \Delta_1\,(u_1u_7 - 2u_3^2) &= 0, \\ \Delta_1\,(u_1u_8 + 6u_3u_4) &= 0. \end{aligned}$$

Hence, the most general simultaneous solution of $\Delta_3 f = 0$, and $\Delta_1 f = 0$, can be expressed as a functional combination of

$$\begin{aligned} &u_1,\ u_2,\ u_4,\ u_5, \\ &v_6 = u_1u_6 + 2u_2u_3, \\ &v_7 = u_1u_7 - 2u_3^2, \\ &v_8 = u_1u_8 + 6u_3u_4. \end{aligned}$$

In order to obtain the most general simultaneous solution of $\Delta_3 f = 0$, $\Delta_1 f = 0$, $\Delta_2 f = 0$, it will be sufficient to obtain the irreducible functional combinations of $u_1, u_2, u_4, u_5, v_6, v_7, v_8$ which satisfy $\Delta_2 f = 0$. Now, it is easy to show that

$$\Delta_2 u_1 = 0, \qquad \Delta_2 u_4 = 0, \qquad \Delta_2 v_6 = 0, \qquad \Delta_2 v_8 = 0\,;$$

and

$$\begin{aligned} \Delta_2 u_2 &= 2u_1, \\ \Delta_2 u_5 &= 6u_2, \\ \Delta_2 v_7 &= 2u_1u_4\,; \end{aligned}$$

so that

$$\begin{aligned} \Delta_2\,(u_1u_5 - \tfrac{2}{3}\,u_2^2) &= 0, \\ \Delta_2\,(v_7 - u_2u_4) &= 0. \end{aligned}$$

Hence, the most general simultaneous solution of $\Delta_3 f = 0$, $\Delta_1 f = 0$, $\Delta_2 f = 0$ can be expressed as a functional combination of

$$\begin{aligned} &u_1, \qquad u_4, \qquad v_6, \qquad v_8, \\ &v_5 = u_1u_5 - \tfrac{3}{2}u_2^2, \\ &w_7 = v_7 - u_2u_4 = u_1u_7 - u_2u_4 - 2u_3^2. \end{aligned}$$

In order to obtain the most general simultaneous solution of $\Delta_3 f = 0$, $\Delta_1 f = 0$, $\Delta_2 f = 0$, $\Delta_4 f = 0$, it will be sufficient to obtain the irreducible functional combinations of u_1, u_4, v_5, v_6, w_7, v_8 which satisfy $\Delta_4 f = 0$. Now it is easy enough to show that

$$\Delta_4 u_4 = 0\,;$$

and that

$$\begin{aligned}
\Delta_4 u_1 &= p,\\
u_1 \Delta_4 v_5 &= 4pv_5 - 3v_6,\\
u_1 \Delta_4 v_6 &= 3pv_6 - 2w_7,\\
u_1 \Delta_4 w_7 &= 2pw_7 - v_8,\\
u_1 \Delta_4 v_8 &= pv_8 + 6u_4^2.
\end{aligned}$$

If, then, we write

$$\frac{v_8}{u_1} = \mathrm{P}_8, \qquad \frac{w_7}{u_1^2} = \mathrm{P}_7, \qquad \frac{v_6}{u_1^3} = \mathrm{P}_6, \qquad \frac{v_5}{u_1^4} = \mathrm{P}_5,$$

these equations become

$$\begin{aligned}
u_1^2 \Delta_4 \mathrm{P}_8 &= 6u_4^2,\\
u_1^2 \Delta_4 \mathrm{P}_7 &= -\mathrm{P}_8,\\
u_1^2 \Delta_4 \mathrm{P}_6 &= -2\mathrm{P}_7,\\
u_1^2 \Delta_4 \mathrm{P}_5 &= -3\mathrm{P}_6\,;
\end{aligned}$$

and therefore, bearing in mind that $\Delta_4 u_4 = 0$, we have

$$\begin{aligned}
&\Delta_4 (\mathrm{P}_8^2 + 12u_4^2 \mathrm{P}_7) = 0,\\
&\Delta_4 (\mathrm{P}_8^3 + 18u_4^2 \mathrm{P}_7 \mathrm{P}_8 + 54u_4^4 \mathrm{P}_6) = 0,\\
&\Delta_4 (\mathrm{P}_8 \mathrm{P}_6 - \mathrm{P}_7^2 + 2u_4^2 \mathrm{P}_5) = 0.
\end{aligned}$$

Hence the most general simultaneous solution of $\Delta_1 f = 0$, $\Delta_2 f = 0$, $\Delta_3 f = 0$, $\Delta_4 f = 0$ can be expressed as a functional combination of u_4, and

$$\begin{aligned}
\mathrm{Q}_7 &= \mathrm{P}_8^2 + 12u_4^2 \mathrm{P}_7,\\
\mathrm{Q}_6 &= \mathrm{P}_8^3 + 18u_4^2 \mathrm{P}_7 \mathrm{P}_8 + 54u_4^4 \mathrm{P}_6,\\
\mathrm{Q}_5 &= \mathrm{P}_8 \mathrm{P}_6 - \mathrm{P}_7^2 + 2u_4^2 \mathrm{P}_5.
\end{aligned}$$

13. Before considering the question as to whether these functions satisfy (i.) and (ii.), and, therefore, also (vii.), it is desirable to modify their expressions.

We have already had the quantity u_4; it is the same as A_0, so that we write

$$u_4 = \mathrm{A}_0\,;$$

and it will be convenient to write

$$-u_8 = \mathrm{A}_1 = (z_{30}, z_{21}, z_{12}, z_{03} \between z_{01}, -z_{10})^3.$$

Then we have

$$Q_7 = \frac{1}{u_1^2}(v_8^2 + 12u_4^2 w_7)$$

$$= \frac{1}{u_1^2}\{u_1^2 u_8^2 + 12u_1 u_4 (u_3 u_8 + u_4 u_7) + 12u_4^2 (u_3^2 - u_2 u_4)\}.$$

But

$$\begin{aligned} u_3 u_8 + u_4 u_7 &= (qs - pt)(p^3 d - 3p^2 qc + 3pq^2 b - q^3 a) \\ &\qquad + \{q(qr - ps) - p(qs - pt)\}(p^2 d - 2pqc + q^2 b) \\ &= q(qr - ps)(p^2 d - 2pqc + q^2 b) + q(qs - pt)(-p^2 c + 2pqb - q^2 a) \\ &= \tfrac{1}{6} q \left(\frac{\partial A_0}{\partial p}\frac{\partial A_1}{\partial q} - \frac{\partial A_0}{\partial q}\frac{\partial A_1}{\partial p}\right) = \tfrac{1}{6} u_1 J_{01}, \end{aligned}$$

where J_{01} denotes the Jacobian of A_0, A_1 with regard to u_{10} and u_{01}. Similarly,

$$u_3^2 - u_2 u_4 = u_1^2 (s^2 - rt) = -u_1^2 H_0;$$

so that

$$Q_7 = A_1^2 + 2A_0 J_{01} + 12A_0^2 H_0,$$

H_0 being the discriminant of A_0.

For the modification of the expression of Q_6 we have, on substituting for the quantities P in terms of the quantities u,

$$Q_6 = u_8^3 + 18\frac{u_4 u_8}{u_1}(u_3 u_8 + u_4 u_7) + 18\frac{u_4^2}{u_1^2}(4u_3^2 u_8 - u_2 u_4 u_8 + 6u_3 u_4 u_7 + 3u_4^2 u_6).$$

The modification of the second term has already been given; for the third we have

$$\begin{aligned} 4u_3^2 u_8 - 4u_2 u_4 u_8 &= 4u_1^2 A_1 H_0; \\ u_2 u_8 + 2u_3 u_7 + u_4 u_6 &= u_{12}\{r(pd - qc) - 2s(pc - qb) + t(pb - qa)\}; \\ &= \tfrac{1}{12} u_1^2 \left\{\frac{\partial^2 A_0}{\partial q^2}\frac{\partial^2 A_1}{\partial p^2} - 2\frac{\partial^2 A_0}{\partial p\,\partial q}\frac{\partial^2 A_1}{\partial p\,\partial q} + \frac{\partial^2 A_0}{\partial p^2}\frac{\partial^2 A_1}{\partial q^2}\right\}; \\ &= -\tfrac{1}{12} u_1^2 H_{01}, \end{aligned}$$

where H_{01} denotes the simultaneous Hessian of A_0 and A_1 with regard to u_{01} and u_{10}. Hence

$$Q_6 = -A_1^3 - 3A_0 A_1 J_{01} + 72\,A_0^2 A_1 H_0 - \tfrac{9}{2}\,A_0^3 H_{01}.$$

For Q_5 we have, after substitution for P_5, P_6, P_7, P_8, the form

$$\begin{aligned} Q_5 &= \frac{1}{u_1^2}(u_6 u_8 - u_7^2) \\ &+ \frac{2}{u_1^3}(u_2 u_3 u_8 + u_4^2 u_5 + 3u_3 u_4 u_6 + u_2 u_4 u_7 + 2u_3^2 u_7) \\ &- \frac{1}{u_1^4}(4u_2^2 u_4^2 - 8u_2 u_4 u_3^2 + 4u_3^4). \end{aligned}$$

Now, for the first set of terms

$$\begin{aligned} u_6 u_8 - u_7^2 &= q^2 \{q^2 (ac - b^2) - pq (ad - bc) + p^2 (bd - c^2)\} \\ &= u_1^2 \mathrm{H}_1 \end{aligned}$$

where H_1 is the Hessian of A_1 considered as a ground-form in q and $-p$; and the third set of terms is

$$-\frac{4}{u_1^4}(u_2 u_4 - u_3^2)^2 = -4\mathrm{H}_0^2.$$

For the middle set of terms it is easily found, by the results already proved, that the terms within the bracket can be expressed in the form

$$\begin{aligned} &u_1 [q \{r^2 d - 3rsc + (2s^2 + rt) b - sta\} + p \{t^2 a - 3tsb + (2s^2 + rt) c - rsd\}] \\ &= u_1^3 \mathrm{L}_2, \end{aligned}$$

say; so that we have

$$\mathrm{Q}_5 = \mathrm{H}_1 + 2\mathrm{L}_2 - 4\mathrm{H}_0^2.$$

14. Considering now the question as to whether each of the functions thus obtained will satisfy (i.) and (ii.), for one and the same numerical value of the index λ probably associated with it, we may proceed as follows. Writing the equations in the form

$$\Omega_0 f = 3\lambda f \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{i}'.),$$

$$\Omega_1 f = 3\lambda f \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{ii}'.),$$

we have the following result :—

$f =$	$\Omega_0 f =$	$\Omega_1 f =$	$\lambda =$
A_0	$6\mathrm{A}_0$	$6\mathrm{A}_0$	2
A_1	$9\mathrm{A}_1$	$9\mathrm{A}_1$	3
J_{01}	$12\mathrm{J}_{01}$	$12\mathrm{J}_{01}$	4
L_2	$12\mathrm{L}_2$	$12\mathrm{L}_2$	4
H_{01}	$9\mathrm{H}_{01}$	$9\mathrm{H}_{01}$	3
H_0	$6\mathrm{H}_0$	$6\mathrm{H}_0$	2
H_1	$12\mathrm{H}_1$	$12\mathrm{H}_1$	4

By means of these results we at once find

$$\begin{aligned} \Omega_0 A_0 &= \ \ 6A_0 = \Omega_1 A_0 ; \\ \Omega_0 Q_7 &= 18 Q_7 = \Omega_1 Q_7 ; \\ \Omega_0 Q_6 &= 27 Q_6 = \Omega_1 Q_6 ; \\ \Omega_0 Q_5 &= 12 Q_5 = \Omega_1 Q_5 . \end{aligned}$$

Hence A_0, Q_5, Q_6, Q_7 satisfy all the necessary equations, and they are therefore invariants; their respective indices are 2, 4, 9, 6; and, therefore, *every invariant which involves differential coefficients of z of order not higher than* 3 *can be expressed as an algebraical function of* A_0, Q_5, Q_6, Q_7, *where* (changing the sign of Q_6 from § 13)

$$\begin{aligned} Q_5 &= H_1 + 2L_2 - 4H_0^2, \\ Q_6 &= A_1^3 + 3A_0A_1J_{01} - 72A_0^2A_1H_0 + \tfrac{9}{2}A_0^3H_{01}, \\ Q_7 &= A_1^2 + 2A_0J_{01} + 12A_0^2H_0, \end{aligned}$$

and the quantities A_0, A_1, J_{01}, H_0, H_1, H_{01}, L_2 *are given by the equations*

$$\begin{aligned} A_0 &= (z_{20}, z_{11}, z_{02} \between z_{01}, -z_{10})^2 ; \\ A_1 &= (z_{30}, z_{21}, z_{12}, z_{03} \between z_{01}, -z_{10})^3 ; \\ J_{01} &= \frac{\partial A_0}{\partial z_{10}} \frac{\partial A_1}{\partial z_{01}} - \frac{\partial A_0}{\partial z_{01}} \frac{\partial A_1}{\partial z_{10}} ; \\ H_0 &= z_{20}z_{02} - z_{11}^2 ; \\ H_{01} &= 12 \{ z_{01} (z_{20}z_{12} - 2z_{11}z_{21} + z_{02}z_{30}) - z_{10} (z_{20}z_{03} - 2z_{11}z_{12} + z_{02}z_{21}) \} \\ &= \frac{\partial^2 A_0}{\partial z_{10}^2} \frac{\partial^2 A_1}{\partial z_{01}^2} - 2 \frac{\partial^2 A_0}{\partial z_{10}\, \partial z_{01}} \frac{\partial^2 A_1}{\partial z_{10}\, \partial z_{01}} + \frac{\partial^2 A_0}{\partial z_{01}^2} \frac{\partial_z A_1}{\partial z_{10}^2} ; \\ H_1 &= (z_{30}z_{12} - z_{21}^2, z_{30}z_{03} - z_{21}z_{12}, z_{21}z_{03} - z_{12}^2 \between z_{01}, -z_{10})^2 ; \\ L_2 &= z_{01} \{ z_{20}^2 z_{03} - 3z_{20}z_{11}z_{12} + (2z_{11}^2 + z_{20}z_{02}) z_{21} - z_{11}z_{02}z_{30} \} \\ &\qquad - z_{10} \{ z_{20}z_{11}z_{03} - (2z_{11}^2 + z_{20}z_{02}) z_{12} + 3z_{11}z_{02}z_{21} - z_{02}^2 z_{30} \} \\ &= \tfrac{1}{24} \left(\frac{\partial A_0}{\partial z_{10}} \frac{\partial H_{01}}{\partial z_{01}} - \frac{\partial A_0}{\partial z_{01}} \frac{\partial H_{01}}{\partial z_{10}} \right) . \end{aligned}$$

The invariant A_0 is that which was obtained before, and it may be called the irreducible invariant of the second order; the invariants Q_5, Q_6, Q_7 may be called the irreducible invariants of the third order.

15. It may be remarked that the quantities additional to A_1 and A_0 which are necessary for the expression of Q_5, Q_6, Q_7 all belong to the simultaneous concomitant

system of A_0 and A_1 regarded as binary ground-forms in z_{01}, $-z_{10}$ as variables.* To this we shall return (§ 34).

A Special Series of Invariants.

16. There is a succession of invariants of consecutive orders, comparatively simple in form, which can be derived by using the remark made in § 9. A set of invariants of the form suggested by the covariants of a binary quantic, which involve only differential coefficients of the quantic with respect to the variables, is derivable by considering the functions in z analogous to HERMITE's "associated covariants" which may be taken to be

$$A_{m-2} = (z_{m,0},\ z_{m-1,1},\ z_{m-2,2}, \dots\dots,\ z_{2,m-2},\ z_{1,m-1},\ z_{0,m}\between z_{01}, -z_{10})^m$$

for values 2, 3, 4, . . . of m.

It is easy to see that each of these functions satisfies the equations (v.) and (vi.), viz., $\Delta_3 f = 0$ and $\Delta_4 f = 0$; these, in fact, are the equations which suggest the functions.

But, when we consider the operators Δ_1 and Δ_2 which do not arise in connexion with covariants of binary forms, we have

$$\begin{aligned}
&\Delta_1 A_{m-2} \\
&= (m-1)\left(z_{0,m-1}\frac{\partial}{\partial z_{1,m-1}} + 2z_{1,m-2}\frac{\partial}{\partial z_{2,m-2}} + 3z_{2,m-3}\frac{\partial}{\partial z_{3,m-3}} + \dots + mz_{m-1,0}\frac{\partial}{\partial z_{m,0}}\right) A_{m-2} \\
&= (m-1)\left\{mz_{m-1,0}z_{01}{}^m - (m-1)z_{m-2,1}\,mz_{01}{}^{m-1}z_{10} + \dots\right\} \\
&= m(m-1)\,z_{10}A_{m-3};
\end{aligned}$$

and, similarly,

$$\Delta_2 A_{m-2} = -m(m-1)\,z_{10}A_{m-3}.$$

Again, in regard to the operators which occur in (i.) and (ii.), it is easy to show that

$$\Omega_0 A_{m-2} = 3mA_{m-2},$$
$$\Omega_1 A_{m-2} = 3mA_{m-2}.$$

If, then, we can obtain combinations of A_0, A_1, A_2, . . . which are homogeneous and of uniform grade, such as to satisfy $\Delta_1 f = 0$ and $\Delta_2 f = 0$, these combinations will be invariants; and it follows from the effect of the linear operators Δ_1 and Δ_2 on the quantities A that any combination of the A's which satisfies $\Delta_1 f = 0$ will also satisfy $\Delta_2 f = 0$.

* SALMON, 'Higher Algebra' (3rd edition), § 198; CLEBSCH, 'Theorie der binären Formen,' § 59; GORDAN, 'Vorlesungen über Invariantentheorie,' vol. 2, § 31. The quantities A_1, A_0, J_{01}, L_2, H_0, H_{01}, H_1 are, save as to numerical factors, respectively the same as SALMON's symbols u, v, (1, 1), L_2, Δ, L_1, (2, 0); as CLEBSCH's symbols ϕ, f, ϑ, q, D, p, Δ; as GORDAN's symbols ϕ, f, ϑ, q, A_{ff}, p, Δ.

Combinations of this kind, which are of uniform grade and are homogeneous, are A_0, $A_0A_2 - k_1A_1^2$, $A_0^2A_3 - l_1A_0A_1A_2 + l_2A_1^3$, $A_0A_4 - m_1A_1A_3 + m_2A_2^2$, and so on. When these are substituted in $\Delta_1 f = 0$ and the coefficients $k, l, m, \ldots$ are determined so that the equation is satisfied, we find the following set of invariants :—

$$\begin{aligned} U_0 &= A_0, \\ U_2 &= A_0A_2 - A_1^2, \\ U_3 &= A_0^2A_3 - \tfrac{10}{3}A_0A_1A_2 + \tfrac{20}{9}A_1^3, \\ U_4 &= A_0A_4 - 5A_1A_3 + \tfrac{25}{6}A_2^2, \\ & \ldots\ldots\ldots\ldots \end{aligned}$$

These combinations suggest an analogy with the coefficients of the principal irreducible covariants of a quantic. If we change the symbols by the relation

$$A_{m-2} = m!\,(m-1)\,C_{m-2},$$

then, except as to numerical factors, the functions are

$$\begin{aligned} & C_0, \\ & C_0C_2 - C_1^2, \\ & C_0^2C_3 - 3C_0C_1C_2 + 2C_1^3, \\ & C_0C_4 - 4C_1C_3 + 3C_2^2, \\ & \ldots\ldots\ldots\ldots \end{aligned}$$

that is, they follow the same law of formation as the leading terms of the covariants referred to; and they can therefore be expressed in terms of the quantities C and can thence be deduced in terms of the quantities A.

All these functions satisfy the equation

$$C_0\frac{\partial U}{\partial C_1} + 2C_1\frac{\partial U}{\partial C_2} + 3C_2\frac{\partial U}{\partial C_3} + \ldots \qquad = 0,$$

that is, they satisfy the equation

$$\sum_{n=1}(n+2)(n+1)A_{n-1}\frac{\partial U}{\partial A_n} = 0,$$

by means of which the numerical coefficients in U can be directly determined.

It is evident from the form of U_{m-2} that the highest order of differential coefficient which enters is the mth, that all the differential coefficients of the mth order enter linearly and into only one set of terms, and that the remaining terms all involve coefficients of lower order of differentiation.

Invariants in the Fourth Order.

17. To obtain the irreducible invariants which involve no differential coefficient of order higher than four, we may proceed as in § 11 by forming the irreducible functions which satisfy the differential equations; among these functions the invariants already obtained will occur.

For convenience, let the differential coefficients of the fourth order be denoted by e, f, g, h, i ($= z_{40}, z_{31}, z_{22}, z_{13}, z_{04}$ respectively). Then, beginning as before with $\Delta_3 f = 0$, the subsidiary equations additional to those already (§ 12) considered are

$$\left[\frac{dp}{q} = \frac{dq}{0}\right] = \frac{de}{4f} = \frac{df}{3g} = \frac{dg}{2h} = \frac{dh}{i} = \frac{di}{0};$$

of which the irreducible independent integrals are

$$\begin{aligned} u_9 &= i, \\ u_{10} &= pi - qh, \\ u_{11} &= p^2 i - 2pqh + q^2 g, \\ u_{12} &= p^3 i - 3p^2 qh + 3pq^2 g - q^3 f, \\ u_{13} &= p^4 i - 4p^3 qh + 6p^2 q^2 g - 4pq^3 f + q^4 e; \end{aligned}$$

and any solution of $\Delta_3 f = 0$ is expressible as a function of $u_1, u_2, \ldots, u_{13}$.

The remainder of the analysis is very similar to that which has been used for the earlier question, and so it is not here reproduced; the following are the results:—

(i) The functional combinations of the thirteen quantities u which satisfy $\Delta_1 f = 0$ (and which are, therefore, the irreducible simultaneous solutions of $\Delta_3 f = 0 = \Delta_1 f$) are

$$\begin{aligned} &u_1, u_2, u_4, u_5, u_9: \\ v_6 &= u_1 u_6 + 2u_2 u_3, \\ v_7 &= u_1 u_7 - 2u_3^2, \\ v_8 &= u_1 u_8 + 6u_3 u_4; \\ v_{10} &= u_1 u_{10} + 3u_3 u_5, \\ v_{11} &= u_1 u_{11} + 6u_3 u_6 + 3u_2 u_7, \\ v_{12} &= u_1^2 u_{12} + 9u_1 u_3 u_7 - 12u_3^3, \\ v_{13} &= u_1^2 u_{13} + 12u_1 u_3 u_8 + 36u_3^2 u_4. \end{aligned}$$

(ii) The functional combinations of these twelve quantities which satisfy $\Delta_2 f = 0$ (and which are therefore the irreducible simultaneous solutions of $\Delta_3 f = \Delta_1 f = \Delta_2 f = 0$) are

$$u_1,\ u_4,\ v_6,\ v_8,\ v_{13}\,;$$
$$v_5 = u_1u_5 - \tfrac{3}{2}u_2^2,$$
$$w_7 = v_7 - u_2u_4\,;$$
$$v_9 = u_1^2u_9 - 6u_1u_2u_5 + 6u_2^3,$$
$$w_{10} = u_1v_{10} - \tfrac{9}{2}u_2v_6,$$
$$w_{11} = u_1v_{11} - 6u_2u_7 + \tfrac{3}{2}u_2^2u_4,$$
$$w_{12} = v_{12} - \tfrac{3}{2}u_2v_8.$$

(iii) When these functional combinations are substituted in turn for f in $\Delta_4 f$, the equations additional to those in § 12 can be transformed to

$$\begin{aligned}
u_1\,\Delta_4 v_9 &= 6pv_9 - 4w_{10},\\
u_1\,\Delta_4 w_{10} &= 5pw_{10} - 3w_{11} + 3u_4v_5,\\
u_1\,\Delta_4 w_{11} &= 4pw_{11} - 2w_{12} + 6u_4v_6,\\
u_1\,\Delta_4 w_{12} &= 3pw_{12} - v_{13} + 9u_4w_7,\\
u_1\,\Delta_4 v_{13} &= 2pv_{13} + 12u_4v_8\,;
\end{aligned}$$

and therefore, if we write

$$\frac{v_{13}}{u_1^2} = P_{13},\quad \frac{w_{12}}{u_1^3} = P_{12},\quad \frac{w_{11}}{u_1^4} = P_{11},\quad \frac{w_{10}}{u_1^5} = P_{10},\quad \frac{v_9}{u_1^6} = P_9,$$

these equations become

$$\begin{aligned}
u_1^2\Delta_4 P_9 &= -4P_{10},\\
u_1^2\Delta_4 P_{10} &= -3P_{11} + 3u_4P_5,\\
u_1^2\Delta_4 P_{11} &= -2P_{12} + 6u_4P_6,\\
u_1^2\Delta_4 P_{12} &= -P_{13} + 9u_4P_7,\\
u_1^2\Delta_4 P_{13} &= 12u_4P_8.
\end{aligned}$$

In addition to the former irreducible solutions, Q_7, Q_6, Q_5, which were obtained from equations in § 13, the following irreducible solutions can be obtained :—

$$\begin{aligned}
Q_{13} &= u_4P_{13} - P_8^2,\\
Q_{12} &= 18u_4^3P_{12} + 3u_4P_8P_{13} + 81u_4^4P_6 - 2P_8^3,\\
Q_{11} &= 72u_4^5P_{11} + 24u_4^3P_8P_{12} + 2u_4P_8^2P_{13} + 144u_4^6P_5 + 108u_4^4P_7^2 - P_8^4,\\
Q_{10} &= 216u_4^7P_{10} + 108u_4^5P_8P_{11} + 18u_4^3P_8^2P_{12} + u_4P_8^3P_{13}\\
&\qquad - (108u_4^6P_8P_5 + 81u_4^4P_8^2P_6 + 18u_4^2P_8^3P_7 + P_8^5),\\
Q_9 &= 1296u_4^9P_9 + 864u_4^7P_8P_{10} + 216u_4^5P_8^2P_{11} + 24u_4^3P_8^3P_{12} + u_4P_8^4P_{13}\\
&\qquad - (216u_4^6P_8^2P_5 + 108u_4^4P_8^3P_6 + 18u_4^2P_8^4P_7 + \tfrac{5}{6}P_8^6).
\end{aligned}$$

These are not necessarily the simplest forms obtainable, but every simultaneous solution of $\Delta_1 f = \Delta_2 f = \Delta_3 f = \Delta_4 f = 0$ can be expressed as a functional combination of u_4, Q_5, Q_6, Q_7, Q_9, . . ., Q_{13}.

It will be seen that the new irreducible functions Q_9, . . ., Q_{13} are linear in the quantities P_9, P_{10}, . . ., P_{13}, and are therefore linear in the partial differential coefficients of the fourth order. In this respect they apparently differ from Q_5, Q_6, Q_7, which are the irreducible invariants of the third rank in differentiation; but, if we take instead of Q_5 an equivalent invariant $144 u_4^2 Q_5 + Q_7^2$, which is

$$288 u_4^6 P_5 + 144 u_4^4 P_6 P_8 + 24 u_4^2 P_7 P_8^2 + P_8^4,$$

the law of successive formation of the invariants (the new) Q_5, Q_6, Q_7 is similar to that for the functions Q_9, Q_{10}, . . ., Q_{13}.

18. But, before it can be asserted that Q_9, Q_{10}, . . . Q_{13} are invariants, it must be shown that they severally for a common value of λ satisfy the equations (i′.) and (ii′.). Now the following results are easily obtained :—

$f =$	$\Omega_0 f =$	$\Omega_1 f =$	$\lambda =$
P_5	0	0	0
P_6	$3P_6$	$3P_6$	1
P_7	$6P_7$	$6P_7$	2
P_8	$9P_8$	$9P_8$	3
P_9	0	0	0
P_{10}	$3P_{10}$	$3P_{10}$	1
P_{11}	$6P_{11}$	$6P_{11}$	2
P_{12}	$9P_{12}$	$9P_{12}$	3
P_{13}	$12P_{12}$	$12P_{12}$	4

From this Table it at once follows that

$$\begin{aligned}
\Omega_0 Q_{13} &= 18Q_{13} = \Omega_1 Q_{13},\\
\Omega_0 Q_{12} &= 27Q_{12} = \Omega_1 Q_{12},\\
\Omega_0 Q_{11} &= 36Q_{11} = \Omega_1 Q_{11},\\
\Omega_0 Q_{10} &= 45Q_{10} = \Omega_1 Q_{10},\\
\Omega_0 Q_9 &= 54Q_9 = \Omega_1 Q_9.
\end{aligned}$$

Hence, Q_9, Q_{10}, Q_{11}, Q_{12}, Q_{13} *are invariants of indices* 18, 15, 12, 9, 6 *respectively; and every invariant which involves no differential coefficient of order higher than the fourth can be expressed as a function of* u_4, Q_5, Q_6, Q_7, Q_9, Q_{10}, Q_{11}, Q_{12}, Q_{13}.

General Inferences.

19. And if, among the sets of irreducible invariants thus obtained, those invariants which involve the partial differential coefficients of the nth order as the highest that occur, and which are linear in those partial differential coefficients of highest order, are called irreducible invariants *proper to the rank n*, then we have the following propositions relating to the complete aggregate of invariants :—

(i) The irreducible invariants can be ranged in sets, each set being proper to a particular rank ;

(ii) There is no irreducible invariant proper to the rank unity ;

(iii) There is a single irreducible invariant ($= u_4 = A_0$) proper to the rank 2 ;

(iv) There are three irreducible invariants ($= Q_5, Q_6, Q_7$) proper to the rank 3 ;

(v) For every value of n greater than 3, there are $n + 1$ irreducible invariants proper to the rank n, and they can be so chosen as to be linear in the differential coefficients of order n ;

(vi) Every invariant can be expressed as a function of the irreducible invariants ; and, if such an invariant have differential coefficients of order r as those of highest order occurring in it, the functional equivalent involves some or all of the aggregate of irreducible invariants proper to ranks not greater than r; it involves some of the irreducible invariants proper to the rank r, but no irreducible invariant proper to a rank greater than r.

Simultaneous Invariants of Two Functions.

20. Hitherto we have considered invariants of only a single dependent variable which is a function of the two independent variables ; but we may consider a second dependent variable, say z', which is also a function of x and y. The two quantities

z and z' are independent of one another; but, if a third dependent variable be introduced, it can, by the elimination of x and y, be expressed in terms of z and z' alone, and its invariants will be expressible partly in terms of the invariants of z and of z', and partly in terms of functions arising in connexion with the transformation of z and z'. It is thus sufficient to consider two, and not more than two, dependent variables when there are two independent variables.

21. In addition to the invariants possessed by each of the dependent variables separately, there will be simultaneous invariants which involve differential coefficients of both the variables; such a simultaneous invariant is

$$J = pq' - p'q,$$

which we have already obtained in § 4.

When the characteristic differential equations of simultaneous invariants are formed by the method already (§ 8) adopted for invariants of a single function, they are as follows:—Let F′ generally denote the same function associated with z' that F denotes associated with z. Then the equations satisfied by a simultaneous invariant ψ of two functions z and z' are

$$\begin{aligned}
\Phi_0\psi &= (\Omega_0 + \Omega'_0)\,\psi = 3\lambda\psi,\\
\Phi_1\psi &= (\Omega_1 + \Omega'_1)\,\psi = 3\lambda\psi,\\
\Theta_1\psi &= (\Delta_1 + \Delta'_1)\,\psi = 0,\\
\Theta_2\psi &= (\Delta_2 + \Delta'_2)\,\psi = 0,\\
\Theta_3\psi &= (\Delta_3 + \Delta'_3)\,\psi = 0,\\
\Theta_4\psi &= (\Delta_4 + \Delta'_4)\,\psi = 0.
\end{aligned}$$

It is easy to verify that J satisfies these equations, its index λ being unity; and it is evident that the invariants of z alone, and those of z' alone, all satisfy these equations.

As in § 11, it is easy to prove that *every simultaneous invariant must involve* p *and* q*; or* p' *and* q'*; or* p, q, p', *and* q'.

22. The only simultaneous invariant so far obtained is J; we proceed to obtain all the simultaneous invariants which involve no differential coefficients of z and of z' which are of order higher than the second, using for this purpose the method adopted in §§ 12, 17. Among these invariants there must evidently occur

$$\begin{aligned}
J &= pq' - p'q,\\
A_0 &= q^2r - 2pqs + p^2t,\\
A'_0 &= q'^2r' - 2p'q's' + p'^2t',
\end{aligned}$$

the two latter being invariants each in one dependent variable only, which necessarily satisfy all the equations.

Taking the equations in the order adopted before and beginning with $(\Delta_3 + \Delta'_3)\psi = 0$, we have the set of subsidiary equations

$$\frac{dp}{q} = \frac{dq}{0} = \frac{dr}{2s} = \frac{ds}{t} = \frac{dt}{0} = \frac{dp'}{q'} = \frac{dq'}{0} = \frac{dr'}{2s'} = \frac{ds'}{t'} = \frac{dt'}{0},$$

nine in number. It is necessary to obtain nine independent integrals of the set; and we may take these in the form

$$\left.\begin{aligned} u_1 &= q \\ u'_1 &= q' \end{aligned}\right\}, \quad \left.\begin{aligned} u_2 &= t \\ u'_2 &= t \end{aligned}\right\}, \quad \left.\begin{aligned} u_3 &= qs' - pt' \\ u'_3 &= q's - p't \end{aligned}\right\}, \quad \left.\begin{aligned} u_4 &= A_0 \\ u_4' &= A'_0 \end{aligned}\right\}, \quad u_5 = J,$$

bearing in mind the property above proved. By the theory of linear partial differential equations of the first order, it follows that every solution of the equation $(\Delta_3 + \Delta'_3)\psi = 0$ can be expressed as a functional combination of u_1, u'_1, u_2, u'_2, u_3, u'_3, u_4, u'_4, u_5. Thus,

$$\begin{aligned} qs - pt &= \frac{u_1 u'_3 - u_2 u_5}{u'_1}, \\ st' - s't &= \frac{u_1 u'_2 u'_3 - u'_1 u_2 u_3 - u_2 u'_2 u_5}{u_1 u'_1}, \\ q^2 r' - 2pqs' + p^2 t &= \frac{u_1{}^2 u'_4 - 2u'_1 u_3 u_5 - u'_2 u_5{}^2}{u'_1{}^2}, \\ q'^2 r - 2p'q's + p'^2 t &= \frac{u'_1{}^2 u_4 + 2u_1 u'_3 u_5 - u_2 u_5{}^2}{u_1{}^2}, \end{aligned}$$

and so on.

To obtain the most general solution of $\Theta_3\psi = 0 = \Theta_1\psi$, it will be sufficient to form the irreducible combinations of $u_1, \ldots, u_5$ which satisfy $\Theta_1\psi = 0$. Now,

$$\Theta_1 u_1 = 0,\ \Theta_1 u'_1 = 0; \quad \Theta_1 u_2 = 0,\ \Theta_1 u'_2 = 0; \quad \Theta_1 u_4 = 0,\ \Theta_1 u'_4 = 0; \quad \Theta_1 u_5 = 0;$$

and

$$\Theta_1 u_3 = qq', \qquad \Theta_1 u'_3 = q'q;$$

so that

$$\Theta_1(u_3 - u'_3) = 0;$$

and, therefore, the irreducible combinations which satisfy $\Theta_1\psi = 0$ are u_1, u'_1, u_2, u'_2, v_3, u_4, u'_4, u_5, where

$$v_3 = u_3 - u_3 = qs' - q's - pt' + p't.$$

To obtain the most general solution of $\Theta_3\psi = 0 = \Theta_1\psi = \Theta_2\psi$, it will be sufficient to form the irreducible combinations of the preceding eight quantities which satisfy $\Theta_2\psi = 0$. Now

$$\Theta_2 u_1 = 0, \quad \Theta_2 u'_1 = 0; \qquad \Theta_2 u_4 = 0, \quad \Theta_2 u'_4 = 0; \qquad \Theta_2 u_5 = 0;$$

and

$$\begin{aligned} \Theta_2 u_2 &= \quad 2u_1, \\ \Theta_2 u'_2 &= \quad 2u'_1, \\ \Theta_2 v_3 &= -\, 3u_5. \end{aligned}$$

Hence

$$\begin{aligned} \Theta_2 (u_1 u'_2 - u_2 u'_1) &= 0, \\ \Theta_2 (3u_2 u_5 + 2u_1 v_3) &= 0; \end{aligned}$$

and therefore, if

$$\begin{aligned} \mathrm{P} &= u_1 u'_2 - u_2 u'_1 = qt' - q't, \\ \mathrm{Q} &= 3u_2 u_5 + 2u_1 v_3, \end{aligned}$$

the irreducible combinations which satisfy $\Theta_1\psi = 0 = \Theta_2\psi = \Theta_3\psi$, are u_1, u'_1, P, u_4, u'_4, u_5, Q.

It is now necessary to obtain the irreducible combinations of these seven quantities which satisfy $\Theta_4\psi = 0$. We have

$$\Theta_4 u_1 = p, \qquad \Theta_4 u'_1 = p';$$

so that

$$u'_1 \Theta_4 u_1 - u_1 \Theta_4 u'_1 = u_5.$$

Again,

$$\Theta_4 \mathrm{P} = pt' - p't + 2qs' - 2q's.$$

Now,

$$q(pt' - p't) = p(\mathrm{P} + q't) - p'qt = p\mathrm{P} + u_5 u_2;$$

so that

$$\begin{aligned} \Theta_4 \mathrm{P} &= 3(pt' - p't) + 2v_3 \\ &= \frac{3}{q}(p\mathrm{P} + u_2 u_5) + 2v_3; \end{aligned}$$

and hence

$$u_1 \Theta_4 \mathrm{P} - 3p\mathrm{P} = \mathrm{Q}.$$

Again,

$$\tfrac{1}{2}\Theta_4 \mathrm{Q} = 3su_5 + pv_3 + q\Theta_4 v_3;$$

but

$$\Theta_4 v_3 = qr' - ps' - (q'r - p's);$$

and

$$\begin{aligned} qr' - ps' &= \frac{1}{q}(q^2 r' - 2pqs' + p^2 t') + \frac{p}{q}(qs' - pt') \\ &= \frac{1}{u_1 u'^2_1}(u_1^2 u'_4 - 2u_1' u_3 u_5 - u'_2 u_5^2) + \frac{pu'_3}{u_1}, \end{aligned}$$

$$q'r - p's = \frac{1}{u_1' u_1^2}(u'^2_1 u_4 + 2u'_1 u_3 u_5 - u'_2 u_5^2) + \frac{p'u'_3}{u'_1};$$

so that

$$\Theta_4 v_3 = \frac{p}{u_1} u_3 - \frac{p' u'_3}{u'_1} + \frac{u_1 u'_4}{u_1^2} - \frac{u'_1 u_4}{u_1^2} - \frac{2u_5}{u_1 u'_1}(u_3 + u_3') - \frac{u_5^2}{u_1^2 u'^2_1} \mathrm{P}.$$

Hence

$$pv_3 + q\Theta_4 v_3 = 2pv_3 + \frac{u_1^2}{u'^2_1} u'_4 - \frac{u'_1}{u_1} u_4 - \frac{u_5^2}{u_1 u'^2_1} \mathrm{P} - \frac{u_5}{u'_1}(2u_3 + u'_3).$$

Hence

$$\begin{aligned} \tfrac{1}{2} u_1 \Theta_4 \mathrm{Q} - p\mathrm{Q} &= u_1 \left(\frac{u_1^2}{u'^2_1} u_4' - \frac{u'_1}{u_1} u_4 \right) - \frac{u_5^2}{u'^2_1} \mathrm{P} - \frac{u_5 u_1}{u'_1} \left(2u_3 + u'_3 - 3q's + 3\frac{pq'}{q} t \right) \\ &= u_1 \left(\frac{u_1^2}{u'^2_1} u_4' - \frac{u'_1}{u_1} u_4 \right) - \frac{u_5^2}{u'^2_1} \mathrm{P} - \frac{u_5 \mathrm{Q}}{u'_1}. \end{aligned}$$

If now we write

$$\frac{u_1}{u'_1} = \mathrm{C},$$

$$\frac{\mathrm{P}}{u_1^3} = \mathrm{B},$$

$$\frac{\mathrm{Q}}{u_1^2} = \mathrm{A},$$

the three results can be put into the forms

$$\begin{aligned} u'^2_1 \Theta_4 \mathrm{C} &= u_5, \\ u'^2_1 \Theta_4 \mathrm{B} &= \mathrm{AC}^{-4}, \\ u'^2_1 \Theta_4 \mathrm{A} &= 2\mathrm{C}^2 u'_4 - 2\mathrm{C}^{-1} u_4 - 2\mathrm{BC}^2 u_5^2. \end{aligned}$$

And, further, we have

$$\Theta_4 u_4 = 0, \qquad \Theta_4 u_4' = 0, \qquad \Theta_4 u_5 = 0.$$

Since the result of operating with Θ_4 on B gives a quantity into which A enters linearly, and the result of operating with Θ_4 on A gives another quantity into which B enters linearly, we are led to assume that the irreducible solution (or solutions) of $\Theta_4 \psi = 0$ are of the form

$$\mathrm{RA} + \mathrm{SB} + \mathrm{T},$$

where R, S, T are independent of A and B. If this be a solution, we have

$$u'^2_1 \Theta_4 \mathrm{T} = \mathrm{R}(2\mathrm{C}^2 u'_4 - 2\mathrm{C}^{-1} u_4 - 2\mathrm{BC}^2 u_5^2) + \mathrm{SAC}^{-4} + \mathrm{A} u'^2_1 \Theta_4 \mathrm{R} + \mathrm{B} u'^2_1 \Theta_4 \mathrm{S};$$

and we suppose R and S so determined that

$$\mathrm{SC}^{-4} = - u'^2_1 \Theta_4 \mathrm{R},$$

$$2\mathrm{RC}^2 u_5^2 = u'^2_1 \Theta_4 \mathrm{S}.$$

But, if $R = C^n$, then

$$SC^{-4} = - nu'^2_1 C^{n-1} \Theta_4 C = - nu_5 C^{n-1};$$

so that

$$S = - nu_5 C^{n+3};$$

and therefore

$$2u_5^2 C^{n+2} = 2RC^2 u_5^2 = - n(n+3) u_5 C^{n+2} u'^2_1 \Theta_4 C$$
$$= - n(n+3) u_5^2 C^{n+2},$$

whence

$$n = -1 \quad \text{or} \quad -2.$$

First, taking $n = -1$, we have

$$u'^2_1 \Theta_4 T = 2Cu'_4 - 2C^{-2} u_4,$$

and therefore

$$Tu_5 = C^2 u'_4 + 2u_4 C^{-1}.$$

Hence we may take as one irreducible solution

$$X = \frac{A}{C} + BC^2 u_5 - \frac{2u_4 + C^3 u'_4}{u_5 C}.$$

Second, taking $n = -2$, we have

$$u'^2_1 \Theta_4 T = 2u'_4 - 2C^{-3} u_4;$$

and therefore

$$Tu_5 = 2Cu'_4 + C^{-2} u_4.$$

Hence we may take as another irreducible solution

$$Y = \frac{A}{C^2} + 2BCu_5 - \frac{u_4 + 2C^3 u'_4}{u_5 C^2}.$$

And it follows from the method of derivation, and by an application of the theory of linear partial differential equations, that every simultaneous solution of the equations $\Theta_1\psi = 0 = \Theta_2\psi = \Theta_3\psi = \Theta_4\psi$ which involves no quantity of order higher than r, s, t, r', s', t' can be expressed as a functional combination of u_5; u_4, u'_4; X, Y.

23. It is now necessary to consider the index equations. We have for u_5 $(= J)$

$$\Phi_0 J = 3J,$$
$$\Phi_1 J = 3J;$$

so that J is an invariant of index unity. For u_4 $(= A_0)$ we have

$$\Phi_0 A_0 = 6A_0,$$
$$\Phi_1 A_0 = 6A_0;$$

so that A_0 is an invariant of index 2; and similarly for $u'_4\,(= A'_0)$,

$$\Phi_0 A'_0 = 6A'_0,$$
$$\Phi_1 A_0' = 6A'_0;$$

so that A'_0 is an invariant of index 2.

It is desirable to modify the forms of X and Y, so as to express them explicitly in terms of the quantities p, p', ... When the values of A, B, C, u_4, u'_4, u_5 are substituted in X, and it is multiplied by $-u_5$, it takes the form

$$q^2r' - 2pqs' + p^2t' + 2\{qq'r - (pq' + p'q)s + pp't\},$$

which may be denoted by $\mathfrak{A}_0$; and when exactly the same operations are applied to Y, it takes the form

$$q'^2r - 2p'q's + p'^2t + 2\{qq'r' - (pq' + p'q)s' + pp't'\},$$

which may be denoted by $\mathfrak{A}'_0$.

It is now easy to verify that

$$\Phi_0\mathfrak{A}_0 = 6\mathfrak{A}_0,$$
$$\Phi_1\mathfrak{A}_0 = 6\mathfrak{A}_0;$$

so that $\mathfrak{A}_0$ is an invariant of index 2; and similarly that

$$\Phi_0\mathfrak{A}'_0 = 6\mathfrak{A}'_0,$$
$$\Phi_1\mathfrak{A}'_0 = 6\mathfrak{A}'_0;$$

so that $\mathfrak{A}'_0$ is an invariant of index 2.

24. The general result of the preceding investigation can be enunciated as follows:—

Every simultaneous invariant of two functions z and z′ of two independent variables, which involves no differential coefficients of order higher than the second, can be expressed in terms of the five irreducible invariants J (*of index* 1) *and* A_0, A'_0, $\mathfrak{A}_0$, $\mathfrak{A}'_0$ (*each of index* 2) *where*

$$\begin{aligned} J &= pq' - p'q, \\ A_0 &= q^2r - 2pqs + p^2t, \\ A'_0 &= q'^2r' - 2p'q's' + p'^2t', \\ \mathfrak{A}_0 &= q^2r - 2pqs' + p^2t' + 2\{qq'r - (pq' + p'q)s + pp't\}, \\ \mathfrak{A}'_0 &= q'^2r - 2p'q's + p'^2t + 2\{qq'r' - (pq' + p'q)s' + pp't'\}, \end{aligned}$$

and $p, q, r, s, t;\ p', q', r', s', t';$ *have their ordinary significations as partial differential coefficients of* z *and of* z'*.

* It is easy to see that the invariant A_0, formed for $z + \lambda z'$ is

$$A_0 + \lambda\mathfrak{A}_0 + \lambda^2\mathfrak{A}'_0 + \lambda^3A'_0.$$

This remark is practically due to Professor CAYLEY.

Theory of Eduction.

25. It has already appeared from § 4 that the operator

$$q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}$$

operating on z' produces an invariant of index unity. But for the purposes of this operation z' may be regarded merely as an unchanging quantity, and, therefore, it may be replaced by an absolute invariant (of index zero); and, when the operator acts upon an absolute invariant, there results a new invariant, of the next higher rank in the differential coefficient of the variable and of index unity.

We can, however, make the operator an absolute invariant, for the index of A_0 is 2; and, therefore,

$$A_0^{-\frac{1}{2}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)$$

is an absolute invariantive operator which, when it operates on an absolute invariant, generates a new absolute invariant of next higher rank.

The operator can evidently be applied any number of times in succession, so that, if I be an absolute invariant,

$$\left\{A_0^{-\frac{1}{2}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)\right\}^r \mathrm{I}$$

is an absolute invariant for all values of the index r.

Similarly, the result of operating upon any absolute invariant with the operator

$$q'\frac{\partial}{\partial x} - p'\frac{\partial}{\partial y}$$

is to give a relative invariant of index unity; and, if we are considering simultaneous invariants in two variables z and z', then

$$\frac{1}{\mathrm{J}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right),$$

$$\frac{1}{\mathrm{J}}\left(q'\frac{\partial}{\partial x} - p'\frac{\partial}{\partial y}\right)$$

are absolute invariantive operators, which, when applied to absolute invariants, produce absolute invariants.

26. Thus, in the case of a single dependent variable, we have

$$A = Q_5 A_0^{-2}, \qquad B = Q_6 A_0^{-\frac{5}{2}}, \qquad C = Q_7 A_0^{-3}$$

as the three irreducible invariants proper to the rank three, and they form the complete system of irreducible absolute invariants within this rank. Hence

$$A' = A_0^{-\frac{1}{2}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)A,$$

$$B' = A_0^{-\frac{1}{2}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)B,$$

$$C' = A_0^{-\frac{1}{2}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)C,$$

are absolute invariants proper to the rank four. But it is not to be inferred that A, B, C, A′, B′, C′ constitute the complete system of irreducible absolute invariants within the rank four.

Again, in the operator the quantities p and q are first differential coefficients of an unchanging quantity z; they can be replaced by first differential coefficients of any absolute invariant I, and then

$$A_0^{-\frac{1}{2}}\left(\frac{\partial I}{\partial y}\frac{\partial}{\partial x} - \frac{\partial I}{\partial x}\frac{\partial}{\partial y}\right)$$

is an absolute invariantive operator, the operation of which on absolute invariants produces other absolute invariants. Hence

$$D = A_0^{-\frac{1}{2}}\left(\frac{\partial B}{\partial y}\frac{\partial}{\partial x} - \frac{\partial B}{\partial x}\frac{\partial}{\partial y}\right)C,$$

$$E = A_0^{-\frac{1}{2}}\left(\frac{\partial C}{\partial y}\frac{\partial}{\partial x} - \frac{\partial C}{\partial x}\frac{\partial}{\partial y}\right)A,$$

$$F = A_0^{-\frac{1}{2}}\left(\frac{\partial A}{\partial y}\frac{\partial}{\partial x} - \frac{\partial A}{\partial x}\frac{\partial}{\partial y}\right)B,$$

are absolute invariants proper to the rank four, and they are of the second degree in the differential coefficients of the fourth order. Among the six quantities A′, B′, C′, D, E, F there is the relation,

$$A'D + B'E + C'F = 0;$$

so that only five of them can be independent.

27. In any higher rank n let I, J, K, . . . be the invariants, absolute and irreducible, proper to that rank; let I′ denote the absolute invariant educed from I by the operator

$$A_0^{-\frac{1}{2}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right),$$

and I_m the absolute invariant educed from I by the operator

$$A_0^{-\frac{1}{2}}\left(\frac{\partial M}{\partial y}\frac{\partial}{\partial x} - \frac{\partial M}{\partial x}\frac{\partial}{\partial y}\right),$$

M being an absolute invariant. Then, by means of all the eductive operators associated with absolute invariants of successive ranks, we can obtain from I the set of educed invariants

$$\begin{array}{l} \mathrm{I}'; \\ \mathrm{I}_a,\ \mathrm{I}_b,\ \mathrm{I}_c; \\ \mathrm{I}_{a'},\ \mathrm{I}_{b'},\ \mathrm{I}_{c'},\ \mathrm{I}_d,\ \mathrm{I}_e,\ \mathrm{I}_f; \\ .\quad .\quad .\quad .\quad .\quad .\quad .\quad .; \\ \mathrm{I}_j,\ \mathrm{I}_k,\ \ldots, \end{array}$$

all proper to the rank $n+1$; and there is a similar set from each of the other invariants J, K, . . .

This number, however, can be at once reduced; for, if I_m be any educed invariant other than I′ and I_a, we have

$$\begin{vmatrix} \mathrm{I}', & q, & p \\ \mathrm{I}_a, & \dfrac{\partial \mathrm{A}}{\partial y}, & \dfrac{\partial \mathrm{A}}{\partial x} \\ \mathrm{I}_m, & \dfrac{\partial \mathrm{M}}{\partial y}, & \dfrac{\partial \mathrm{M}}{\partial x} \end{vmatrix} = 0;$$

and therefore

$$\mathrm{I}_a\mathrm{M}' = \mathrm{I}'\mathrm{M}_a + \mathrm{I}_m\mathrm{A}',$$

which shows that I_m can be expressed in terms of I′ and I_a, and of invariants proper to lower ranks if M be different from J, K, . . ., and that, if M coincide with one of the invariants J, K, . . ., the invariant I_m can be expressed in terms of the set I′, J′, . . ., the set I_a, J_a, . . ., and of invariants proper to lower ranks.

It therefore follows that the invariants, educed from the absolute irreducible invariants I, J, K, . . . proper to the rank n, can be expressed in terms of I′, J′, K′, . . . ; I_a, J_a, K_a, . . . proper to the rank $n+1$, and of invariants proper to lower ranks. All these educed invariants are, if n be greater than 3, linear in the partial differential coefficients, which are of order $n+1$, and so determine the rank of the invariants.

We know that, for values of n greater than 3, the number of irreducible invariants proper to the rank n is $n+1$, all of which can be made absolute on division by an appropriate power of A_0; hence, the number of invariants educed as above is $2(n+1)$, which must all be expressible in terms of the $n+2$ irreducible invariants proper to the rank $n+1$. But so far there is nothing to indicate which of them, or how many of them, are equivalent to irreducible invariants proper to the rank to which they belong.

28. Again, we have seen that there are four simultaneous invariants of two functions proper to the rank 2, and that there is a single invariant proper to the rank 1; so that

$$\mathrm{C}_0 = \mathrm{A}_0 \mathrm{J}^{-2}, \qquad \mathrm{C}'_0 = \mathrm{A}'_0 \mathrm{J}^{-2}, \qquad \mathfrak{C}_0 = \mathfrak{A}_0 \mathrm{J}^{-2}, \qquad \mathfrak{C}'_0 = \mathfrak{A}'_0 \mathrm{J}^{-2},$$

are absolute irreducible invariants proper to the rank 2. Let

$$\frac{1}{\mathrm{J}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)\mathrm{C}_0 = \mathrm{F}, \qquad \frac{1}{\mathrm{J}}\left(q'\frac{\partial}{\partial x} - p'\frac{\partial}{\partial y}\right)\mathrm{C}_0 = \mathrm{F}';$$

$$\frac{1}{\mathrm{J}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)\mathrm{C}'_0 = \mathrm{G}, \qquad \frac{1}{\mathrm{J}}\left(q'\frac{\partial}{\partial x} - p'\frac{\partial}{\partial y}\right)\mathrm{C}'_0 = \mathrm{G}';$$

$$\frac{1}{\mathrm{J}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)\mathfrak{C}_0 = \mathfrak{F}, \qquad \frac{1}{\mathrm{J}}\left(q'\frac{\partial}{\partial x} - p'\frac{\partial}{\partial y}\right)\mathfrak{C}_0 = \mathfrak{F}';$$

$$\frac{1}{\mathrm{J}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)\mathfrak{C}'_0 = \mathfrak{G}, \qquad \frac{1}{\mathrm{J}}\left(q'\frac{\partial}{\partial x} - p'\frac{\partial}{\partial y}\right)\mathfrak{C}'_0 = \mathfrak{G}';$$

then F, F′, G, G′, $\mathfrak{F}$, $\mathfrak{F}'$, $\mathfrak{G}$, $\mathfrak{G}'$, are eight educed invariants proper to the rank three. But instead of q and p, or q' and p', we can substitute the first differential coefficients of any unchanging quantity, say of any one of the absolute invariants C_0, C'_0, $\mathfrak{C}_0$, $\mathfrak{C}'_0$, and thus educe new invariants. All these, however, can be expressed in terms of the set of eight already retained; for we at once have

$$\frac{\partial \mathrm{C}_0}{\partial x} = p\,\mathrm{F}' - p'\,\mathrm{F},$$

$$\frac{\partial \mathrm{C}_0}{\partial y_3} = q\,\mathrm{F}' - q'\,\mathrm{F};$$

and therefore

$$\frac{1}{\mathrm{J}}\left(\frac{\partial \mathrm{C}_0}{\partial y}\frac{\partial}{\partial x} - \frac{\partial \mathrm{C}_0}{\partial x}\frac{\partial}{\partial y}\right)\mathrm{C}'_0 = \mathrm{J}\,(\mathrm{F}'\mathrm{G} - \mathrm{F}\,\mathrm{G}'),$$

which proves the statement.*

Hence, through the present class of eductive operators we are able to derive from the simultaneous invariants proper to a rank n double the number of educed invariants proper to the next higher rank; but it is not to be inferred that they are all irreducible, or that they form the complete system of irreducible invariants proper to that rank.

29. The foregoing linear operators are not the only eductive operators; in fact, each new invariant suggests a new eductive operator. For the fundamental property of non-variation on the part of z, the differential coefficients of which are combined into invariants, enables us to substitute for z any other unchanging quantity, such as an absolute invariant. Thus, for instance, if I be any absolute invariant, then

$$\left(\frac{\partial \mathrm{I}}{\partial y}\right)^2\frac{\partial^2 \mathrm{I}}{\partial x^2} - 2\frac{\partial \mathrm{I}}{\partial x}\frac{\partial \mathrm{I}}{\partial y}\frac{\partial^2 \mathrm{I}}{\partial x\,\partial y} + \left(\frac{\partial \mathrm{I}}{\partial x}\right)^2\frac{\partial^2 \mathrm{I}}{\partial y^2},$$

* Similarly for functions of $z + \lambda z'$; thus

$$\mathrm{F}\,(z + \lambda z') = \mathrm{F} + \lambda\,(\mathfrak{F} + \mathrm{F}') + \lambda^2(\mathfrak{G} + \mathfrak{F}') + \lambda^3(\mathrm{G} + \mathfrak{G}') + \lambda^4\mathrm{G}'.$$

the same function of I as A'_0 is of z', is an invariant of index 2, and

$$q^2\frac{\partial^2 I}{\partial x^2} - 2pq\frac{\partial^2 I}{\partial x\,\partial y} + p^2\frac{\partial^2 I}{\partial y^2} + 2\left\{(qr-ps)\frac{\partial I}{\partial y} - (qs-pt)\frac{\partial I}{\partial x}\right\},$$

the same function of z and I as $\mathfrak{A}_0$ is of z and z', is an invariant of index 2.

30. The expressions for educed invariants can be applied as follows to obtain the expressions for the effects of the *operation of* Θ_1, Θ_2, Θ_3, Θ_4 (§ 20) *on differential coefficients of the invariants with regard to the variables.*

Let V be an invariant of index m, and let the operators $q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}$, $q'\frac{\partial}{\partial x} - p'\frac{\partial}{\partial y}$ be denoted by δ and δ' respectively. Then

$$V_1 = J\delta V - mV\delta J,$$
$$V'_1 = J\delta' V - mV\delta' J$$

are invariants of index $m+2$; they must satisfy the equations

$$\Theta_1 f = 0 = \Theta_2 f = \Theta_3 f = \Theta_4 f.$$

Hence

$$J\Theta\delta V = mV\Theta\delta J,$$
$$J\Theta\delta' V = mV\Theta\delta' J,$$

are satisfied for each of the operators Θ, because J and V are themselves invariants. Now, actual substitution gives

$$\begin{aligned} \Theta_1\delta J &= 3qJ, & \Theta_1\delta' J &= 3q'J;\\ \Theta_2\delta J &= -3pJ, & \Theta_2\delta' J &= -3p'J;\\ \Theta_3\delta J &= 0, & \Theta_3\delta' J &= 0;\\ \Theta_4\delta J &= 0, & \Theta_4\delta' J &= 0;\end{aligned}$$

and therefore

$$\begin{aligned} \Theta_1\delta V &= 3qVm, & \Theta_1\delta' V &= 3q'Vm;\\ \Theta_2\delta V &= -3pVm, & \Theta_2\delta' V &= -3p'Vm;\\ \Theta_3\delta V &= 0, & \Theta_3\delta' V &= 0;\\ \Theta_4\delta V &= 0, & \Theta_4\delta' V &= 0.\end{aligned}$$

Now, since

$$\Theta_1(\delta V) = q\Theta_1\frac{\partial V}{\partial x} - p\Theta_1\frac{\partial V}{\partial y},$$

and

$$\Theta_1\delta' V = q'\Theta_1\frac{\partial V}{\partial x} - p'\Theta_1\frac{\partial V}{\partial y},$$

it follows from the first pair of equations that

$$\left.\begin{aligned}\Theta_1 \frac{\partial V}{\partial x} &= 3mV \\ \Theta_1 \frac{\partial V}{\partial y} &= 0\end{aligned}\right\} .$$

Similarly, from the second pair

$$\left.\begin{aligned}\Theta_2 \frac{\partial V}{\partial x} &= 0 \\ \Theta_2 \frac{\partial V}{\partial y} &= 3mV\end{aligned}\right\} ;$$

from the third pair

$$\left.\begin{aligned}\Theta_3 \frac{\partial V}{\partial x} &= \frac{\partial V}{\partial y} \\ \Theta_3 \frac{\partial V}{\partial y} &= 0\end{aligned}\right\} ;$$

and from the fourth pair

$$\left.\begin{aligned}\Theta_4 \frac{\partial V}{\partial x} &= 0 \\ \Theta_4 \frac{\partial V}{\partial y} &= \frac{\partial V}{\partial x}\end{aligned}\right\} .$$

And the general laws, of which these are particular examples, and which can be established by means of the successive educts of the invariant V, are

$$\left.\begin{aligned}\Theta_1 \frac{\partial^n V}{\partial x^s \partial y^{n-s}} &= s(3m+n-1)\frac{\partial^{n-1} V}{\partial x^{s-1}\partial y^{n-s}} \\ \Theta_2 \frac{\partial^n V}{\partial x^{n-s} \partial y^{s}} &= s(3m+n-1)\frac{\partial^{n-1} V}{\partial x^{n-s}\partial y^{s-1}} \\ \Theta_3 \frac{\partial^n V}{\partial x^r \partial y^{n-r}} &= r\,\frac{\partial^n V}{\partial x^{r-1}\partial y^{n-r+1}} \\ \Theta_4 \frac{\partial^n V}{\partial x^{n-r} \partial y^{r}} &= r\,\frac{\partial^n V}{\partial x^{n-r+1}\partial y^{r-1}}\end{aligned}\right\} .$$

From these the effect on V of any combinations in any order of the operators $\partial/\partial x$, $\partial/\partial y$, Θ_1, Θ_2, Θ_3, Θ_4, can be deduced.

31. The following is another application of the theory of eduction. The index of U_2 (§ 16) is 6, so that $U_2U_0^{-3}$ is an absolute invariant, and therefore

$$\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right) U_2U_0^{-3}$$

is an invariant, say

$$V = V_0\,\delta U_2 - 3U_2\,\delta U_0.$$

Now the quantities U are expressed in terms of the quantities A; and from the values of those quantities it at once follows that

$$\begin{aligned}\delta A_m &= A_{m+1} + (qr - sp)\frac{\partial A_m}{\partial p} + (qs - tp)\frac{\partial A_m}{\partial q} \\ &= A_{m+1} - \tfrac{1}{2}\left(\frac{\partial A_0}{\partial p}\frac{\partial A_m}{\partial q} - \frac{\partial A_0}{\partial q}\frac{\partial A_m}{\partial p}\right) \\ &= A_{m+1} - \tfrac{1}{2} J_{0m};\end{aligned}$$

and therefore in particular

$$\begin{aligned}\delta A_0 &= A_1, \\ \delta A_1 &= A_2 - \tfrac{1}{2}J_{01}, \\ \delta A_2 &= A_3 - \tfrac{1}{2}J_{02}.\end{aligned}$$

Hence

$$\begin{aligned}\delta U_2 &= \delta(A_0A_2 - A_1^2) \\ &= A_0A_3 - A_1A_2 - \tfrac{1}{2}A_0J_{02} + A_1J_{01};\end{aligned}$$

and therefore

$$V = A_0^2A_3 - 4A_0A_1A_2 + 3A_1^3 - \tfrac{1}{2}A_0^2J_{02} + A_0A_1J_{01},$$

an invariant proper to the rank 5. But

$$U_3 = A_0^2A_3 - \tfrac{10}{3}A_0A_1A_2 + \tfrac{20}{9}A_1^3$$

is an invariant proper to the rank 5; hence

$$Z = V - U_3 = \tfrac{7}{9}A_1^3 - \tfrac{2}{3}A_0A_1A_2 - \tfrac{1}{2}A_0^2J_{02} + A_0A_1J_{01}$$

is an invariant, and it is evidently proper to the rank 4. It must, therefore, be expressible in terms of the irreducible invariants within the rank 4 given by $u_4, Q_5, Q_6, Q_7, Q_9, \ldots, Q_{13}$. The verification of this inference is as follows.

32. We have

$$Q_{13} = u_4P_{13} - P_8^2;$$

when the values of P_{13} and of P_8—viz.:

$$\frac{1}{u_1'^2}(u_1^2u_{13} + 12u_1u_3u_8 + 36u_3^2u_4) \quad \text{and} \quad \frac{1}{u_1}(u_1u_8 + 6u_3u_4)$$

respectively—are substituted, we at once have

$$\begin{aligned}Q_{13} &= u_4u_{13} - u_8^2 \\ &= A_0A_2 - (-A_1)^2 = A_0A_2 - A_1^2,\end{aligned}$$

thus identifying Q_{13} with U_2.

Again, we have

$$\begin{aligned} Q_{12} &= 18u_4^3P_{12} + 3u_4P_8P_{13} + 81u_4^4P_6 - 2P_8^3 \\ &= 3u_4X + 81u_4^4P_6 - 2P_8^3 ; \end{aligned}$$

so that X, denoting $6u_4^2P_{12} + P_8P_{13}$, includes all terms proper to the rank 4. When we substitute for P_8, P_{12}, and P_{13} their values we have

$$\begin{aligned} X &= \frac{6u_4^2}{u_1^3}(u_1^2u_{12} + 9u_1u_3u_7 - 12u_3^3 - \tfrac{3}{2}u_2v_8) + \frac{v_8}{u_1^3}(u_1^2u_{13} + 12u_1u_3u_8 + 36u_3^2u_4) \\ &= \frac{6u_4^2u_{12} + v_8u_{13}}{u_1} + X', \end{aligned}$$

where

$$X' = \frac{18u_4^2}{u_{13}}(3u_1u_3u_7 - 4u_3^3 - \tfrac{1}{2}u_2v_8) + \frac{12v_8}{u_1^3}(u_1u_3u_8 + 3u_3^2u_4).$$

Now, for the first part of X we have

$$\frac{6u_4u_{12} + v_8u_{13}}{u_1} = u_8u_{13} + \frac{6u_4}{u_1}(u_4u_{12} + u_3u_{13}) ;$$

and

$$u_4u_{12} + u_3u_{13} = \tfrac{1}{8}\left\{\left(p\frac{\partial u_4}{\partial p} + q\frac{\partial u_4}{\partial q}\right)\frac{\partial u}{\partial p} + \left(q\frac{\partial u_{13}}{\partial q} + p\frac{\partial u_{13}}{\partial p}\right)\left(-\frac{\partial u_4}{\partial p}\right)\right\} = -\tfrac{1}{8}u_1J_{02},$$

the former of the two last lines being obtained partly from the forms of u_3 and u_{12} and partly because u_4 and u_{13} are homogeneous in p and q. Hence

$$\begin{aligned} X &= X' + u_8u_{13} + \tfrac{3}{4}u_4J_{02} \\ &= X' - A_1A_2 - \tfrac{3}{4}A_0J_{02}, \end{aligned}$$

and X′ includes terms of rank not greater than 3. It thus appears that the aggregate of the terms proper to the rank 4 are functionally the same in Z as in Q_{12}; and we have

$$\begin{aligned} \frac{Q_{12}}{3u_4} - \frac{3Z}{2u_4} &= X' + 27u_4^3P_6 - \tfrac{2}{3}\frac{P_8^3}{u_4} - \tfrac{7}{6}\frac{A_1^3}{u_4} - \tfrac{3}{2}A_1J_{01} \\ &= X' + 27u_4^3P_6 - \tfrac{2}{3}\frac{P_8^3}{u_4} + \tfrac{7}{6}\frac{u_8^3}{u_4} + \tfrac{3}{2}u_8J_{01}. \end{aligned}$$

Now, from § 13 we have

$$J_{01} = \frac{6}{u_1}(u_3u_8 + u_4u_7) ;$$

and from the values of P_7 and P_8 it follows that

$$u_8 = P_8 - \frac{6u_3u_4}{u_1},$$

$$u_7 = u_1P_7 + \frac{u_2u_4}{u_1} + \frac{2u_3^2}{u_1}.$$

Substituting now in X′, in J_{01}, and for u_8 the values as given for u_7 and u_8 in the last two equations (so as to express all the aggregates of coefficients proper to the rank 3 in terms of P_6, P_7, P_8 and to leave the residue of terms—if there be such a residue—as a function of u_1, u_2, u_3, u_4) and gathering together like terms, we find

$$\begin{aligned} X' + 27u_4{}^3P_6 - \tfrac{2}{3}\frac{P_8{}^3}{u_4} + \tfrac{7}{6}\frac{u_8{}^3}{u_4} + \tfrac{3}{2}u_8J_{01} &= 27u_4{}^3P_6 + \tfrac{1}{2}\frac{P_8{}^3}{u_4} + 9u_4P_7P_8 \\ &= \frac{Q_6}{2u_4}. \end{aligned}$$

Hence we have

$$Z = \tfrac{2}{9}Q_{12} - \tfrac{1}{3}Q_6\,;$$

and it follows that *the first educt of* U_2 $(= Q_{13})$ *when reduced by means of* U_3 *is functionally equivalent to the invariant* Q_{12}.

33. In the preceding investigation the Jacobian of the function A_0 and any other function A_m of the series in § 16 entered. The following formulæ, interesting in themselves, are of use in a verification that Z actually satisfies all the differential equations which are characteristic of an invariant :—

For $m > 2$,

$$\Delta_1\frac{\partial A_{m-2}}{\partial p} = m(m-1)\,q\,\frac{\partial A_{m-3}}{\partial p}$$

$$\Delta_1\frac{\partial A_{m-2}}{\partial q} = m(m-1)\left(q\frac{\partial A_{m-3}}{\partial q} + A_{m-3}\right)$$

and, for $m = 2$,

$$\Delta_1\frac{\partial A_0}{\partial p} = -\,2q^2$$

$$\Delta_1\frac{\partial A_0}{\partial q} = 2pq \quad ;$$

and, for $m > 2$,

$$\Delta_2\frac{\partial A_{m-2}}{\partial p} = -\,m(m-1)\left(p\frac{\partial A_{m-3}}{\partial p} + A_{m-3}\right)$$

$$\Delta_2\frac{\partial A_{m-2}}{\partial q} = -\,m(m-1)\,p\,\frac{\partial A_{m-3}}{\partial q}$$

and, for $m = 2$,

$$\Delta_2\frac{\partial A_0}{\partial p} = 2pq$$

$$\Delta_2\frac{\partial A_0}{\partial q} = -\,2p^2 \quad ;$$

and, for all values of m,

$$\left.\begin{aligned} \Delta_3 \frac{\partial \mathrm{A}_{m-2}}{\partial p} &= 0 \\ \Delta_3 \frac{\partial \mathrm{A}_{m-2}}{\partial q} &= -\frac{\partial \mathrm{A}_{m-2}}{\partial p} \end{aligned}\right\} ;$$

$$\left.\begin{aligned} \Delta_4 \frac{\partial \mathrm{A}_{m-2}}{\partial p} &= -\frac{\partial \mathrm{A}_{m-2}}{\partial q} \\ \Delta_4 \frac{\partial \mathrm{A}_{m-2}}{\partial q} &= 0 \end{aligned}\right\} .$$

From these it follows that, if neither l nor m be zero,

$$\left.\begin{aligned} \Delta_1 \mathrm{J}_{l,m} &= (l+1)(l+2)\left(q\mathrm{J}_{l-1,m} - \mathrm{A}_{l-1}\frac{\partial \mathrm{A}_m}{\partial p}\right) + (m+1)(m+2)\left(q\mathrm{J}_{l,m-1} + \mathrm{A}_{m-1}\frac{\partial \mathrm{A}_l}{\partial p}\right) \\ \Delta_2 \mathrm{J}_{l,m} &= -(l+1)(l+2)\left(p\mathrm{J}_{l-1,m} + \mathrm{A}_{l-1}\frac{\partial \mathrm{A}_m}{\partial q}\right) - (m+1)(m+2)\left(p\mathrm{J}_{l,m-1} - \mathrm{A}_{m-1}\frac{\partial \mathrm{A}_l}{\partial q}\right) \\ \Delta_3 \mathrm{J}_{l,m} &= 0 = \Delta_4 \mathrm{J}_{l,m} \end{aligned}\right\} ;$$

and

$$\left.\begin{aligned} \Delta_1 \mathrm{J}_{0,m} &= -2q(m+2)\mathrm{A}_m + (m+2)(m+1)\left(q\mathrm{J}_{0,m-1} + \frac{\partial \mathrm{A}_0}{\partial p}\mathrm{A}_{m-1}\right) \\ \Delta_2 \mathrm{J}_{0,m} &= 2p(m+2)\mathrm{A}_m - (m+2)(m+1)\left(p\mathrm{J}_{0,m-1} - \frac{\partial \mathrm{A}_0}{\partial q}\mathrm{A}_{m-1}\right) \\ \Delta_3 \mathrm{J}_{0,m} &= 0 = \Delta_4 \mathrm{J}_{0,m} \end{aligned}\right\} .$$

Connexion with Theory of Binary Forms.

34. In connexion with the fact (§ 15) that the irreducible invariants proper to the rank 3 are expressible in terms of the simultaneous concomitants of A_0 and A_1, viewed as binary forms (quadratic and cubic) in q and $-p$ as variables, it is important to remark that the equations $\Delta_3 f = 0$ and $\Delta_4 f = 0$ are in fact the differential equations satisfied by all concomitants of binary forms which have q and $-p$ for their variables, and have

$$\begin{array}{lllll} r, & s, & t\,; & & \\ a, & b, & c, & d\,; & \\ e, & f, & g, & h, & i\,; \\ \cdot & \cdot & \cdot & \cdot & \cdot \end{array}$$

for their coefficients, that is, of $\mathrm{A}_0, \mathrm{A}_1, \mathrm{A}_2, \ldots$, viewed as binary forms. Each form of a concomitant-system satisfies the differential equations characteristic of its concomitants; and it thus appears how A_0, A_1, A_2, . . . are (§§ 9, 16) simultaneous solutions of the two characteristic equations in question. Moreover, since the Jacobian of two binary forms occurs in their concomitant-system, and therefore satisfies the characteristic equations, it is now evident that the quantities denoted by $J_{l,m}$, being

$$\frac{\partial A_l}{\partial p}\frac{\partial A_m}{\partial q} - \frac{\partial A_l}{\partial q}\frac{\partial A_m}{\partial p},$$

must satisfy the equations $\Delta_3 f = 0 = \Delta_4 f$.

Hence, it appears that one method of obtaining the irreducible invariants, which are proper to the rank n and are additional to those proper to ranks less than n, is as follows:—(1) to obtain the concomitants of A_{n-2}, and the simultaneous concomitants of A_{n-2}, and of the concomitant-system of A_{n-3}, A_{n-4}, . . . , A_1, A_0, viewed as binary forms; (2) to frame the combinations of these concomitants which will satisfy the remaining characteristic equations $\Delta_1 f = 0 = \Delta_2 f$; (3) to select from among these combinations such as are, from the supposed known algebraical relations among the concomitants, found to be irreducible.

35. Again, in the case of binary forms in two systems of variables, q and $-p$, q' and $-p'$, and with coefficients

$$\begin{array}{llll} r, & s, & t, & \\ a, & b, & c, & d, \\ \cdot\ \cdot\ \cdot & & & \end{array} \qquad \begin{array}{llll} r', & s', & t', & \\ a', & b', & c', & d', \\ \cdot\ \cdot\ \cdot & & & \end{array}$$

the characteristic equations satisfied by their simultaneous concomitants are of the form

$$(\Delta_3 + \Delta'_3)\psi = 0 = (\Delta_4 + \Delta'_4)\psi,$$

that is,

$$\Theta_3\psi = 0 = \Theta_4\psi.$$

And every solution of these equations, with proper limitations as to degree and grade, is a concomitant. Hence, every functional invariant of the two dependent variables z and z' already considered can be expressed in terms of simultaneous concomitants of the set of quantities A_0, A'_0; A_1, A'_1; . . . , viewed as binary forms in variables q and $-p$, q' and $-p'$.

Thus, for example, we have seen the simultaneous functional invariants, proper to the rank 2, are five in number, and they are—one, J, being the covariant $pq' - p'q$ in the variables alone; two, A_0 and A'_0, being the quadratic forms; and two, $\mathfrak{A}_0$ and $\mathfrak{A}'_0$, which can be exhibited in the respective forms

$$\tfrac{1}{2}\left(q\frac{\partial}{\partial q'} + p\frac{\partial}{\partial p'}\right)^2 A'_0 + \left(q'\frac{\partial}{\partial q} + p'\frac{\partial}{\partial p}\right) A_0,$$

and

$$\tfrac{1}{2}\left(q'\frac{\partial}{\partial q} + p'\frac{\partial}{\partial p}\right)^2 A_0 + \left(q\frac{\partial}{\partial q'} + p\frac{\partial}{\partial p'}\right) A'_0,$$

which are combinations of polar emanants of A_0 and A'_0, the fundamental quadratic forms. And, from the note to § 24, it follows that they can also be represented in the forms

$$\left(q' \frac{\partial}{\partial q} + p' \frac{\partial}{\partial p} + r' \frac{\partial}{\partial r} + s' \frac{\partial}{\partial s} + t' \frac{\partial}{\partial t}\right) A_0,$$

$$\left(q \frac{\partial}{\partial q'} + p \frac{\partial}{\partial p'} + r \frac{\partial}{\partial r'} + s \frac{\partial}{\partial s'} + t \frac{\partial}{\partial t'}\right) A'_0,$$

and also in the forms

$$\tfrac{1}{2}\left(q \frac{\partial}{\partial q'} + p \frac{\partial}{\partial p'} + r \frac{\partial}{\partial r'} + s \frac{\partial}{\partial s'} + t \frac{\partial}{\partial t'}\right)^2 A'_0,$$

$$\tfrac{1}{2}\left(q' \frac{\partial}{\partial q} + p' \frac{\partial}{\partial p} + r' \frac{\partial}{\partial r} + s' \frac{\partial}{\partial s} + t' \frac{\partial}{\partial t}\right)^2 A_0.$$

36. Returning now to the functional invariants of only a single dependent variable, we have seen that they are combinations of the simultaneous covariants of A_0, A_1, A_2, . . . , considered as binary forms in q and $-p$; and all these simultaneous covariants satisfy the equations $\Delta_3 f = 0$, and must, therefore, be expressible in terms of $u_1, u_2, \ldots, u_5, \ldots, u_9, \ldots$. The actual expressions may be obtained as follows:—

From the values of the quantities u we have

$$\left\{\begin{aligned} t &= u_2, \\ u_1 s &= u_3 + p u_2, \\ u_1^2 r &= u_4 + 2p u_3 + p^2 u_2\,; \end{aligned}\right.$$

$$\left\{\begin{aligned} d &= u_5, \\ u_1 c &= -u_6 + p u_5, \\ u_1^2 b &= u_7 - 2p u_6 + p^2 u_5, \\ u_1^3 a &= -u_8 + 3p u_7 - 3p^2 u_6 + p^3 u_5\,; \end{aligned}\right.$$

$$\left\{\begin{aligned} i &= u_9, \\ u_1 h &= -u_{10} + p u_9, \\ u_1^2 g &= u_{11} - 2p u_{10} + p^2 u_9, \\ u_1^3 f &= -u_{12} + 3p u_{11} - 3p^2 u_{10} + p^3 u_9, \\ u_1^4 e &= u_{13} - 4p u_{12} + 6p^2 u_{11} - 4p^3 u_{10} + p^4 u_{10}\,; \end{aligned}\right.$$

and so on. It thus appears that any differential coefficient of z, when multiplied by a power of u_1 equal to the x-grade of the differential coefficient, is linearly expressible in terms of the quantities u proper to its rank, the coefficients of these quantities u in the expression being powers of u.

But in the case of the function A_{n-2}, which is

$$(z_{n,0},\ z_{n-1,1},\ z_{n-2,2},\ \ldots \between q,\ -p)^n,$$

the weights of its concomitants are estimated by assigning to $z_{n,0}$, $z_{n-1,1}$, $z_{n-2,2}$, . . . the weights 0, 1, 2, . . . in succession, that is, the integers which represent the y-grade of these coefficients $z_{n-s,s}$.

If we have a covariant Ψ of order m which is simultaneous to A_{n-2}, $A_{n'-2}$, $A_{n''-2}$, . . . , and of degrees l, l', l'', . . . in their respective coefficients, and its leading term be $C_0 q^m$, then the weight of C_0 is

$$\tfrac{1}{2}(nl + n'l' + n''l'' + \ldots - m),$$

which is, therefore, the number representing the y-grade of C_0, considered as the leading term of a functional invariant. Since the grade of each of the coefficients of A_{n-2} is n, it follows that the grade of C_0, so far as it involves the coefficients of A_{n-2} is ln, and, therefore, the grade of C_0 is, in the aggregate,

$$nl + n'l' + n''l'' + \ldots .$$

But the aggregate grade of C_0 is the sum of the x-grade and the y-grade; hence, the x-grade of C_0 is

$$\tfrac{1}{2}(nl + n'l' + n''l'' + \ldots + m).$$

In order, then, to express Ψ in terms of the quantities u, we should proceed to substitute for the coefficients r, s, t, . . . the values above obtained, and assuming

$$\Psi = C_0 q^m - C_1 q^{m-1} p + \ldots ,$$

it is evident that the only term in Ψ from which terms independent of p can come is the first term. Moreover, since Ψ is expressible as a function of the quantities u alone, it follows that when these substitutions are carried out the terms involving p must disappear, for p is the only non-u quantity which enters into the expressions substituted; and the value of Ψ is, therefore, the aggregate of terms which survive, that is, the aggregate of terms independent of p arising from $C_0 q^m$.

Now, in C_0 this aggregate is obtained by replacing a coefficient, $z_{m,n}$, by a quantity $\pm u_0 u_1^{-m}$; since C_0 is isobaric *qua* seminvariant, it is of uniform x-grade *qua* part of functional invariant; and therefore the result of these substitutions is to give a function of u_2, u_3, u_4, . . . , divided by a power of u_1 equal to the x-grade of C_0, that is, divided by

$$u_1^{\frac{1}{2}(nl + n'l' + n''l'' + \ldots + m)}.$$

If, then, Γ_0 denote this function of u_2, u_3, u_4, . . . , we have

$$\Psi = u_1^m \,.\, u_1^{-\frac{1}{2}(nl + n'l' + n''l'' + \ldots + m)},$$

or

$$u_1^{\frac{1}{2}(nl + n'l' + n''l'' + \ldots - m)} \Psi = \Gamma_0.$$

37. Hence we have the following theorem:

To express any simultaneous concomitant Ψ of

$$(r, s, t \between q, -p)^2,$$
$$(a, b, c, d \between q, -p)^3,$$
$$(e, f, k, h, i \between q, -p)^4,$$
$$\cdots\cdots\cdots$$

in terms of the quantities $u_1, u_2, u_3, u_4, \ldots$, which are the irreducible solutions of $\Delta_3 f = 0$, the equation characteristic of all these concomitants, it is sufficient to take the coefficient C_0 of the highest power of q in Ψ, to construct a new function Γ_0, which is the same combination of the coefficients of

$$(u_4, u_3, u_2 \between q, -p)^2,$$
$$(-u_8, u_7, -u_6, u_5 \between q, -p)^3,$$
$$(u_{13}, -u_{12}, u_{11}, -u_{10}, u_9 \between q, -p)^4,$$
$$\cdots\cdots\cdots\cdots$$

as C_0 is of the coefficients of the binary quantics, and to divide Γ_0 by $u_1^{\frac{1}{2}(nl+n'l'+n''l''+\ldots-m)}$, where m is the degree of Ψ in q and $-p$, and $l, l', l'', \ldots$ are the degrees of C_0 in the coefficients of $A_{n-2}, A_{n'-2}, A_{n''-2}, \ldots$ respectively.

The theorem is illustrated by one or two examples which have already occurred in the reduction of Q_5, Q_6, Q_7. Thus, for $H_0 = rt - s^2$, we have only one quantic entering into its composition, viz., A_0; so that $n = 2$, $l = 2$; $l' = 0 = l'' \ldots$, and $m = 0$ hence,

$$u_1^{\frac{1}{2}\cdot 2\cdot 2} H_0 = u_4 u_2 - u_3^2,$$

that is,

$$H_0 = (u_4 u_2 - u_3^2) u_1^{-2}.$$

Again

$$-\tfrac{1}{12} H_{01} = r(pd - qc) - 2s(pc - qb) + t(pb - qa)$$
$$= q(-cr + 2bs - at) + p(dr - 2cs + bt);$$

so that two quantics enter. Thus, we have, for H_{01},

$$n = 2, \quad l = 1;$$
$$n' = 3, \quad l' = 1; \qquad l'' = l''' = \ldots = 0;$$
$$m = 1.$$

Hence

$$-\tfrac{1}{12} u_1^{\frac{1}{2}(2\cdot 1 + 3\cdot 1 - 1)} H_{01} = -u_4(-u_6) + 2u_7 u_3 - u_2(-u_8)$$
$$= u_4 u_6 + 2u_3 u_7 + u_2 u_8$$

as before; and so for others.

38. We might, if we pleased, carry the theorem further, for every simultaneous concomitant satisfies the two characteristic equations $\Delta_3 f = 0$, $\Delta_4 f = 0$, and is therefore expressible in terms of the simultaneous irreducible solutions of these two equations. Such irreducible solutions are necessarily functional combinations of $u_1, u_2, u_3, \ldots$ such as satisfy $\Delta_4 f = 0$; and, as in the earlier cases of §§ 14, 17, it is easy to show that these irreducible solutions within the rank three are equivalent to the set

$$\begin{aligned} u_4 &= A_0, \\ (u_2 u_4 - u_3^2)\, u_1^{-2} &= H_0, \\ u_8 &= -A_1, \\ (u_4 u_7 + u_3 u_8)\, u_1^{-1} &= J_{01}, \quad \text{(dropping a factor 6)} \\ (u_6 u_8 - u_7^2)\, u_1^{-2} &= H_1, \\ (u_5 u_8^2 - 3u_6 u_7 u_8 + 2u_7^3)\, u_1^{-3} &= Q, \end{aligned}$$

which are f, A_{ff}, ϕ, ϑ, Δ, Q respectively in GORDAN'S notation. Thus, every simultaneous concomitant within the rank three can be expressed algebraically—though not necessarily rationally—in terms of these six quantities.

The actual expression can be obtained by a development of the method adopted in the preceding theorem. It is first necessary to replace the covariant by its value in terms of $u_1, u_2, u_3, \ldots$; then to substitute by means of the equations

$$\begin{aligned} u_4 &= A_0, \\ u_2 u_4 &= u_3^2 + u_1^2 H_0, \\ u_8 &= A_1, \\ u_4 u_7 &= u_1 J_{01} + u_3 A_1, \\ u_4^2 u_6 u_8 &= u_1^2 u_4^2 H_1 + (u_1 J_{01} + u_3 A_1)^2, \\ u_5 u_4^3 u_8^2 &= u_1^3 u_4^3 Q + (u_1 J_{01} + u_3 A_1)^3 + 3u_1^2 u_4^2 H_1 (u_1 J_{01} + u_3 A_1), \end{aligned}$$

for $u_2, u_4, u_5, u_6, u_7, u_8$. The result, we know, must appear as a function of A_0, H_0, A_1, J_{01}, H_1, Q; and, therefore, the terms involving u_3 will disappear, and the factors u_1 will cancel.

For example, in the case of H_{01}, $= p$ (GORDAN) $= L_1$ (SALMON), we have, dropping the factor 12 and using L_1, which is $\frac{1}{2} H_{01}$,

$$\begin{aligned} u_1^2 L_1 &= -u_2 u_8 - 2u_3 u_7 - u_4 u_6 \\ &= \frac{A_1}{A_0}(u_3^2 + u_1^2 H_0) - 2\frac{u_3}{A_0}(u_1 J_{01} + u_3 A_1) + \frac{1}{A_0 A_1}\{u_1^2 A_0^2 H_1 + (u_1 J_{01} + u_3 A_1)^2\} \\ &= \left(\frac{A_1}{A_0} H_0 + \frac{A_0}{A_1} H_1 + \frac{J_{01}^2}{A_0 A_1}\right); \end{aligned}$$

so that

$$L_1 A_0 A_1 = A_1^2 H_0 + A_0^2 H_1 + J_{01}^2.$$

Similarly for others of the simultaneous concomitants of A_0 and A_1. And it is not difficult to show that all functional invariants within the rank 4, or, what is the equivalent, all the simultaneous concomitants of A_0, A_1, A_2, considered as three binary forms, can be expressed in terms of the foregoing six quantities A_0, H_0, A_1, J_{01}, H_1, Q, and the succeeding five, viz.:—

$$
\begin{aligned}
u_{13} &= A_2, \\
(u_{12}u_4 + u_3u_{13})\, u_1^{-1} &= J_{02}, \\
(u_{11}u_{13} - u_{12}^2)\, u_1^{-2} &= H_2, \\
(u_{10}u_{13}^2 - 3u_{11}u_{12}u_{13} + 2u_{12}^3)\, u_1^{-3} &= \Phi_2, \\
(u_9u_{13} - 4u_{10}u_{12} + 3u_{11}^2)\, u_1^{-4} &= I_2.
\end{aligned}
$$

Inferences can also be deduced as to the expressibility of the simultaneous concomitants of A_0 and A_2 alone as simultaneous quantics, and of the simultaneous concomitants of A_1 and A_3 alone, as simultaneous concomitants; but all such results are chiefly interesting from the point of view of the theory of binary forms, and are more useful in that theory than in the theory of functional invariants.

III. *Total Eclipse of the Sun observed at Caroline Island, on 6th May,* 1883.

By Captain W. DE W. ABNEY, *C.B., R.E., F.R.S.*

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[PLATES 1, 2.]

OWING to the representations of the Committee on Solar Physics, who communicated with the Royal Society the desirability of observing this eclipse, an expedition was organised under the auspices of the latter body. The Council of the Royal Society having requested me to draw up a report on the Total Eclipse observed at Caroline Island, I undertook the task so far as relates to the results which were obtained with the same instruments which were employed in the observations of the Total Eclipse in Egypt in 1882.

Two observers, Mr. H. LAWRANCE and Mr. C. R. WOODS, who had both taken part in the Eclipse Expedition to Egypt as assistants to Professors LOCKYER and SCHUSTER, were entrusted with the arduous duty of making the observations. The expedition was devoted entirely to photographic work, the main object being to continue the photographic observations which had been carried on in Egypt, consisting of photographs of the corona taken on very rapid plates with varying exposure, photographs of the corona taken with a slitless spectroscope (the prismatic camera), and a photograph of the corona spectrum, the image of the moon and the corona being thrown on the slit cutting the diameter of the former. There is no occasion to describe the instruments which were employed for the first two classes of observations, as they have been fully described in the previous communication to the Royal Society by Professor SCHUSTER and myself which appears in the 'Philosophical Transactions' for 1884. The photographic spectroscope which was employed on this occasion differed in one detail, and in one detail only, in that the dispersion was doubled, two medium dense flint prisms of $62\frac{1}{2}^{\circ}$ being employed instead of one prism of the same angle. The experience gained in Egypt seemed to show that, if the coronal light was equally bright in the two eclipses, the rapid plates used on both occasions would be amply adequate to secure photographs with the larger dispersion. Besides these observations several others were made, but did not meet with the success it was hoped they would have done. A photoheliograph, giving a 4-inch solar image, was attached to an equatorial mount, in addition to the wooden camera carrying a lens of 5 ft. 6 in. focus, with which the smaller-sized pictures of the corona were taken in Egypt. The pictures taken with the former, though sufficiently exposed, showed that a large image could be utilised. They were not, however, satisfactory, owing to a multiplicity of images being formed, due to the shake given to the instrument by the insertion of the slides in the smaller instrument, the large pictures requiring considerably more exposure than these latter. In the matter of spectroscopic analysis of the eclipse phenomena, Mr. LOCKYER devised an ingenious contrivance for securing impressions of the bright lines seen immediately after and before totality. These photographs were only partially successful, and will not be considered in this report. The number of instruments to be used by the two observers and the assistants they hoped to obtain were nine, entailing the use of 11 cameras. Only two equatorial mountings accompanied the expedition, and it was impossible to mount all these instruments on them, had it been advisable, indeed, to do so. A siderostat, having a 12-inch silver-on-glass mirror, was therefore taken, four of the instruments being stationary, reflected light being utilised.

The following was the disposition of the instruments :—

On the 1st equatorial—

A. A finder of $3\frac{3}{4}$-inch aperture was attached to the above for viewing the eclipse.

B. A 7-prism spectroscope, with camera attached, for obtaining photographs near the sun's limb immediately before and after totality.

C. A 6-inch achromatic telescope by COOKE, of York, the eye-piece being withdrawn. Attached to it was a RUTHERFURD grating of 17,200 lines to the inch, to be used for obtaining spectra of the corona in the 1st and 2nd order, two cameras being employed.

D. A slit spectroscope, having one prism of dense flint glass. The condenser throwing the image of the moon on the slit was a photographic lens by DALLMEYER, of 6-inch focus.

On the 2nd equatorial were mounted—

M. The photoheliograph for taking 4-inch pictures.

N. The corona camera, having a lens of 4-inch aperture, and 5 ft. 6 in. focal length.

The instruments used with the siderostat were—

F. A photographic spectroscope to be used without a condenser, consisting of one prism of white flint, a collimator $4\frac{1}{2}$ feet long, and a lens attached to the camera of 3-inch aperture, and of about 9-inch focus. In this case the photographic plate was caused to move vertically during exposure of the plate by means of clock-work for the registration of bright lines immediately before and after totality.

G. A slit spectroscope of two prisms of the same dimensions as that used in Egypt in the eclipse of 1882, and described in Dr. SCHUSTER's and my report in the 'Phil. Trans.,' 1884. The whole apparatus was the same as that described in that paper, with the exception that the two prisms were employed instead of one. The use to which this instrument was to be put has already been referred to.

H. The prismatic camera also described in the paper just referred to.

K and L. A concave ROWLAND grating of 5 feet focus arranged for taking ring spectra in the 1st and 2nd orders.

(The same letters are attached to the above as are to be found in Appendix II. in the instructions for adjustment drawn up by Mr. LOCKYER.)

The time table of exposures is given in Appendix III., and the times indicated were very closely followed.

The party was attached to the American Expedition under the command of Professor HOLDEN, arrangements to this effect having been made by the President of the Royal Society. The instructions issued to them will be found in Appendix I. The combined parties were taken from Panama in a United States man-of-war, and landed on Caroline Island on April 20th. The instruments were ready for use on the 3rd May. Owing to bad weather it appears that the trial of the instruments was much impeded, but that they were in fair working order by the 6th, the day of the eclipse. The instruments were packed up on the 7th May and two following days, and the party left the island on the 9th.

The following are the notes made by Messrs. WOODS and LAWRANCE regarding the atmospheric conditions immediately after the eclipse :—

11.5—Fleecy clouds over sun.
11.13—Birds flocking in air; light greyish.
11.15—Fleecy clouds over sun.
11.20— ,, ,, ,,
11.30—Totality commenced ; sky very cloudy.
4 minutes before totality—BAILEY's beads visible.
2.45—Totality; BAILEY's beads more plainly seen.
5 minutes after totality—Sky clouded over.

They described the corona on the following limb as being very full of detail, with many curved rays. Shortly after totality they saw the 1474 line, with a pocket spectroscope having a condensing lens and slit. Taking off the slit they saw as rings 1474, D_3, and C, D_3 and C being very faint. In mid totality they only saw the 1474 line very bright on the west side.

At end of totality the structure on the preceding limb is described by them as most beautiful, exceeding the other side in detail. In the spectroscope they saw the same rings, but the 1474 line by far the brightest. The spectrum of the corona during totality, when viewed with a pocket spectroscope, appeared continuous and bright. The light was nearly as bright as in Egypt. The corona extended to $2\frac{1}{2}$ diameters, and strongly resembled that of 1882. Mr. WOODS states that the coronal light was more natural than in Egypt, and Mr. LAWRANCE describes it as not so violet as in Egypt.

Results.—Although photographs were taken successfully in nearly every instrument, it is to be regretted that the majority have so far proved to be of but little use. At present I have not been able to utilise for measurement more than the photograph of the spectrum of the corona taken with the two-prism slit spectroscope, and the corona photographs taken in the camera with the lens of 5ft. 6in. focus. These last had exposures given of 1 sec., 2 secs., 3 secs., 10 secs., 20 secs., and 120 secs.

The photographs taken with the slitless spectroscopes are good, but they possess no great features of interest. The prominences were of small height and few in number, and I have been unable to mark any distinction in the light they emitted. The rings of light due to 1474, D_3, and other substances which were noticed in the eclipse of 1882 are absent, probably because of the greater angular diameter of the moon. I have, therefore, not given either drawings or measurements of these photographs.

The negatives of the corona were placed at my request in the hands of Mr. WESLEY, Assistant Secretary of the Royal Astronomical Society, and he has made two drawings from them, in one of which the coronal detail near the limb is shown, being taken from the photographs which had but short exposure, and in the other the coronal detail further from the sun, being sketched from the photographs to which long exposure had been given. The general features of the corona are those which might be expected from the sun-spot period in which the eclipse took place, a matter which was discussed in the Report of the Egyptian Eclipse, and which scarcely need be restated here. The corona spectrum has been carefully measured by Mr. LAWRANCE and myself. The method we adopted was as follows:—First, I took some measurements of the most prominent lines and recorded them, taking out the wave-lengths by the same method employed in measuring the photograph in the Egyptian eclipse, the reference spectra taken after the eclipse on the same plate being utilised for the purpose. Mr. LAWRANCE then carefully and independently measured the photograph three separate times. All lines were rejected which he did not measure in all these sets of measurements. I then measured it myself in the same way, and rejected all lines which did not appear in each of my measures. Finally, the lines taken as absolutely present were those which appeared in my expurgated measures and in Mr. LAWRANCE'S. By this means it is believed that every line of which there can be no doubt has been recorded, whilst there are many others whose existence is doubtful but which are probably present. Lists of each sets of measures are given, which may be useful in comparing the lines obtained in this photograph with the Egyptian negatives, and those which may be obtained in future eclipses. That a large number are coronal lines is a fact, and the coincidences between those found in the photograph now under discussion and the Egyptian one, in which all the lines given were undoubtedly coronal, is important. It will be better in future eclipse expeditions to place the slit of the spectroscope tangential to the moon's limb in preference to normally. This has been done in the recent eclipse observed in the West Indies (August, 1886), with most satisfactory results. The chief point to attain is to separate all prominence light from the coronal light, as it tends to mask the true spectrum of the latter. From the photographs I have examined I have come to the conclusion that not much more is to be learnt at present from them. It may be that as more eclipses come to be observed with the same instruments, or at all events on the same lines, the photographs of the Caroline Island station will prove to be of greater value than they seem to be now.

If we compare the corona of this eclipse with that of the eclipse in Egypt, perhaps the most striking feature is the absence of the hydrogen lines. In Egypt the photograph shows, besides the lines which may be presumed to be hydrogen at H, at least two other lines of hydrogen, λ 4340 and λ 4101. In the Caroline Island photographs these lines are entirely absent. It may be well to draw attention to the fact that in the former eclipse the prominences were very marked, and in the prismatic (slitless) spectrum the hydrogen rings were very powerfully shown. In the eclipse now under consideration the prominences were very small, and the prismatic (slitless) spectrum gave no result other than rings at H and K. It would seem, then, that the corona at the time of the Egyptian eclipse was illuminated more or less by the prominence light. If this be admitted, we ought to find that the corona during the Caroline Island eclipse was illuminated by the light which emanated from the matter which gave H and K so strongly in the ring spectrum. Looking at the list of lines, we find that such is the case. Calcium was evidently present in the light, more especially near the limb of the moon. We find that three calcium lines are shown *reversed* across the dark moon, and two iron lines. It is somewhat hard to see how these reversed lines made their appearance in such a locality. It is quite evident that they must be due to reflected light. I can find no trace of FRAUNHOFER lines about G outside the corona, such as Dr. SCHUSTER and myself found in the Egyptian eclipse photograph, and which would be the first to appear in the photographic plate were any reflected sunlight as it reaches us present in those regions. It should be remarked that the reversed lines across the moon are extremely faint, but perfectly distinguishable and measurable. Most of the lines in the spectrum of the corona lie near the moon's limb, and have quite a different aspect to those delineated in the Egyptian eclipse negative, and some of them are probably prominence lines, and I think it would be dangerous to found any theory on the discovery of new lines in the coronal spectrum from the list of lines here recorded.

In conclusion, I think I may say that the two English observers, Mr. H. A. LAWRANCE and Mr. C. R. WOODS, deserve every credit for the amount of work they did. The large number of instruments they were called upon to utilise during the eclipse, and which they evidently most skilfully manipulated, could only have been done by those who were thoroughly competent, and who possessed a freedom from a tendency to excitement, which occasions such as that on which they were engaged is apt to create, more especially when they have a heavy responsibility resting upon them. The results they brought home show how assiduously they worked, and how completely they carried out the programme with which they were entrusted.

Corona Spectrum, 1883.

Abney's measures, in wave-lengths.		Lawrance's measures, in wave-lengths.		Adopted lines.	Remarks.
1	2	1	2		
3837	3837	3836±2	3835	3836	
		3883			
3898		3898			
3934	3934	3934	3934	3934	K reversed across moon Fe Ca
	3954				
3969	3969	3969	3969	3969	H reversed across moon Fe Ca
3986		3986			
3998		3997		3998	Reversed across moon Fe
4015	4016	4014	4018	4016	E 4015
		4030	4031		
		4038	4036		
4045				4045	E 4044
4057		4055		4056	E 4057
4065	4065		4063	4064	E 4067
4071	4071			4071	Reversed across moon Fe
	4076		4074	4075	Ca (4077)
4081		4081			
4086		4085		4085	E 4085
4092	4092		4094	4092	Reversed across moon Ca
			4113		
			4125		
4131				4131	Reversed across moon Ca Fe
4137	4142	4142	4146	4144	
		4153	4158		
			4169	4169	E 4168 Ca 4167
4184	4185	4183	4187	4185	
		4191	4194	4192	E 4195
		4213		4213	E 4213 Ca Fe Sr (4215·5)
		4220			
4225	4227		4228	4227	E 4224 Ca Sr (4226·3)
			4237	4242	E 4241 Fe 4235·5
		4248	4246		Fe 4245·2
4253	4249	4254	4255	4255	E 4252
		4261			
		4274±2			
4279	4279		4280	4279	
	4290	4291		4291	E 4289 Ca Cr Ce 4289·4
4310		4310			
	4313		4314		
	4331		4329		
4354	4354	4351	4353	4353	Fe 5352
		4360	4358		
4370	4370		4363±3	4370	E 4370

CORONA Spectrum, 1883 (continued).

ABNEY'S measures, in wave-lengths.		LAWRANCE'S measures, in wave-lengths.		Adopted lines.	Remarks.
1	2	1	2		
		4377			
		4383			
		4400			
		4427			
4450		4448			
4464		4465			
4473		4473		4473	E 4473 4471·4 often in prominences
4490		4490			
4501		4502		4501	E 4501
	4518		4518		
4545		4546±1			
		4557	4555±3		
		4571			
		4577			
4602		4606			
4620	4636		4636		
		4642			
	4672		4674		
		4695			
			4706		
4720		4717	4717	4717	Fe 4717
4730	4730	4729±1		4730	Fe 4730·7
			4738		
4754		4754			
			4760		
			4764		
4780		4776			
		4798			
			4803		
			4818		
		4845			
	4949		4955		

E signifies lines found in the photograph of the corona spectrum taken in Egypt, 1882.
In the column marked 1 the lines were found in three different measurements.
In the column marked 2 the lines were found in two different measurements.

In the adopted spectrum only those lines which were each measured by the two computers on each limb of the moon have been taken as coronal, unless a coincidence was noted between lines measured on one and the coronal spectrum of 1882 taken in Egypt. It will be noted that lines occurring in Ca and Fe lie very close to those given in the adopted spectrum.

Appendix I.

Government Eclipse Expedition, 1883.

Instructions to Observers.

1. In case of any difficulty at any port, either on going out or coming home, Mr. Lawrance to hand Foreign Office letter herewith to the British Consul at that port, and ask his assistance.

2. On joining the American party, Mr. Lawrance and Mr. Woods to report themselves to the astronomer in charge of the expedition, and to hand him the accompanying letter, taking his advice and following his instructions with reference to the transference of the instruments to the United States ship of war.

3. On arriving at the place of observation, the instruments to be erected on a site to be chosen by the American astronomer in charge.

4. Packing cases to be re-closed up as far as possible, and to be protected from damage and the weather. Care to be taken not to damage tin cases.

5. The gratings to be kept together, and special precautions to be taken with regard to them, as also with the silvering of the siderostat mirrors. Mr. Lawrance to give special attention to this point.

6. For as many days as possible before the eclipse all the instruments to be arranged as during the eclipse, and from 11.23 A.M., local mean time, to 11.48 A.M., local mean time, complete rehearsals of all the observations intended to be made during the eclipse to be most rigidly carried out.

7. A statement of the days on which these rehearsals have been made to be given in the report of the operations.

8. If the aforesaid times, derived from Mr. Hind, do not agree with the times determined by the American astronomers, the instructions of the astronomer in charge are to be taken.

9. Instruments to be focussed and trial plates taken, if possible, at least three days before totality. These trial plates to be carefully preserved.

10. The rehearsal on the day before the eclipse should be a complete rehearsal with photographic plates, exactly as during the eclipse itself; and these plates to be developed at once, and brought home.

11. The observers should confer with the American astronomer in charge regarding time signals before and after totality.

12. If additional observing power can be obtained from the American party, the additional observers to be trained to obtain photographs with the photoheliographs, and, if desirable, the time table for that instrument to be handed over to them, they being placed in entire charge of that part of the operations.

13. If such assistance cannot be afforded, then, if the photoheliograph programme cannot be carried out in its entirety, the large pictures to be alone attempted.

14. Special attention to the rating of the clocks, including the eclipse clock and siderostat, to be given at least three days before the eclipse.

15. A quarter of an hour before totality, clocks to be wound, and caps and stops which had been hitherto used to diminish the amount of light to be removed, if necessary.

16. The timekeeper should be asked to give these instructions in a loud voice, as experience has shown that this is apt to be forgotten.

17. In the observations and adjustments during the eclipse, no deviation from the time table and adjustments to be made except after consultation, and with the approval of the American astronomer in charge.

18. The clockwork of the integrating spectroscope to be so adjusted that the plate will fall through one inch in eight minutes.

19. The distance of plate from concave grating to be that given by Captain ABNEY for vertical distortion.

20. In equatorial, the slits to be parallel and vertical in the meridian, and their centres lying on the same part of the sun.

21. All slits to be $\frac{1}{500}$ in. = No. 2 on Captain ABNEY's screw, with the exception of the integrating spectroscope, which should be $\frac{1}{250}$ in.

22. At some convenient time—say 100 secs.—near the middle of totality, the slits of equatorial to be brought to the point of reappearance.

23. The plates to be developed and copied at the first convenient time after the eclipse is over.

24. Half the positives and half the negatives to be handed to the British Consul at Callao, to be forwarded to the Foreign Office for transmission to the Science and Art Department by the next mail after that by which the observers leave.

25. On arrival at Callao, a cypher telegram to be despatched to Secretary, Kensington Museum, London, giving the results obtained with each instrument, and stating any other matter of importance.

26. Great care to be taken in repacking the instruments after the eclipse. Tin cases to be re-closed.

27. A detailed report, to be prepared before arrival at Callao, of the general results to be posted to me immediately on arrival at Callao, in case of any delay *en route*.

28. If a convenient opportunity arises for sending this report from the Marquesas, this course to be followed as well as the other.

29. It is to be understood that the records of the eclipse are the property of the British Government.

30. In case no pictures are taken with the small photoheliograph, Mr. LAWRANCE is requested to ask the American astronomer in charge for an oriented positive of the corona to facilitate reference here.

31. Mr. LAWRANCE is empowered to hand to the American astronomer in charge positives of any of the pictures taken by the English party which he may require for a similar purpose, and to obtain a receipt for them.

W. SPOTTISWOODE, Pres. R.S.,
Feb. 16, 1883.

J. NORMAN LOCKYER,
Feb. 16, 1883.

APPENDIX II.

Adjustments.

B. Seven-prism spectroscope.
F line in centre of plate.
C. Flat grating spectroscope.
First order—F in centre of plate.
Second order—F in centre of plate.
D. Dense prism.
F in centre of plate.
F. Integrating HILGER (Flash).
G in centre of plate.
G. Red end slit.
H. Red end prismatic camera.
K. First order blue. ROWLAND.
F in centre of plate.
L. Second order blue. ROWLAND.
H in centre of plate.
M. 4″ photoheliograph.
See that sun runs along horizontal wire.
N. Small photoheliograph.

J. NORMAN LOCKYER,
Feb. 16, 1883.

APPENDIX III.

Time Table.

Time.	Siderostat.					Equatorial.				Photoheliographs.	
	F	K	L	H	G	B	C		D	M	N
	HILGER.	ROWLAND grating.		Prismatic camera.	Slit spectroscope. 2 prisms.	7 prisms.	Grating.		Dense prism. Quarter-inch slide.	Large photo-heliograph.	Corona camera.
		1st order.	2nd order.				Red, 1st order.	Blue, 2nd order.			
10 min.	..	..	..	..	..	Expose	Expose	Expose			
9											
8	..	..	..	..	..	..	..	..	Expose		
7	..	..	..	..	..	..	..	..	Expose		
6	Ref. spectrum 30 secs.	..	..	..	..	..	..	..	Expose		
5	..	..	..	..	..	..	..	..	Expose		
4	..	..	..	..	..	..	..	..	Expose		
3	..	..	..	..	..	..	Expose	Expose	Expose		
2	..	..	..	..	..	Expose	..	..	Expose		
1	..	..	..	..	..	..	..	..	Expose		
40 secs.	..	Expose	Expose	..	..	..	Run				
30	..	..	..	..	..	..	$\frac{1}{4}$ inch				
20	..	Expose	Expose								
3											
2	Expose and start clock										
1											
Totality	..	Expose	Expose	Expose col. plate	Expose	Expose	Expose	Expose	Expose	Expose	Expose 1 sec.
300 secs.											
290											
280	..	..	..	..	..	..	..	..	..	..	Expose 20 secs.
270											
260											
250											
240											
230	..	..	..	Shut							

APPENDIX III. (continued).

Time Table (continued).

Time.	Siderostat. F Hilger.	Siderostat. K Rowland grating. 1st order.	Siderostat. L Rowland grating. 2nd order.	Siderostat. H Prismatic camera.	Siderostat. G Slit spectroscope. 2 prisms.	Equatorial. B 7 prisms.	Equatorial. C Grating. Red, 1st order.	Equatorial. C Grating. Blue, 2nd order.	Equatorial. D Dense prism. Quarter-inch slide.	Photoheliographs. M Large photo-heliograph.	Photoheliographs. N Corona camera.
220	..	..	..	Expose gel. plate							
210	..	..	..	..	..	..	..	..	..	..	Expose
200	..	Expose	Expose	..	..	..	..	..	Expose	Expose	
190											
180											
170											
160											
150											
140											
130											
120	..	..	..	Shut							
110	..	..	..	Expose col. plate							
100											
90	..	..	..	..	..	..	..	..	..	..	Shut
80											
70	..	..	..	..	..	..	..	..	..	..	Expose 3 secs.
60											
50	..	..	..	..	..	..	..	..	..	Expose	
40 + x	..	..	..	..	..	..	..	..	..	..	Expose 10 secs.
30 + x											
20 + x	..	..	..	..	..	..	..	..	..	..	Expose 2 secs.
10 + x											
Just before end	..	Expose	Expose	Shut	Shut	Expose	Expose, run $\frac{1}{4}$ inch	Expose	Expose	Shut	

APPENDIX III. (continued).

Time Table (continued).

Time.	Siderostat.					Equatorial.				Photoheliographs.	
	F HILGER.	K ROWLAND grating. 1st order.	L ROWLAND grating. 2nd order.	H Prismatic camera.	G Slit spectroscope. 2 prisms.	B 7 prisms.	C Grating. Red, 1st order.	C Grating. Blue, 2nd order.	D Dense prism. Quarter-inch slide.	M Large photo-heliograph.	N Corona camera.
1 sec.											
2											
3											
4	..	Expose	Expose								
5											
10	..	Expose	Expose								
15											
20											
30											
40											
50											
1 min.	..	Shut	Shut	..	..	..	Expose	Expose	Expose		
2	Shut										
3	..	..	..	..	..	..	..	..	Expose		
4											
5	..	..	..	..	..	Expose	Expose	Expose	Expose		
6											
7	..	..	..	..	..	..	..	..	Expose		
8											
9	..	..	..	..	..	..	..	..	Expose		
10	Ref. 25 secs.	..	..	..	..	Shut	Shut	Shut	Shut		
Ref. spectra, say half hour after totality	1 sec.	..	..	..	2 secs.	10 secs.	10 secs.	10 secs.	1 sec.	Run	Run

J. NORMAN LOCKYER,
Feb. 16, 1883.

The following Report was written from U.S.S. "Hartford," at sea.

April 20th, at 7 o'clock in the morning, we came in sight of Caroline Island.

A boat was sent off under Lieutenant QUALTROUGH, and on his return we learnt that there were two empty frame-houses belonging to Messrs. HOLDER BROS., of Leadenhall Street, to whom the island is leased, and seven native inhabitants.

The disembarkation commenced that afternoon, and was concluded next day; but, as at nightfall a large part of the goods were still on the shore, the "Hartford" lay-by all night, and landed strong parties at daylight, to carry the boxes up to the site chosen by Professor HOLDEN for the observatory, while W. M. PEACOCK, the cooper, put in the foundations for our three piers.

The landing was very difficult, as the boats had to be run in through a narrow opening in the reef; then the boxes had to be carried through fifty yards or so of water, varying from two to three feet deep; then over fifty yards of sharp irregular coral rock, that cut the men's shoes to pieces; and then along a soft sandy beach, up hill, for more than a quarter of a mile. Our best and most hearty thanks are due to Captain C. C. CARPENTER, who superintended the disembarkation; to Lieutenant-Commander E. WHITE, the executive officer, who saw personally to the lading of the boats; to Lieutenant-Commander J. W. MILLER, who received the goods on shore; to Lieutenant QUALTROUGH, the Cadets, and Warrant Officers, who looked after the working parties on shore.

The "Éclaireur" came in on the evening of the 22nd, just as the "Hartford" was leaving, with the French expedition, consisting of Messrs. JANSSEN, TROUVELOT, PALISA, and TACCHINI.

The landing party left with us consisted of—

Lieutenant EDWARD F. QUALTROUGH.
WILLIAM S. DIXON, Esq., M.D.
Cadet, W. B. FLETCHER.
Cadet, J. G. DOYLE.
Seaman-Gunner, H. R. YEWELL.
Carpenter, PETER MURPHY.
Carpenter's Mate, CHARLES EMMS.
Seaman, JAMES HAROLD.
O. Seaman, JOHN MACKINNON.
O. Seaman, C. H. PERKINS.
O. Seaman, J. SMITH (Cook).
Steward, P. BURNS.
Servant, T. BROOKS.
Servant, MORTIMER SPENCE.

By Saturday, the 28th of April, the siderostat, equatorial, and photoheliograph were erected and adjusted in position. The arrangement of the nest of spectroscopes for use with the siderostat was taken in hand, and the spectroscopes were attached to the equatorial.

We had a great deal of trouble with the photoheliograph, as the tube did not fit the cradle; the clock went badly, and the square box could not be perfectly adjusted for parallelism.

By Thursday evening, the 3rd of May, we were nearly ready for trial plates, which we hoped to take the following day; but it turned wet, and before noon on Friday over five inches of rain had fallen, and our dark room was destroyed, all the dye being washed out of our ruby curtain and window.

The early part of the week was taken up in arranging the various spectroscopes, which took up a good deal of time, and in rating the clockwork slide and equatorial and photoheliograph clocks.

At last the latter went fairly well, but that of the equatorial could not be made to go fast enough, so that recourse had to be made to the fine motions.

On the day previous to the eclipse the weather was very unsettled, and the rehearsals and final adjustments occupied so much time that we were unable to take trial plates.

The photoheliograph stand vibrated so badly that to steady it two cords were attached to the end of the polar axis and fastened to stakes driven in the ground.

The weather on the 6th was very unsettled till about 9 o'clock, when the sky commenced to clear and the instruments were uncovered; by 10 o'clock the sky was moderately clear. After first contact the lenses were dusted, slits cleaned, and the adjustments inspected. Forty minutes before totality the plateholders which had been filled during the night were served out.

The following are the reports of each observer of the work done during totality:—

Mr. H. A. Lawrance's Report of work done during the Eclipse.

About 40 minutes before totality Mr. Woods gave me the plateholders, which I put into the cameras, and examined the screens to see that the three instruments were in good adjustment, then I moved the slides ready for exposure and wound the clock. The slits of the spectroscopes were parallel and nearly tangential to the point of disappearance.

I commenced to expose 10 minutes before totality, and followed the time table, with the exception that 100 seconds after totality I shifted to the other side of the sun and made a new exposure on each plate; after totality, by mistake, I shifted the grating plates at 3 instead of 5 minutes. I took reference spectra 25 minutes after totality.

The corona, examined through the finder, was full of delicate detail near the limb, especially upon the preceding one.

With a pocket spectroscope, with lens in front of the slit, I only saw the green line 1474; and, taking off the slit and examining with the prism at mid-totality, I saw the 1474 ring very brilliant, while C and D_3 were faint, with a lot of continuous spectrum. F I could not see, although I looked for it.

H. A. Lawrance.

Mr. C. R. Woods' Report.

The instruments under my charge were arranged as proposed in England, the integrating spectroscope, slit spectroscope, and prismatic camera being adjusted and focussed with F in the centre of the plate. The Rowland grating was placed normal with the siderostat mirror, and the first and second order on the brightest side adjusted, with F and H respectively in the centres of the plates. Some difficulty was experienced in getting the clockwork to move the slide of the integrating spectroscope sufficiently slow, as the desired rate of speed had been changed too late before starting to enable the alteration to be made at home; during the 8 minutes' run of the clock the plate was moved through the space of $1\frac{1}{4}$ inches.

Five minutes previous to totality the siderostat mirror was finally adjusted and the clock wound up. A red end collodion plate, coated 15 minutes before, was then washed and placed in one of the prismatic camera slides. All other slides had been filled the night previously with gelatine plates. At one minute before totality (not 2 seconds, as stated, I believe *erroneously*, in the instructions), the clockwork of the integrating spectroscope slide was started. At 40 seconds before, total exposures were made in the Rowland grating cameras. At totality, the prismatic camera and slit spectroscope were opened. The three exposures in the former instrument were performed as arranged, the last being closed 5 seconds after the lapse of 300 seconds. The slit spectroscope was closed at the end of the 300 seconds. The exposures in the Rowland grating were carried out strictly to programme, except as to the last exposure during totality, when, owing to longer totality than was expected, the plates were moved up between 10 and 15 seconds after the lapse of the 5 minutes. The clock of the integrating spectroscope ran down at about $1\frac{1}{2}$ minutes after totality, and the slit was covered over simultaneously with the stopping of the clock.

Several long intervals during exposures enabled me to look at the corona and my surroundings. The corona resembled that of 1882 in its general character, the streamers seeming to extend to a little over 2 diameters. Several stars were visible, but the amount of illumination of the sky seemed little less than that of the Egyptian eclipse; but, unlike the latter, its light was more natural, and the landscape lacked the weird colouring that was so noticeable during the eclipse last year.

Two minutes after totality I took the red end plate into the dark room to develop it. Having to manipulate it almost in the dark, it got torn in putting it in. On letting in orange light, half of it was still on the plate, but nothing appeared on that part, which, in spite of my utmost care, also tore into several pieces, leaving nothing on the plate save the gelatine edging.

Five minutes after the eclipse a cloud passed over the sun, and shortly after the sky clouded over.

The plates were developed in the evening, and the copies made on the two following nights.

The six photographs taken with the small photoheliograph are very good, the one with 2 minutes' exposure extending as far as those of JANSSEN, which were exposed during the whole totality. All taken with the large instrument show slight shifts, probably due to the changing of the slides in the smaller instrument; still they will be useful in making out the detail near the limb, and we believe that from the nine plates a drawing can be made that will show the whole structure from the limb to the furthest extension of the corona.

With the second order flat grating apparently we do not seem to have caught anything, but before stating that we have been unsuccessful we must examine the plate under better conditions of illumination; in the first order grating H and K are present as bright lines at the commencement and end of totality; the dense prism spectroscope also shows bright lines at the beginning and end, especially at the end, H, K, *h*, *f*, F being very marked.

Two gelatine red end plates in the prismatic camera were successful as photographs, but, owing to the comparative absence of prominences, will not be so fruitful in their results as the photograph obtained with this instrument in 1882. The slit spectroscope gave a good photograph from the ultra-violet to the green. The spectrum appears mainly continuous, but differs on the two sides of the disc. H and K are very marked, but do not extend across the interval, as they did in last year's. The hydrogen line near G extends out nearly a solar diameter; *h*, F, and 1474 also appear. There are other lines, but they are not so numerous as in the 1882 eclipse. The ROWLAND grating seems to have given no useful results, but this is due to the same cause as the indifferent results in the prismatic camera. The integrating spectroscope was successful. There was little or no perceptible change in the character of the spectrum till just before totality, when the brightest lines of the reversion spectrum were caught. H and K, 1474, and the hydrogen lines are most prominent. The flash was also caught at the end of totality. During totality no result appears to have been obtained.

We commenced packing up on the 7th; and on the 9th, at 5 P.M., we left Caroline Island for Honolulu.

IV. *On Evaporation and Dissociation.*—Part VIII. *A Study of the Thermal Properties of Propyl Alcohol.*

By Professor William Ramsay, *Ph.D., F.R.S., and Professor* Sydney Young, *D.Sc.*

Received June 14,—Read June 21, 1888.

[Plates 3–7.]

In continuation of our investigations of the thermal properties of pure liquids, we have now determined the vapour-pressures, vapour-densities, and expansion in the liquid and gaseous states, of Propyl Alcohol, and from these results we have calculated the heats of vaporization at definite temperatures. The range of temperature is from 5° to 280°, and the range of pressure from 5 mms. to 56,000 mms.

Preparation of pure Propyl Alcohol.—A sample of propyl alcohol was procured from Kahlbaum, of Berlin. It was dried with barium oxide, and then with small quantities of sodium; but in this case the results were not nearly so satisfactory as with methyl and ethyl alcohol, for propyl alcohol is soluble in water, forming a mixture or "hydrate," which boils constantly at a lower temperature than the pure alcohol. It is not completely decomposed by sodium, and can be separated only by repeated fractional distillation. This hydrate was first described by Chancel ('Comptes Rendus,' vol. 68, 1867, p. 659), who, observing that it boiled with perfect constancy, assumed that it possessed a definite composition, and gave it the formula C_3H_8O, H_2O. It has more recently been examined by Konowalow (Wiedemann's 'Annalen,' vol. 14, 1881, p. 34), who has determined the vapour-pressures of varying mixtures of propyl alcohol and water at definite temperatures. Konowalow finds that the composition of the mixture, the vapour of which exerts the greatest pressure, is not the same at different temperatures, but that the mixture contains more alcohol at high temperatures than at low. From this it has been concluded that the composition of the "hydrate" must depend on the pressure under which the liquid is distilled. We have proved experimentally that this is the case (but we reserve a discussion of this interesting substance for a future paper), and we give the results of our experiments in an Addendum to this paper.

After repeated fractionation we succeeded in obtaining a quantity of propyl alcohol which boiled almost constantly at 97°·6 (the rise of temperature during distillation being less than 0°·1) under a pressure of 763·8 mms. This sample was employed for the determination of vapour-pressures at low temperatures, and for the constants at high temperatures with quantities A, B, and D (see p. 140).

The specific gravity of the alcohol was determined several months later, and a fresh quantity of the alcohol was prepared from the hydrate into which the greater part of the alcohol had been converted. The hydrate was treated with dry potassium carbonate, when, as described by CHANCEL (*loc. cit.*), two layers were formed, the lower one being an aqueous solution of potassium carbonate, the upper one consisting of the partially hydrated alcohol. The alcohol was fractionated several times, potassium carbonate being each time added to the most volatile distillate, until a quantity was obtained, boiling from 97°·1 to 97°·15 under a pressure of 752 mms.

For the determinations of vapour-density at high temperatures with quantity C, the alcohol was refractionated. The portion employed boiled constantly at 97°·1 under a pressure of 750·6 mms.

Reduced to 760 mms. these temperatures would be (1) 97°·45 ; (2) 97°·4 ; (3) 97°·4.

The boiling-point of propyl alcohol has been determined by numerous experimenters, and the results obtained by several are very concordant; the most reliable appear to be the following :—

Observer.	Reference.	Pressure.	Temperature.	Temperature reduced to 760 mms.
BRÜHL	'Annalen der Chemie,' vol. 200, p. 173	752·2	97–97·2	97·35
ZANDER	,, ,, ,, 214, p. 153	760·0	97·40	97·40
LINNEMANN	,, ,, ,, 161, p. 26	760·0	97·41	97·41
SCHIFF	,, ,, ,, 220, p. 101	752·4	97·10	97·35
KONOWALOW	*Loc. cit.*	749·2	97·00	97·37

PIERRE and PUCHOT ('Annales de Chimie,' vol. 22, 1871, p. 276) found 98°; and PERKIN ('Chem. Soc. Trans.,' vol. 45, p. 446) gives two determinations: 97°·5 to 98°·5 and 98°, but the boiling-point of the alcohol employed in the final determination of specific gravity is not stated.

Apparatus employed.—The apparatus employed was the same as that described in our memoir on Ethyl Oxide ('Phil. Trans.,' A, 1887, p. 57).

Experimental Results.

Vapour-Pressures at Low Temperatures.—These were determined by our dynamical method. The thermometer had been standardized by determinations of the vapour-pressures of water ; its zero point was redetermined.

Table I.

Pressure.	Temperature.	Pressure.	Temperature.	Pressure.	Temperature.
mms.	°	mms.	°	mms.	°
5·25	5·5	34·5	33·7	235·35	69·5
5·10	4·9	42·05	36·95	240·1	70·0
5·25	5·5	50·05	40·0	273·7	72·85
6·10	7·5	57·55	42·3	312·85	75·85
7·25	9·7	67·6	45·2	355·8	78·8
8·70	12·8	79·7	48·2	404·9	81·9
9·20	13·4	94·6	51·4	455·5	84·6
10·55	15·6	110·95	54·5	505·6	87·0
13·30	18·7	130·8	57·6	561·9	89·6
16·40	21·75	151·85	60·5	615·1	92·0
20·30	25·1	175·3	63·4	672·7	94·3
24·55	28·2	201·1	66·2	760·7	97·5
29·35	31·1	204·8	66·7		

These results were plotted on sectional paper, curves drawn through them, and the pressures corresponding to equal intervals of temperature read off.

Specific Gravity of Propyl Alcohol.—A Sprengel's tube of the form recommended by Perkin was employed. The weighings were reduced to a vacuum :—

Weight of water at 16°·7	15·3169 grms.
Capacity of tube at 16°·7	15·3339 c.cs.
Weight of alcohol at 0°	12·5577 grms.
Capacity of tube at 0°	15·3275 c.cs.
Specific gravity at 0°	0·81929.
Volume of 1 grm. at 0°	1·22056 c.cs.

Weight of alcohol at 10°·72	12·4338 grms.
Capacity of tube at 10°·72	15·3316 c.cs.
Specific gravity at 10°·72	0·81099.
Volume of 1 grm. at 10°·72	1·23306 c.cs.

The results of other observers* are not very easy to compare with ours ; some of them are given in terms of water at 0°, others at 4°, and others again at the same

* A tabulated statement of determinations of the boiling-point and specific gravity of propyl alcohol is given by Lossen, 'Annalen der Chemie u. Pharmacie,' vol. 214, p. 105.

temperature as the alcohol. The best method of comparison appears to be to reduce the results of other observers to true densities, and to read from the curve constructed from our observations the densities at the corresponding temperatures. The most reliable results appear to be the following (the references are the same as before):—

Observer.	Specific gravity $\frac{t^\circ}{t'^\circ}$	Density or $\frac{t^\circ}{4^\circ}$	Result from our curve.
Brühl	·8044 20°/4°	·8044 at 20°	·8035
Zander	·8069 17°/0°	·8068 at 17°	·8059
Linnemann	·8066 15°/15°	·8059 at 15°	·8076
Perkin	·80883 15°/15°	·8082 at 15°	·8076
,,	·80247 25°/25°	·8002 at 25°	·7995
Schiff	·7366 97°·1/4°	·7366 at 97°·1	·7353

It will be seen that our results are in close agreement with those of other observers; they are very slightly lower than most of the others, but at 15° our result is rather higher than Linnemann's.

Constants at High Temperatures.—Four different amounts of propyl alcohol were employed for these determinations. The vapour-pressures and the orthobaric volumes and compressibilities of the liquid were determined with the largest quantity, A, the weight of which was calculated from its volume and specific gravity. The smaller quantities, B, C, D, were employed for the determinations of the volumes of a gram of vapour. The weight of B was ascertained by comparisons of its volume with that of A under similar conditions of temperature and pressure. The weight of C was obtained in a similar manner from that of B, and the weight of D similarly from that of C.

Weight of quantity A.—The actual volumes of this quantity at various temperatures were plotted on sectional paper, and a curve drawn through the points; the volumes at 0° and 10°·72 were read off, and the weight calculated from the densities at those temperatures.

Volume of A at 0°	0·32335.	Volume of 1 grm. at 0°	1·22056.
Volume of A at 10°·72	0·32675.	Volume of 1 grm. at 10°·72	1·23306.

	Grm.
Weight from volume at 0°	0·26492
Weight from volume at 10°·72	0·26499
Mean	0·26496

		Grm.
Weight of B.	Mean of numerous comparisons with A	0·03763.
Weight of C.	Mean of numerous comparisons with B	0·006015.
Weight of D.	Mean of numerous comparisons with C	0·005180.

Constants with the largest quantity, A.—The vapour-pressures at each temperature were, as usual, determined at the widest possible limits of volume.

Volume of 1 grm. at 23°·7 = 1·2493. Specific gravity 0·8004.

The pure liquids employed for obtaining constant temperatures were carbon bisulphide, ethyl alcohol, chlorobenzene, bromobenzene, aniline, methyl salicylate, and bromonaphthalene ('Chem. Soc. Trans.,' 1885, p. 640). We think it unnecessary to state in each case the liquid employed and the pressure under which it boiled.

Temperature.	Volume of 1 grm.	Specific gravity. (Weight of 1 c.c.)	Pressure.
°	c.cs.		mms.
30	1·2569	0·7956	800
	1·2522	..	32,590
40	1·2712	0·7866	800
	1·2659	..	22,910
	1·2612	..	53,480
50	1·2850	0·7782	800
	1·2796	..	19,730
	1·2743	..	53,480
60	1·2992	0·7697	800
	1·2934	..	23,730
	1·2873	..	53,480
70	1·3137	0·7612	800
	1·3070	..	26,210
	1·3017	..	53,480
80	1·3298	0·7520	800
	1·3274	..	6,750
	1·3207	..	29,090
	1·3180	..	53,480
90	1·3742	0·7423	800
	1·3412	..	17,070
	1·3346	..	37,970
100	..	..	842
			843
			842
			843
		Mean vap.-press.	= 842·5
	1·3651	0·7326	800
	1·3618	..	11,510
	1·3550	..	31,910
	1·3483	..	53,480
110	..	..	1,204
			1,207
			1,205
			1,207
		Mean vap.-press.	= 1,206
	1·3863	0·7214	1,200
	1·3822	..	7,415
	1·3754	..	27,910
	1·3687	..	47,480

Temperature.	Volume of 1 grm.	Specific gravity. (Weight of 1 c.c.)	Pressure.
°	c.cs.		mms.
120	..	..	1,681 1,683 1,684 1,684
		Mean vap.-press.	= 1,683
	1·4028	0·7129	1,684
	1·3961	..	18,540
	1·3894	..	35,000
	1·3833	..	53,490
130	..	..	2,292 2,293 2,291 2,302
		Mean vap.-press.	= 2,295
	1·4292	0·6997	2,300
	1·4231	..	12,300
	1·4163	..	26,100
	1·4097	..	40,290
	1·4031	..	54,680
140	..	..	3,077 3,071 3,076 3,072
		Mean vap.-press.	= 3,074
	1·4572	..	1,160
	1·4546	0·6875	3,074
	1·4505	..	7,305
	1·4439	..	20,190
	1·4371	..	33,430
	1·4303	..	48,270
150	..	..	4,053 4,062 4,067 4,054
		Mean vap.-press.	= 4,059
	1·4835	0·6741	4,022
	1·4775	..	11,250
	1·4708	..	23,010
	1·4642	..	35,230
	1·4576	..	44,690
160	..	..	5,270 5,257 5,272 5,259
		Mean vap.-press.	= 5,264
	1·5144	0·6603	5,294

Temperature.	Volume of 1 grm.	Specific gravity. (Weight of 1 c.c.)	Pressure.
°	c.cs.		mms.
170	..	..	6,657
			6,720
			6,702
			6,702
		Mean vap.-press.	= 6,695
	1·5518	0·6444	6,750
	1·5458	..	14,012
	1·5391	..	22,689
	1·5323	..	29,197
	1·5256	..	38,919
	1·5189	..	45,898
	1·5122	..	55,431
180	..	..	8,374
			8,388
			8,387
			8,398
		Mean vap.-press.	= 8,387
	1·5913	0·6284	8,560
	1·5865	..	11,810
	1·5797	..	18,710
	1·5730	..	24,260
	1·5663	..	31,430
	1·5596	..	38,000
	1·5529	..	45,750
	1·5497	..	52,510
190	..	..	10,488
			10,467
			10,468
			10,441
		Mean vap.-press.	= 10,466
	1·6356	0·6114	10,410
	1·6343	..	12,020
	1·6274	..	16,680
	1·6206	..	22,040
	1·6138	..	26,650
	1·6070	..	32,250
	1·6004	..	38,020
	1·5938	..	43,800
	1·5869	..	49,500
200	..	..	12,867
			12,817
			12,831
			12,856
		Mean vap.-press.	= 12,828
	1·6888	0·5921	12,829
	1·6820	..	16,540
	1·6753	..	20,030
	1·6617	..	27,840
	1·6481	..	36,170
	1·6346	..	45,710
	1·6278	..	50,607

Temperature.	Volume of 1 grm.	Specific gravity. (Weight of 1 c.c.)	Pressure.
°	c.cs.		mms.
210	..	..	15,525
			15,562
			15,614
			15,599
		Mean vap.-press.	= 15,575
	1·7489	0·5718	15,576
	1·7435	..	17,910
	1·7299	..	23,680
	1·7164	..	29,580
	1·7028	..	36,050
	1·6892	..	43,240
	1·6757	..	51,690
220	..	..	18,626
			18,653
			18,692
			18,712
		Mean vap.-press.	= 18,671
	1·8244	0·5481	18,695
	1·8121	..	21,690
	1·7985	..	25,710
	1·7849	..	29,860
	1·7713	..	34,680
	1·7576	..	39,970
	1·7440	..	45,540
	1·7304	..	51,710
230	..	..	22,108
			22,158
			22,144
			22,234
		Mean vap.-press.	= 22,161
	1·9081	0·5241	22,223
	1·8944	..	24,870
	1·8808	..	27,520
	1·8672	..	30,490
	1·8536	..	33,900
	1·8399	..	37,290
	1·8262	..	41,090
	1·8126	..	45,170
	1·7990	..	49,810
	1·7854	..	54,770
240	..	..	26,204
			26,224
			26,201
			26,204
		Mean vap.-press.	= 26,208
	2·0308	0·4924	26,202
	2·0182	..	27,160
	2·0044	..	28,640
	1·9907	..	30,140
	1·9632	..	33,860
	1·9360	..	38,050
	1·9085	..	43,430
	1·8812	..	49,340
	1·8677	..	53,170

Temperature.	Volume of 1 grm.	Specific gravity. (Weight of 1 c.c.)	Pressure.
	c.cs.		mms.
250°	..	..	30,729
			30,725
			30,822
			30,840
		Mean vap.-press.	= 30,779
	2·2093	0·4526	30,831
	2·1989	..	31,160
	2·1703	..	32,470
	2·1426	..	33,950
	2·1150	..	35,640
	2·0875	..	37,940
	2·0600	..	40,390
	2·0327	..	43,470
	2·0049	..	47,050
	1·9774	..	51,300
260	..	..	36,137
			36,087
			36,101
			36,115
		Mean vap.-press.	= 36,110
	2·5601	0·3906	36,115
	2·5468	..	36,221
	2·4770	..	36,880
	2·4072	..	38,060
	2·3375	..	39,560
	2·2680	..	42,050
	2·1994	..	45,380
	2·1294	..	50,560
	2·1018	..	53,260
263·15	..	..	37,950
263·5	..	..	38,100
263·54	3·0102	0·3322	38,110
263·64	4·1697	..	38,130
(Critical	3·4696	..	38,110
Temperature)	3·0368	..	38,120
	2·7568	..	38,130
	2·6172	..	39,170
	2·4773	..	40,760
	2·3378	..	43,970
	2·2682	..	46,960
	2·1997	..	50,530
	2·1711	..	52,950
270	4·1702	..	41,540
	3·7496	..	41,940
	3·4701	..	42,150
	3·1770	..	42,620
	2·8973	..	43,340
	2·7571	..	44,000
	2·6172	..	45,480
	2·4776	..	47,760
	2·3381	..	52,130
	2·2686	..	55,630
280·1	4·1713	..	47,000
	3·7505	..	48,040
	3·4710	..	48,550
	3·1778	..	49,730
	2·8981	..	51,540
	2·7578	..	53,310
	2·6179	..	55,510

Second Quantity, B. Weight = 0·03763.

Temperature.	Volume of 1 grm.	Pressure.	Vapour-density.
°	c.cs.	mms.	
230	29·34	14,115	38·10
	26·38	15,254	39·14
	24·41	16,114	40·03
	22·35	17,120	41·16
	20·38	18,195	42·46
	19·40	18,793	43·20
	18·41	19,368	44·16
	17·43	20,037	45·09
	16·45	20,674	46·33
	15·48	21,373	47·61
	14·50	21,990	49·39
	Vapour-pressure	= 22,094	
240	29·35	14,602	37·48
	26·39	15,866	38·37
	23·34	17,356	39·65
	21·37	18,483	40·66
	19·40	19,742	41·93
	18·42	20,376	42·80
	17·43	21,073	43·72
	16·45	21,848	44·68
	15·48	22,606	45·89
	14·50	23,388	47·35
	13·54	24,229	48·97
	12·58	25,067	50·95
	11·62	25,869	53·44
	Vapour-pressure	= 26,102	
250	29·35	15,098	36·95
	26·39	16,414	37·80
	23·35	18,012	38·94
	21·38	19,209	39·87
	19·41	20,575	41·01
	17·44	22,126	42·44
	16·46	22,897	43·45
	15·49	23,811	44·41
	14·51	24,688	45·72
	13·54	25,657	47·13
	12·58	26,635	48·87
	11·62	27,658	50·94
	10·67	28,725	53·42
	9·72	29,649	56·81
	8·78	30,546	61·10
	Vapour-pressure	= 30,809	
260	29·36	15,647	36·32
	26·41	16,999	37·18
	23·35	18,710	38·20
	21·38	19,997	39·03
	19·41	21,424	40·13
	17·44	23,087	41·44
	15·49	24,939	43·20
	13·54	27,006	45·62
	12·58	28,178	47·07
	11·62	29,367	48·89
	10·67	30,586	51·12

Temperature.	Volume of 1 grm.	Pressure.	Vapour-density.
°	c.cs.	mms.	
260	9·72	31,867	53·85
	8·78	32,961	57·69
	7·83	34,138	62·45
	6·88	35,275	68·75
	6·41	35,646	73·06
	Vapour-pressure	= 35,955	
263·64 (Critical Temperature)	9·73	32,467	53·21
	8·78	33,854	56·54
	7·83	35,224	60·92
	6·88	36,433	67·01
	5·94	37,309	75·85
	4·99	37,940	88·66
	4·05	37,969	109·20
	3·11	38,068	141·80
	2·64	39,037	162·70
270	29·37	16,098	35·96
	26·41	17,548	36·69
	23·36	19,379	37·56
	21·39	20,744	38·32
	19·42	22,266	39·32
	17·44	24,002	40·60
	15·49	26,076	42·08
	13·55	28,373	44·23
	11·63	30,953	47·24
	9·73	33,807	51·70
	7·83	36,917	58·81
	5·94	39,765	72·00
	5·00	40,908	85·13
	4·05	41,753	98·17
	3·11	42,824	127·50
	2·64	45,250	142·00
	2·36	51,749	139·00
280·15	29·38	16,572	35·58
	26·41	18,097	36·23
	23·36	19,982	37·10
	21·39	21,417	37·80
	19·42	23,005	38·76
	17·45	24,907	39·84
	15·50	27,073	41·28
	13·55	29,609	43·16
	11·63	32,469	45·86
	9·73	35,803	49·72
	7·83	39,485	56·00
	5·94	43,429	67·14
	5·00	45,290	76·53
	4·05	47,200	90·50
	3·59	48,304	100·00
	3·11	50,451	110·20
	2·83	52,312	116·90

Third Quantity, C. The experiments with this quantity were made with a new pressure apparatus, a new volume-tube, and new pressure-gauges. Fresh samples of methyl salicylate and bromonaphthalene were also employed for heating the tube.

At each temperature the propyl alcohol vapour was made to occupy the largest possible volume, and was left for several hours until the vapour-pressure of mercury had attained its maximum. Readings were taken every half hour to ascertain when the pressure had become constant. The subsequent readings were taken at diminishing volumes.

Temperature.	Volume of 1 grm.	Pressure.	Vapour-density.
°	c.cs.	mms.	
150	172·8	2,359	32·25
	162·4	2,497	32·41
	151·9	2,657	32·57
	141·4	2,838	32·77
	130·8	3,044	33·01
	120·2	3,281	33·33
	109·5	3,565	33·66
	98·5	3,891	34·21
	Vapour-pressure	= 4,053	
180	172·9	2,554	32·02
	152·0	2,884	32·25
	130·9	3,325	32·48
	115·0	3,745	32·84
	98·8	4,295	33·31
	82·6	5,043	33·95
	66·3	6,117	34·80
	55·4	7,091	36·02
	49·9	7,721	36·71
	Vapour-pressure	= 8,365	
200	173·0	2,683	31·67
	152·1	3,033	31·86
	131·0	3,492	32·14
	109·7	4,129	32·46
	93·5	4,786	32·85
	77·2	5,683	33·51
	60·9	7,013	34·43
	49·9	8,309	35·44
	44·5	9,136	36·20
	39·0	10,129	37·24
	33·5	11,362	38·63
	Vapour-pressure	= 12,691	
220	173·1	2,813	31·46
	152·2	3,182	31·64
	131·1	3,667	31·88
	109·7	4,335	32·20
	93·5	5,035	32·53
	77·2	5,995	33·09
	60·9	7,435	33·83
	49·9	8,850	34·66
	39·0	10,895	36·07
	33·5	12,318	37·12
	28·0	14,123	38·81
	22·4	16,454	41·56
	19·6	17,850	43·73
	Vapour-pressure	= 18,711	

Temperature.	Volume of 1 grm.	Pressure.	Vapour-density.
°	c.cs.	mms.	
230	173·2	2,881	31·34
	162·8	3,053	31·46
	147·0	3,367	31·60
	131·1	3,761	31·70
	115·1	4,252	31·93
	99·0	4,904	32·21
	82·7	5,783	32·69
	66·4	7,081	33·28
	55·4	8,317	33·90
	44·5	10,108	34·76
	33·5	12,784	36·48
	22·4	17,250	40·44
	16·8	20,587	45·09
	Vapour-pressure	= 22,183	
240	173·2	2,939	31·32
	152·3	3,327	31·47
	131·1	3,844	31·63
	109·8	4,552	31·90
	93·6	5,284	32·24
	77·3	6,304	32·73
	60·9	7,847	33·34
	50·0	9,380	34·02
	44·5	10,375	34·54
	39·0	11,606	35·21
	33·5	13,180	36·08
	28·0	15,215	37·46
	22·4	17,965	39·50
	19·6	19,674	41·26
	16·8	21,630	43·76
	14·1	23,894	47·43
	12·4	25,344	50·75
	Vapour-pressure	= 26,227	
260	173·3	3,061	31·23
	152·3	3,469	31·35
	131·2	4,006	31·52
	109·9	4,746	31·77
	93·6	5,521	32·04
	77·3	6,593	32·50
	61·0	8,228	33·02
	50·0	9,860	33·60
	44·5	10,929	34·05
	39·0	12,259	34·62
	33·5	13,977	35·34
	28·0	16,224	36·49
	22·4	19,320	38·23
	19·7	21,284	39·61
	16·9	23,612	41·63
	14·1	26,509	44·39
	12·4	28,499	46·87
	11·3	29,850	49·12
	Vapour-pressure	= 36,285	

Temperature.	Volume of 1 grm.	Pressure.	Vapour-density.
°	c.cs.	mms.	
279·9	173·4	3,184	31·13
	152·4	3,609	31·24
	131·3	4,168	31·41
	109·9	4,943	31·63
	93·7	5,754	31·87
	77·3	6,883	32·28
	61·0	8,596	32·77
	50·0	10,342	33·22
	39·0	12,904	34·10
	33·6	14,750	34·72
	28·0	17,196	35·69
	22·4	20,581	37·21
	19·7	22,810	38·32
	16·9	25,488	39·98
	14·1	28,886	42·25

Fourth Quantity, D. The experiments with this quantity were made with the old apparatus.

Temperature.	Volume of 1 grm.	Pressure.	Vapour-density.
°	c.cs.	mms.	
130	212·6	1,826	32·26
	191·1	2,014	32·54
	184·0	2,084	32·66
	176·9	2,158	32·80
	169·1	2,236	33·13
	Vapour-pressure	= 2,288	
150	212·7	1,940	31·86
	191·2	2,147	32·02
	169·2	2,402	32·35
	154·9	2,607	32·56
	140·6	2,847	32·84
	126·4	3,134	33·20
	112·2	3,484	33·63
	98·1	3,905	34·31
	Vapour-pressure	= 4,023	
180	212·9	2,097	31·68
	191·4	2,320	31·85
	169·3	2,612	31·98
	155·0	2,837	32·15
	140·7	3,102	32·39
	126·5	3,431	32·59
	112·3	3,821	32·96
	98·2	4,320	33·34
	84·3	4,953	33·88
	70·5	5,809	34·53

Reduction and Arrangement of Results.

Vapour-Pressures.—The vapour-pressures experimentally determined, and also those calculated by the formula $\log p = a + b\alpha^t + c\beta^t$, are given in the following table:—

Temperature.	Pressure.							
	Still method.	A.	B.	C.	D.	Mean.	Calculated.	Δp.
0°	3·44	..	..	..	..	3·44	3·49	+ ·05
10	7·26	..	..	..	..	7·26	7·39	·13
20	14·50	..	..	..	..	14·5	14·78	·28
30	27·60	..	..	..	..	27·6	28·13	·53
40	50·20	..	..	..	..	50·2	51·12	·92
50	87·20	..	..	..	..	87·2	89·00	1·80
60	147·00	..	..	..	..	147·0	148·97	1·97
70	239·00	..	..	..	..	239·0	240·44	1·44
80	376·00	..	..	..	..	376·0	375·31	− 0·69
90	574·00	..	..	..	..	574·0	568·11	− 5·89
100	..	842·5	..	..	..	842·5	835·89	− 6·61
110	..	1206·0	..	..	..	1206	1198·2	− 7·8
120	..	1683·0	..	..	..	1683	1677·0	− 6·0
130	..	2295·0	..	..	2288	2293	2295·9	+ 2·9
140	..	3074·0	..	..	..	3074	3080·3	6·3
150	..	4059·0	..	4053	4023	4052	4057·1	5·1
160	..	5264·0	..	..	..	5264	5253·4	− 10·6
170	..	6695·0	..	..	..	6695	6697·8	+ 2·8
180	..	8387·0	..	8365	..	8383	8418·8	35·8
190	..	10466·0	..	..	..	10466	10445	− 21
200	..	12828·0	..	12691	..	12801	12809	+ 8
210	..	15575·0	..	..	..	15575	15539	− 36
220	..	18671·0	..	18711	..	18679	18667	− 12
230	..	22161·0	22094	22183	..	22154	22230	+ 76
240	..	26208·0	26102	26227	..	26194	26263	69
250	..	30779·0	30809	..	..	30785	30807	22
260	..	36110·0	35955	36285	..	36103	35908	− 195

In calculating the mean the greatest weight has always been given to the determinations with the largest quantity, A. The constants employed were calculated from pressures at 20°, 80°, 140°, 200°, and 260°. The constants for the formula are—

$$a = 4\cdot479370.$$

$$\log b = \bar{1}\cdot3915059. \qquad \log \alpha = 0\cdot001641423.$$

$$\log c = 0\cdot5509601. \qquad \log \beta = \bar{1}\cdot99657025.$$

$$c \text{ is negative.} \qquad t = t^\circ c - 20.$$

Determinations of the vapour-pressures of propyl alcohol by the statical method are given by Konowalow. They are reproduced in the following table, together with the pressures calculated from our constants for Biot's formula. It is to be noticed that our results at these temperatures were obtained by the dynamical method.

Series I.

Temperature.	Pressure. Konowalow observed.	Pressure. R. and Y. calculated.	Temperature.	Pressure. Konowalow observed.	Pressure. R. and Y. calculated.
°			°		
11·50	8·1	8·2	59·40	143·25	144·6
16·80	10·0	11·9	59·90	146·90	148·3
21·80	17·2	17·1	70·40	245·80	245 0
28·35	24·7	25·4	74·90	304·20	300·3
30·60	29·5	29·2	80·50	384·10	383·5
33·75	35·7	35·4	81·75	405·20	404·7
39·10	48·3	48·5	81·90	406·40	407·2
49·20	85·3	85·3	89·60	561·70	559·3
52·35	101·0	100·9	98 60	794·90	793·4

Series II.

Temperature.	Pressure. Konowalow observed.	Pressure. R. and Y. calculated.
°		
16·5	10·9	11·65
52·4	101·1	101·10
59·9	148·5	148·30
70·5	247·7	246·10
82·1	411·4	410·60

It will be seen that, with the single exception of the observation at 16°·8 in Series I., the agreement is extremely satisfactory. The vapour-pressures have also been determined by Dr. A. Richardson by our method ('Chem. Soc. Trans.,' vol. 49, p. 763) with concordant results.

Orthobaric Volumes of 1 *Gram of Liquid.*

Temperature.	Volume.	Specific gravity.*	Temperature.	Volume.	Specific gravity.	Temperature.	Volume.	Specific gravity.
°	c.cs.		°	c.cs.		°	c.cs.	
0	1·221	0·8193	100	1·365	0·7325	200	1·689	0 5920
10	1·233	0·8110	110	1·385	0·7220	210	1·750	0·5715
20	1·245	0·8035	120	1·406	0·7110	220	1·823	0·5485
30	1·256	0·7960	130	1·430	0·6995	230	1·912	0·5230
40	1·270	0·7875	140	1·455	0·6875	240	2·032	0·4920
50	1·285	0·7785	150	1·484	0·6740	250	2·210	0·4525
60	1·299	0·7700	160	1·515	0·6600	260	2·561	0·3905
70	1·314	0·7610	170	1·550	0·6450	263·15	2·899	0·3450
80	1·330	0·7520	180	1·591	0·6285	263·50	2·959	0·3380
90	1·347	0·7425	190	1·637	0·6110	263·54	3·012	0·3320

* As in our former memoirs, the specific gravities are referred to water at 4°, and are therefore true masses of one cubic centimetre.

Orthobaric Volumes of 1 *Gram of Vapour.*

Temperature.	Volume of 1 grm.	Specific gravity (mass of 1 c.c.).	Vapour-density.	Temperature.	Volume.	Specific gravity.	Vapour-density.
°	c.cs.			°	c.cs.		
80	958·0	0·00104	30·50	180	44·50	0·0225	37·6
90	643·0	0·00156	30·90	190	35·40	0·0282	38·9
100	443·0	0·00226	31·30	200	28·30	0·0353	40·5
110	312·0	0·00320	31·80	210	22·65	0·0442	42·7
120	225·0	0·00443	32·40	220	18·00	0·0556	45·6
130	165·0	0·00605	33·00	230	14·21	0·0704	49·5
140	124·0	0·00805	33·70	240	11·06	0·0904	54·9
150	93·9	0·01060	34·50	250	8·50	0·1180	62·1
160	72·3	0·01380	35·45	260	6·20	0·1610	74·4
170	56·4	0·01770	36·45				

The following table gives the densities of the unsaturated vapour at equal intervals of temperature and pressure.

Pressure.	Temperatures.											
	130°	150°	180°	200°	220°	230°	240°	250°	260°	263°·64	270°	280°
mms.												
2,000	32·49	31·95	31·59	31·33	31·11	30·99	30·93	..	30·80	..	..	30·70
4,000	..	34·41	33·06	32·46	32·04	31·86	31·71	..	31·50	..	..	31·32
6,000	..	..	34·80	33·75	33·02	32·76	32·52	..	32·16	..	..	31·89
8,000	..	..	37·02	35·19	34·17	33·73	33·39	..	32·90	..	..	32·50
10,000	..	..	..	37·11	35·43	34·86	34·38	..	33·66	..	..	33·12
12,000	..	..	..	39·42	36·93	36·16	35·50	..	34·51	..	..	33·78
14,000	..	..	..	..	38·85	37·74	36·84	..	35·48	..	..	34·47
16,000	..	..	..	..	41·26	39·63	38·41	37·44	36·51	..	35·86	35·23
18,000	..	..	..	..	44·40	41·97	40·23	38·89	37·72	..	36·84	36·06
20,000	..	..	..	..	..	45·00	42·30	40·47	39·00	..	37·89	36·97
22,000	..	..	..	..	..	48·99	44·94	42·33	40·51	..	39·10	38·02
24,000	..	..	..	..	..	..	48·42	44·70	42·25	..	40·48	39·15
26,000	..	..	..	..	..	..	53·76	47·70	44·28	..	42·03	40·41
28,000	..	..	..	..	..	..	..	51·66	46·62	..	43·83	41·82
30,000	..	..	..	..	..	..	..	57·90	49·80	..	46·02	43·38
32,000	..	..	..	..	..	..	..	..	54·66	51·90	48·72	45·24
34,000	..	..	..	..	..	..	..	..	61·80	56·97	52·11	47·43
36,000	..	..	..	..	..	..	..	..	..	64·32	56·40	50·01
38,000	..	..	..	..	..	..	..	..	..	..	62·64	53·16
40,000	..	..	..	..	..	..	..	..	..	..	..	57·12
42,000	..	..	..	..	..	..	..	..	..	..	..	62·40

Heats of Vaporization.

The heats of vaporization are calculated from the thermo-dynamical formula,

$$\frac{L}{s_1 - s_2} = \frac{dp}{dt} \cdot \frac{t}{J}.$$

The values of dp/dt were calculated in the same manner as with the other liquids. The pressures for one-tenth of a degree above and below the required temperature were calculated by means of the equation $\log p = a + b\alpha^t + c\beta^t$, and the difference was multiplied by 5 to obtain the value for 1°. The pressures were reduced to grams per square centimetre, and the value of J was taken as 42,500.

Temperature.		dp/dt		$s_1 - s_2$	L.
° C.	° Abs.	mms.	grms.	c.cs.	cals.
80	353	15·99	21·7	957·00	173·0
90	363	22·73	30·9	642·00	169·0
100	373	31·16	42·4	442·00	164·0
110	383	41·70	56·7	311·00	159·0
120	393	54·45	74·0	224·00	153·0
130	403	69·75	94·8	164·00	147·0
140	413	87·60	119·1	123·00	142·4
150	423	108·20	147·1	92·40	135·3
160	433	131·50	178·8	70·80	129·0
170	443	157·80	214·5	54·90	122·8
180	453	187·00	254·2	42·90	116·3
190	463	219·00	297·7	33·80	109·6
200	473	254·00	345·3	26·60	102·2
210	483	292·50	397·7	20·90	94·5
220	493	334·00	454·0	16·20	85·3
230	503	379·00	515·0	12·30	75·0
240	513	428·00	582·0	9·03	63·4
250	523	481·00	654·0	6·29	50·6
260	533	540·00	734·0	3·64	33·5

The heat of vaporization of propyl alcohol at the boiling-point 97°·4 would be 165·2 calories.

Pressures and Temperatures of Propyl Alcohol at Definite Volumes.

In our previous papers we have given tables of the volumes of a gram of substance at definite temperatures and pressures.

We have recently shown, however, in two papers read before the Physical Society of London and published in the 'Philosophical Magazine' (May and August, 1887), that, when the volume of a stable liquid or gas is kept constant, a very simple relation exists between the pressure and the absolute temperature, which is expressed by the equation

$$p = bt - a,$$

where p is the pressure, t the absolute temperature, and b and a are constants depending on the substance and on the volume occupied by a gram of it.

We have, therefore, considered it better to construct lines of equal volume or "isochors," and to read temperatures and pressures from the isochors, rather than to read the volumes of a gram from isobars constructed from the isotherms.

Owing to the directions assumed by the isochors, it is most convenient to give temperatures (Centigrade) at definite pressures for volumes below the critical volume, and pressures at definite temperatures for larger volumes.

Volumes Smaller than Critical Volume.

Volume.	Pressure in metres of mercury.										
	5	10	15	20	25	30	35	40	45	50	55
1·26	32·40	33·05	33·70	34·35	35·00	35·70	36·35	37·00	37·65	38·30	39·00
1·28	48·10	48·80	49·50	50·20	50·90	51·60	52·30	53·00	53·70	54·40	55·10
1·30	62·80	63·55	64·30	65·05	65·80	66·30	67·05	67·80	68·55	69·30	70·00
1·32	75·40	76·20	77·00	77·75	78·55	79·30	80·10	80·90	81·65	82·45	83·20
1·34	87·55	88·35	89·15	90·00	90·80	91·65	92·45	93·25	94·10	94·90	95·75
1·36	98·85	99·70	100·55	101·40	102·30	103·15	104·00	104·85	105·60	106·50	107·35
1·38	109·00	109·90	110·80	111·70	112·60	113·50	114·40	115·30	116·20	117·10	118·00
1·40	118·40	119·35	120·30	121·25	122·20	123·15	124·10	125·05	126·00	126·95	127·90
1·42	127·05	128·05	129·05	130·05	131·05	132·05	133·05	134·05	135·05	136·05	137·05
1·44	134·75	135·80	136·85	137·90	138·90	139·95	141·00	142·05	143·10	144·10	145·15
1·46	142·00	143·10	144·20	145·25	146·35	147·40	148·50	149·60	150·65	151·75	152·80
1·48	148·55	149·70	150·85	151·95	153·10	154·20	155·35	156·50	157·60	158·75	159·85
1·50	..	155·85	157·05	158·20	159·40	160·55	161·75	162·95	164·10	165·30	166·45
1·52	..	161·85	163·10	164·30	165·55	166·75	168·00	169·25	170·45	171·70	172·90
1·54	..	167·35	168·60	169·90	171·15	172·45	172·70	174·95	176·25	177·50	178·80
1·56	..	172·40	173·70	175·05	176·35	177·70	179·00	180·30	181·65	182·95	184·30
1·58	..	177·30	178·70	180·05	181·40	182·80	184·20	185·55	186·95	188·30	189·70
1·60	..	..	183·15	184·55	186·00	187·40	188·85	190·30	191·70	193·15	194·55
1·62	..	..	187·65	189·10	190·60	192·05	193·55	195·05	196·50	198·00	199·55
1·64	..	..	191·95	193·45	195·00	196·50	198·05	199·60	201·10	202·65	204·15
1·66	..	..	195·50	197·05	198·65	200·20	201·80	203·40	204·95	206·55	208·10
1·70	..	..	202·40	204·10	205·80	207·50	209·20	210·90	212·60	214·30	216·00
1·75	..	..	209·80	211·65	213·45	215·30	217·15	219·00	220·85	222·65	224·50
1·80	..	..	..	218·05	220·00	222·00	224·00	226·00	228·00	229·95	231·95
1·85	..	..	..	..	225·75	227·85	230·00	232·15	234·30	236·45	238·55
1·90	..	..	..	..	230·60	232·90	235·20	237·50	239·80	242·10	244·40
1·95	..	..	..	..	..	237·00	239·50	241·95	244·40	246·90	249·35
2·00	..	..	..	..	..	240·60	243·25	245·90	248·55	251·20	253·80
2·10	..	..	..	..	..	..	248·80	251·80	254·80	257·80	260·80
2·20	..	..	..	..	..	..	..	256·10	259·50	262·90	266·30
2·30	..	..	..	..	..	..	..	259·40	263·25	267·05	270·90
2·40	..	..	..	..	..	..	..	261·70	266·00	270·25	274·55
2·50	..	..	..	..	..	..	..	263·20	267·95	272·75	277·50
2·60	..	..	..	..	..	..	..	264·30	269·40	274·50	279·60
2·70	..	..	..	..	..	..	..	265·25	270·45	275·60	
2·80	..	..	..	..	..	..	..	265·90	271·40	277·00	
2·90	..	..	..	..	..	..	..	266·00	272·00	278·10	
3·00	..	..	..	..	..	..	..	266·10	272·60	279·15	

It is, however, possible to give a few pressures at even volumes and temperatures, and these, and also the pressures in the following table for volumes up to 30 c.cs. are represented by crosses in the plates (Plate 3 and 6).

Volumes Larger than Critical Volume.

Volume.	Temperature.											
	130°	150°	180°	200°	220°	230°	240°	250°	260°	263°·64	270°	280°
c.cs.												
3	..	..	..	..	..	..	..	..	..	38,120	43,000	50,660
4	..	..	..	..	..	..	..	..	..	38,120	41,680	47,280
5	..	..	..	..	..	..	..	..	..	37,960	40,850	45,380
6	..	..	..	..	..	..	..	..	..	37,280	39,610	43,280
7	..	..	..	..	..	..	..	..	35,190	36,280	38,190	41,200
8	..	..	..	..	..	..	..	..	34,000	34,940	36,580	39,150
9	..	..	..	..	..	..	..	30,390	32,650	33,480	34,920	37,180
10	..	..	..	..	..	..	..	29,430	31,360	..	33,300	35,230
12	..	..	..	..	..	..	25,710	27,270	28,820	..	30,380	31,940
14	..	..	..	..	..	..	23,910	25,190	26,470	..	27,776	29,040
16	..	..	..	..	..	21,130	22,220	23,310	24,390	..	25,480	26,570
18	..	..	..	..	..	19,800	20,730	21,660	22,580	..	23,510	24,440
20	..	..	..	..	17,660	18,480	19,310	20,130	20,950	..	21,780	22,600
25	..	..	..	..	15,340	15,940	16,540	17,150	17,750	..	18,350	18,950
30	..	..	..	12,474	13,430	13,910	14,390	14,860	15,340	..	15,820	16,300
40	..	..	..	9,998	10,656	10,985	11,314	..	11,972	..	..	12,630
50	..	..	7,780	8,299	8,818	9,078	9,337	..	9,856	..	..	10,375
60	..	..	6,695	7,109	7,523	7,730	7,937	..	8,351	..	..	8,765
80	..	..	5,210	5,504	5,798	5,945	6,092	..	6,386	..	..	6,680
100	..	3,898	4,252	4,488	4,724	4,842	4,960	..	5,196	..	..	5,432
120	..	3,321	3,606	3,796	3,985	4,080	4,175	..	4,365	..	..	4,555
140	..	2,876	3,120	3,283	3,447	3,528	3,610	..	3,772	..	..	3,935
170	2,258	2,391	2,590	2,724	2,857	2,923	2,990	..	3,122	..	..	3,255
200	1,955	2,063	2,224	2,332	2,439	2,493	2,547	..	2,654	..	..	2,762

These pressures agree very well with those read directly from the isotherms, except near the condensing point at low temperatures, and to a much smaller extent at high temperatures. The greatest error is at the lowest pressure at 150°, and amounts to 1·45 per cent.

The approximate critical temperature of propyl alcohol is 263°·7, the approximate critical pressure 38,120 mms., and the approximate volume of 1 grm. 3·6 c.cs. The first two of these constants must be very nearly correct; the third cannot be determined with nearly the same accuracy.

Addendum.

(Added February 11, 1889.)

The conclusions of Konowalow (*loc. cit.*) regarding the nature of the so-called hydrate of propyl alcohol have been fully confirmed. It was found that the composition of the mixture, which boiled constantly under a pressure of 198·7 mms., differed from that obtained under the ordinary atmospheric pressure; the lower the pressure, the higher is the percentage of water in the distillate.

The boiling-points of four different samples of the mixture were determined under pressures varying from 746 to 762 mms.; corrected to 760 mms., the temperatures observed were 87°·65, 87°·85, 87°·9, and 87°·6—mean 87°·75.

The vapour-pressures were determined by both the dynamical and statical methods, with the following results:—

Dynamical Method.

Pressure.	Temperature.	Pressure.	Temperature.	Pressure.	Temperature.
mms.	°	mms.	°	mms.	°
5·75	− 0·9	38·9	27·4	269·8	63·5
6·75	+ 1·25	46·0	30·2	304·3	66·1
8·15	4·1	57·1	33·6	347·9	69·1
9·0	5·5	66·8	36·3	358·7	69·8
9·2	5·85	78·9	39·2	405·7	72·6
10·1	7·2	92·55	42·2	466·4	75·9
12·55	9·8	109·5	45·3	526·6	78·9
15·8	13·3	123·5	47·65	590·9	81·9
19·05	16·1	143·7	50·5	658·8	84·5
22·75	18·7	167·4	53·6	760·6	87·9
27·55	21·7	200·6	57·4		
32·75	24·6	232·0	60·3		

Statical Method.

Temperature.	Pressure.	Pressure read from curve constructed from results by dynamical method.
°	mms.	mms.
13·3	16·6	15·7
15·1	18·2	17·7
15·7	19·2	18·45
25	35·5	33·8
30	47·9	45·4
40 *	83·8 and 83·95	81·1
50	142·1	138·9
60	231·7	226·5
70	365·4	361·5
75	452·7	450·0

* The following pressures were also observed at 40° in the vapour-density tube:—83·5, 82·35, 82·55, 82·25.

It will be seen that the results by the statical method are uniformly a very little higher than by the dynamical method. The behaviour of the substance resembles that of an imperfectly purified stable substance more closely than that of a dissociating body.

A considerable number of determinations of vapour-density were made under varying conditions of temperature and pressure. It was proved, however, that condensation—probably of water—took place on the sides of the tube, and the results at the same temperature and pressure could be made to vary considerably by altering the conditions in such a manner as to increase or diminish the chance of such condensation taking place. The rise of vapour-density at low temperatures or high pressures was in no case greater than could be accounted for by premature condensation of liquid, and the only conclusion to be drawn from the results is that combination of propyl alcohol and water does not take place in the gaseous state.

The contraction on mixing propyl alcohol and water at 0°, in the ratio of 71·46 per cent. of alcohol to 28·54 of water, was ascertained by determining the specific gravities of the alcohol and of the mixture, that of water being known. For 1 grm. of the mixture the contraction was 0·0215 c.c., or 1·857 per cent. With ethyl alcohol and methyl alcohol the contraction is considerably greater.

It may be stated, in conclusion, that we have obtained no experimental evidence of chemical combination between propyl alcohol and water.

V. *The Radio-Micrometer.*

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Communicated by Professor A. W. Rücker, *F.R.S.*

Received March 8,—Read April 19, 1888,—Revised January 5, 1889.

In the preliminary note on the Radio-micrometer which I had the honour to present to the Royal Society last year (1887), I promised to complete, as far as I might be able, the development of the instrument, and, in case of any great improvement in the proportions of the parts, to exhibit an instrument in the improved form. In the present paper I have shown how the best sizes of the several parts may be determined, and how the best result may be attained.

I must, however, first refer to the fact that on February 5, 1886, M. d'Arsonval showed, at a meeting of the Physical Society of France, an instrument called by him the Thermo-galvanometer, with which mine is in all essential respects identical. The invention of an instrument for measuring radiant heat, in which one junction of a closed thermo-electric circuit suspended in a strong magnetic field is exposed to radiation, is due entirely to M. d'Arsonval, and I need hardly say that it was in ignorance of the fact that he had preceded me that my communication was made to the Royal Society. As soon as I became acquainted with M. d'Arsonval's work, I took the earliest opportunity of admitting his claim to priority (see 'Nature,' vol. 35, p. 549).

I venture, however, to think that, although the differences between M. d'Arsonval's thermo-galvanometer and my radio-micrometer are essentially differences of detail, that even at the time of my original communication I had succeeded in producing the most sensitive instrument of practical utility, with the exception perhaps of the bolometer, which had up to that time been constructed for the measurement of radiant energy. As I hope to be able to show in the present communication that I have still further improved the proportions of the several parts, it may perhaps be fortunate that I was not aware that so able and ingenious a physicist had already made an instrument with the properties which I regarded as of so much importance, viz., the low resistance and small moment of inertia of the circuit, the small capacity for heat of the junction, the quickness and dead-beat character of the indications, and its freedom from extraneous influences. Had I known of M. d'Arsonval's work, I should probably have given no more attention to the idea.

15.3.89

It may, perhaps, be worth mentioning that I was led to attempt to make some improvement in the thermopile in consequence of the extraordinary sensibility which Professor LANGLEY had given to the bolometer, because it seemed incredible that, with so small a temperature coefficient of resistance as metals possess, an instrument depending on change of resistance should compare favourably with one equally well carried out, in which the comparatively large electromotive force of a thermo-electric junction is made use of.

The first object obviously was to reduce the mass of the exposed part of the pile. I was, therefore, naturally led to the use of fine wires or thin plates connected with a galvanometer in the usual way, but all the forms which such an instrument might take seemed to promise very little after the idea of the suspended circuit occurred to me. I, therefore, put them on one side, and devoted myself to the perfection of the instrument which forms the subject of the present paper. I notice, however, that there is an account of an instrument of such a kind in the January Number of the 'Philosophical Magazine' of this year (1888).

The considerations which led to the use of the general form of circuit, *i.e.*, one composed of a thin flat bar of antimony and bismuth, having its ends connected by a thin copper wire, which forms the remaining sides of a square or rectangle, are very simple.

A pair of metals must be chosen which have a high thermo-electric power, and which are not to any great extent magnetic, and which can be made exceedingly thin. Of ordinary metals antimony and bismuth so excel others in thermo-electric power that, unless they fail in other respects, they will be the best for the purpose. The diamagnetism is but a small disadvantage, and this may be overcome. The great density of the metals is objectionable; further, the difficulty of making the circuit increases as these metals are made thinner. The low conductivity for electricity of these metals is also a disadvantage, but this is balanced by their correspondingly low conductivity for heat. The disadvantages seem to be more than outweighed by the great thermo-electric power of the combination, if no attempt is made to complete the circuit with these metals only, but if copper is used for this purpose. In this way, the strongly diamagnetic metal is kept out of the intense part of the field; the high conductivity compared to its mass of copper, in which respect aluminium only is superior (but this cannot be soldered), is made use of to convey the current from one end of the bar to the other, round a circuit which may be of sufficient extent to enclose a large area in the magnetic field. Thus, the copper part of the circuit may have less weight and less resistance than it would have if made of antimony and bismuth.

As to the form of this hoop of copper, mechanical considerations determine that it shall be rectangular, for the pole pieces and central core could not conveniently be made to suit a circuit of other form; otherwise it would appear that a circular or elliptic form would be preferable.

I have attached the circuit to the lower end of a very thin capillary glass tube about 6 cm. long, which hangs at the end of a quartz fibre, made by the bow and arrow process described in the 'Philosophical Magazine' of June, 1887. Close to the

Fig. 1.

Fig. 2.

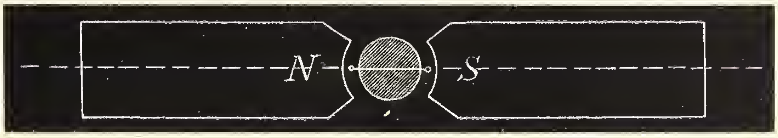
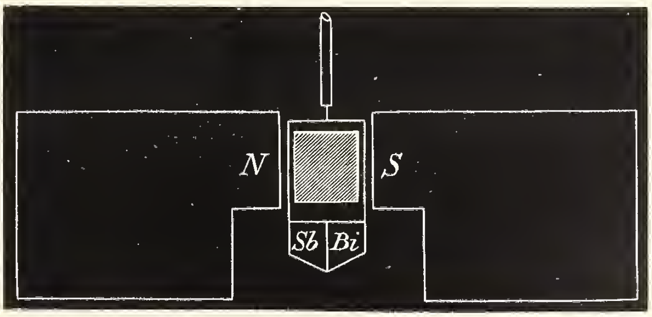


Fig. 3.



top of the glass tube is fastened a very light galvanometer mirror, *m*, so that the heat which may fall upon it shall have no influence on the junction lower down (see fig. 1).

The form of pole pieces which I have used is shown in figs. 2 and 3, which are a plan and a vertical section through the dotted line respectively. The central drum of iron, which serves to intensify and make more uniform the magnetic field, is shown shaded in these figures.

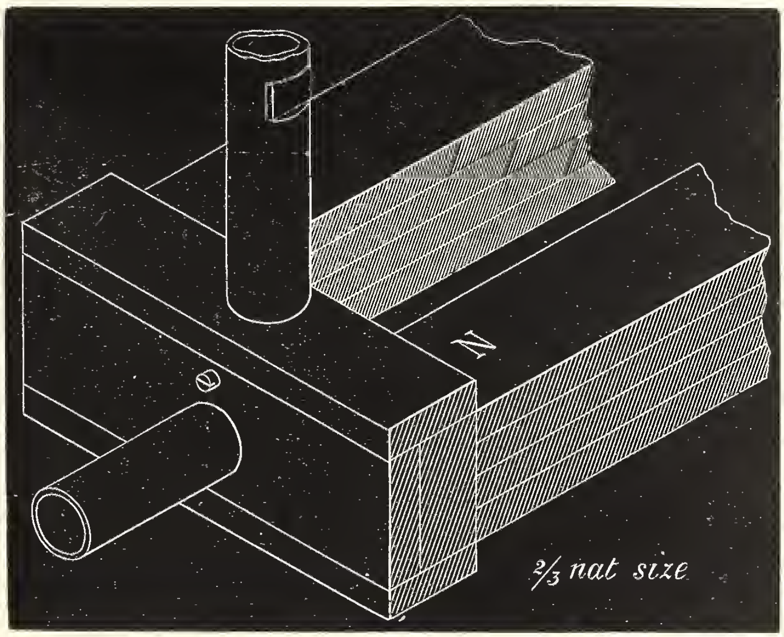
The copper hoop works in the annular space between the pole pieces and the central drum, where the field is most intense, while the active bar of antimony and bismuth hangs in the large cavity below the drum, where it is exposed to the radiation to be measured and where the weakness of the field prevents the diamagnetism from giving trouble. The pole pieces are screwed to massive brass plates, which cover the upper, front, and lower sides. The ends are left bare, as is the whole of the other side, which rests against the flat ends of a powerful horseshoe magnet. The remaining uncovered part between the poles of the magnet is covered with a piece of glass, which enables one, when levelling the instrument, to see if the circuit is free, or, when directing a spectrum upon the junction, to see that the desired colour is in its right place. It also protects the junction from air currents. Through the brass plate forming the front of the instrument, exactly opposite the junction, passes a brass tube open at the ends. This may carry a long tube, such as Professor LANGLEY used in the bolometer, fitted with gradually diminishing diaphragms, which effectually prevent air-currents from penetrating into the chamber. It should also carry, just in front of the active plate, a vertical slit, so as to confine the received radiation to the line of junction and as small a distance on either side as may be desired. A screw passing through the front plate holds the iron drum securely and truly in its place. The upper brass plate is pierced by a brass tube, about 31 cm. long, in which there is a window at the level of the mirror. There is a simple form of torsion head at the upper end of the tube which carries the quartz fibre (see fig. 4).

I should here point out in what respects my instrument differs from that of M. D'ARSONVAL. He makes his circuit of a pair of wires with the two junctions in the axis of motion, one above and one below. The wires are made of palladium and silver. The circuit is hung by a fibre of silk, and is directed by the action of the magnet on a small piece of iron wire attached to the circuit. *One of the junctions is protected by fixing over it the mirror which reflects the beam of light on to the scale.* The radiation to be measured is concentrated on to the other junction. M. D'ARSONVAL does not appear to have used for this purpose carefully formed pole pieces, but has simply hung the junction between the legs of a vertical horseshoe magnet, using a central hollow drum of iron within the circuit. He sometimes uses no central drum, but then he places the two wires very much nearer together, forming a long, narrow rectangle, which thus has a very small moment of inertia. He speaks of the great sensibility and quickness of his instrument, which also is dead beat.

In the preliminary note on the radio-micrometer ('Roy. Soc. Proc.,' vol. 42, p. 191), I pointed out in a provisional manner how the instrument may be made as perfect as possible by so choosing the length of the rectangle, the thickness of the copper wire, the number of turns or of junctions, the strength of the field, or the torsion of the fibre, that, if any of them are made more or less, the value of the instrument will be diminished. As I have now completed the calculations up to a point beyond which it would be difficult to go, and where I believe but little on which the perfection of the instrument practically depends remains to be found, I wish, without further delay, to explain the formulæ I have obtained.

Owing to the large number of variables, and the complicated manner in which they are involved, it would be difficult by any direct mathematical process to find the best value for every one at the same time; what I have done is to take the variations, one or two at a time, in such an order that those taken later shall require little or no modification of the results previously found.

Fig. 4.



It is evident that the quickness of the instrument, whatever proportions may be given to it, will increase as the sensitive plate is diminished in thickness; and later on it will be shown that, besides the quickness, the ultimate sensibility, *i.e.*, the deviation for a given rate of radiation, may also be increased as the thickness is diminished. It may, therefore, be taken as a fact that, the thinner the plate, the more sensitive will be the instrument. When I began the calculations I did not think it likely that a plate composed of metals so difficult to work could be made much less than $\frac{1}{4}$ mm. thick, and, accordingly, that has been assumed as one starting point. I have found, however, not the slightest difficulty in producing plates thinner than this, but I have in what follows taken this quantity as the thickness when wishing to find the numerical values given by the formulæ.

The next thing to determine upon is the general size of the circuit. If it is made large, the enclosed magnetic field will be increased; but, supposing the period of oscillation to remain invariable, the moment of torsion must be increased, and, further, the resistance may be increased. Both of these actions will reduce the sensibility. Let it be supposed that there are two instruments in all respects the same, except that the circuit of one is n times the length and is n times the width of that of the other, and let it be supposed that the thickness of the bar and of the wire of the circuit is the same in each case; the value of the enclosed magnetic fields will be as $n^2 : 1$; the moment of inertia, and therefore the moment of torsion, will be as $n^3 : 1$; and the resistance as $n : 1$. Therefore, $\frac{\text{included magnetic field}}{\text{torsion} \times \text{resistance}}$ will be as $1 : n^2$; that is, the angle of deflection will be n^2 times as great in the smaller as in the larger instrument. But there is a limit to the smallness, owing to two causes. The moment of inertia is made up of two parts, the circuit and the mirror, on which account, when the moment of inertia of the mirror becomes comparable with that of the circuit, the smaller instrument will have a greater moment of inertia, and, therefore, its sensibility on this account will be less than that given by the above rule.

The second reason why there is a superior limit to the sensibility as the instrument is reduced in size is due to the fact that the sensitive plate conducts more heat from the hot to the cold junction in the case of the shorter plate, and thus, for a given rate of radiation, the junction will not be so hot; and, hence, the current will be less than it would be if the diminished resistance were the only cause of change. It is, therefore, necessary to choose some length of plate or width of rectangle which can conveniently be made in practice, and, assuming this as a constant, to find by calculation the best values of all the variables, and so, as it were, to fit to the plate the best possible instrument. Something must be assumed as a starting point, and this seemed, on the whole, the most convenient. I have assumed the plate, then, to be composed of two squares of antimony and bismuth, each 5 mm. in the side and $\frac{1}{4}$ mm. thick, soldered edge to edge, thus forming a plate $10 \times 5 \times \frac{1}{4}$ mm. This assumption I have made simply for the sake of numerical calculation; any size may be equally well taken as the basis of operations, provided that in the equations which follow the proper numerical values are assigned to the constants.

Of the three dimensions of the plate, the length, the thickness, and the breadth, the first two only need be considered as assumptions which can in any way affect the result, for it matters not how the breadth be varied, provided that the sectional area of the wire and the moment of torsion are varied in the same proportion, *i.e.*, if the whole breadth is exposed to the radiant energy.

It will first be convenient to see how the circuit can be formed, so as to give the best results when the magnetic field is supposed constant; it will be seen later that this best is not the ultimate best when the field and the circuit are adapted to one another.

The circuit may be considered best from more than one point of view. It may be with respect to weight, or with respect to moment of inertia.

The best circuit with respect to weight would be that which would give the greatest deflection when supported by a particular bifilar arrangement free from torsion. The strength of the field, the thermo-electric power of the junction, and the value of the radiation, being constant factors, may be omitted for the present.

Let the following be the meanings of the several symbols that will be used :—

(·1481)	W.	The weight of the plate, mirror, and stem, the invariable or dead weight.
	w.	The weight of the hoop of copper, the variable weight.
($6·742 \times 10^6$)	C.	The resistance of the plate, the invariable or dead resistance.
	r.	The resistance of the hoop of copper, the variable resistance.
	l.	The length of the rectangle.
	n.	The number of turns of wire.
(·00895)	u'.	The weight of a unit piece of copper ($1 \times ·1 \times ·01$ cm.).
($1·642 \times 10^6$)	v.	The resistance of a unit piece of copper ($1 \times ·1 \times ·01$ cm.).
	a.	The sectional area of the wire ($·1 \times ·01$ being considered unit area).

The reason for taking 1/1000th of a square centimetre as the unit of sectional area for the wire is that this is not very different from the actual sizes that will be required, and that it avoids the absurdity of supposing a hoop of wire of such a size made of wire of 1 sq. cm. in sectional area. It is a matter of convenience, and nothing more. The mirror that I have used is 6 mm. in diameter, and weighs ·04 grm. The glass stem may be taken as ·005 grm. The weights and resistance of the three metals are taken from LUPTON'S Tables, and the numerical values of the several quantities so calculated are enclosed in parentheses before their respective symbols. All the quantities, except when otherwise specified, are in C.G.S. units.

It is necessary to bear in mind that a certain excess of wire may be required, over and above that actually necessary to reach the active bar. Though in the numerical examples which follow I have not allowed for any, I have taken care in the equations to introduce a symbol, p, which must be explained. Imagine a circuit made of one turn 1 cm. wide and l cm. long: then, if there is no excess, the amount of wire will be $2l + 1$; but, if there is an excess, then the amount will be $2l + 1 +$ excess. I have used the symbol p in the following calculations for $1 +$ excess, and I have assumed that both the resistance and the weight vary with the total length. The only possible discrepancy can be due to the manner in which the current leaves the wire for the plate.

Considering, first, the case of a circuit of only one turn of wire, the variable resistance r and weight w of the wire will be

$$r = (2l + p)\, v \div a,$$
$$w = (2l + p)\, u' \times a.$$

The conductivity G of the whole circuit will be

$$G = \frac{a}{(2l + p)\, v + aC}.$$

The efficacy of the circuit with respect to its weight, E_{wt}, *i.e.*, the moment which it can exert upon a unit field for every gramme that it weighs when unit E.M.F. is acting at the junction, is

$$E_{wt} = \frac{l \times G}{W + w} = \frac{la}{\{(2l + p)\, v + aC\}\{W + (2l + p)\, au'\}}.$$

Now, it is evident that there must be a maximum value for E_{wt}, both when l and when a is varied, for, if either is made very great or very small, more is lost than is gained. If, therefore, the expression for E_{wt} is treated in the usual way to find the two maxima, the result will be found to be

$$\text{best } a = \sqrt{\frac{Wv}{u'C}} \quad \ldots \ldots \ldots \ldots \quad (1),$$

$$l^2 = \frac{(W + pau')(pv + aC)}{4vau'};$$

or, substituting the value of a above, it will be found that

$$2l = p + \sqrt{\frac{CW}{u'v}} \quad \ldots \ldots \ldots \ldots \quad (2).$$

If, further, the circuit is supposed to have n turns, the corresponding expressions will be

$$a = \sqrt{\frac{Wv}{u'C}},$$

$$2nl = (2n - 1)\, p + \sqrt{\frac{CW}{u'v}} \quad \ldots \ldots \ldots \quad (3).$$

Thus, the size of wire that is most suitable is independent of the length of the rectangle or of the number of turns. The numerical values of these quantities for 1 . . . 5 turns are as follows :—

n.	Best l.	E_{wt}.	Best a.
1	4·621	$9·202 \times 10^{-7}$	2·0075.
2	2·811	7·565 ,,	
3	2·207	6·423 ,,	
4	1·905	5·580 ,,	
5	1·724	4·933 ,,	

Thus, it appears that one turn is better than any other number, provided that the circuit may have sufficient length, and, of course, that the magnetic field is sufficiently extended. This result is really self-evident, for, whatever may be the efficacy of a circuit of say two turns of the best length, one of one turn of twice the length must be better, as in this case the value of the enclosed magnetic field will be the same, while the resistance and the weight will each be less, and, further, the circuit of twice the length will not have the best length for one of one turn only.

If the length of the circuit is limited to 1 cm., then two turns are better than one or three. The series of figures found on this supposition are not of sufficient importance to be worth giving.

It is interesting, however, to notice how slowly the efficacy changes when the length of the circuit is not the best. The following figures show this :—

l.	E_{wt}.	
3	$8·742 \times 10^{-7}$	
3·5	9·036 ,,	
4	9·152 ,,	
4·5	9·200 ,,	
4·621	9·202 ,,	max.
5	9·190 ,,	

Thus, it is not a matter of much consequence whether the circuit is very near the best length or not.

If it is desired to find the numerical value of the resistance or the weight of the wire part of the circuit when it is of the best length and sectional area, the following expressions, which have been obtained by substituting in those for r and w the best values found for a and l, may be used to save time :—

$$\text{best } r = C\left(1 + 2p\sqrt{\frac{u'v}{CW}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

$$\text{best } w = W\left(1 + 2p\sqrt{\frac{u'v}{CW}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

From these it is seen that that length and size of wire is best of which the weight as much exceeds the dead weight as the resistance exceeds the dead resistance. In the same way, the efficacy of the best circuit may be shown to be

$$\text{best } E_{wt} = \frac{1}{8\sqrt{(\mathrm{CW}u'v)} + pu'v} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

If, finally, the breadth be made a variable, the following equations will give the best conditions :—

As before,

$$\text{best } a = \sqrt{\frac{wr}{u'\mathrm{C}}};$$

and this is true whatever length, number of turns, width, or even shape the circuit may have. When b is greater than 1,

$$\text{for the best length, } 2l = 2b - 1 + e + \sqrt{\frac{\mathrm{CW}}{u'v}},$$

$$\text{for the best breadth, } 2b = 2l - 1 + e + \sqrt{\frac{\mathrm{CW}}{u'r}}.$$

where e is the excess used for soldering.

Therefore, for any breadth the length must exceed the breadth by as much as the breadth should exceed the length when that is given. In other words, the circuit is improved by adding to it *ad infinitum*, so that a square of infinite size is the best rectangle. If it should happen that $\sqrt{(\mathrm{CW}/u'v)} + e < 1$, then the circuit in the same way would be made worse by increasing the dimensions. As a matter of fact, in the particular case, taking e as 0, this quantity is equal to 8·24.

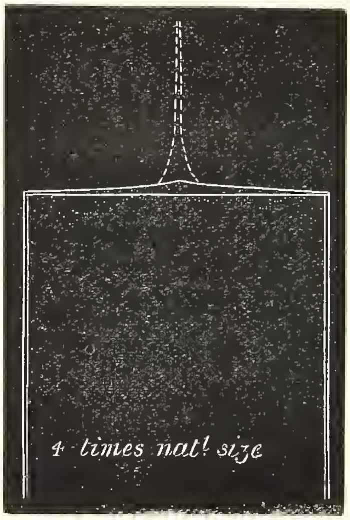
If, on the other hand, b is less than 1, then the quantity $2b - 1 + e$ in the two equations above must be replaced by $b + 1 + e$, a quantity necessarily positive; hence, whether b is less than or greater than 1, the circuit cannot be too large.

If the same process that has been followed in finding the best conditions with respect to weight be employed to find them with respect to moment of inertia, a difficulty arises in consequence of the fact that the upper end or cross wire of the circuit has a resistance which depends upon its length simply, while it has a moment of inertia which is only one-third of what it would have if it were placed alongside of one of the side wires of the circuit. Thus, while the expression for r remains as before, viz., $(2l + 1 + e)\, v \div a$, that for the moment of inertia of the wire will be $\{(6l + 3c + 1)/12\}\, u'a$. If with these values the attempt is made to find the best values for a and l with respect to moment of inertia, a complicated cubic equation results and symmetrical expressions can no more be obtained. If, however, the two coefficients in parentheses had the same value, there would be no difficulty.

While trying to find a remedy for this difficulty, I noticed that a wire of uniform section is not the best form of conductor when the moment of inertia is taken into account, *i.e.*, of that part of the wire which crosses the axis. That this is so is evident in two ways. Since a wire of uniform section has the least resistance for a given length and weight, it cannot have the least resistance for a given length and moment of inertia, except when it is parallel to the axis of rotation. Or, considering the cross piece only, since the uniform section is that which gives the least resistance for its weight, it is clear that a very small change of form, such for instance as would occur if a thin skin were removed from the outer and were transferred to those portions nearer the axis of rotation, would produce no appreciable change in resistance, but it would reduce the moment of inertia; therefore, there is an advantage in having those parts near the axis thicker than those more distant from it. If we suppose that the resistance of any element of the wire between the axis and the ends is inversely proportional to the cross section, and that the resistance of the whole piece is the sum of the resistances of the several elements, which is true as long as the rate of change of section is not so rapid as to make the stream lines notably inclined, then the best variation of section will be that in which no change will be made in the resistance ÷ the moment of inertia if a thin skin be transferred from one part to another. Taking the axis as origin of rectangular coordinate, and calling the sectional area y, then for all values of x, $1/y \div yx^2$, *i.e.*, $1/y^2x^2$ must be constant or y must vary inversely as x. It thus appears that, if the hoop is cut out of sheet copper instead of wire, that part which forms the upper bar should be bounded by two hyperbolas of such dimensions that the width at the ends of the cross bar is the same as that of the side pieces, which are uniform. If the metal is chosen of such a thickness, t, that the best section of the sides hereafter to be found $= t \times \cdot 1$ mm., then the error due to the inclination of the stream lines already pointed out will not be appreciable, except within about $\frac{1}{2}$ mm. on either side of the axis. It will be well at once to point out that the value of this supposed form for the cross bar does not at all lie in the fact that it is the best form, for nothing worth consideration would be gained by adopting it, but the reason for bringing it forward is this: the total resistance of either half of the cross bar will be, if the resistance of the same length of the wire be called 1, $= \int_0^1 x\,dx = \frac{1}{2}$; the moment of inertia of either half will be, if that of the same length of the side be called 1, $= \int_0^1 x\,dx = \frac{1}{2}$. That is, not only has the best form been found for the cross bar, but the coefficients for moment of inertia and for resistance have at the same time been made identical, and thus all the expressions found with respect to weight will equally apply with respect to moment of inertia.

It may be objected, since, as already mentioned, the resistance of these parts close to the axis is greater than is supposed, on account of the inclination of the stream lines and the consequent concentration of the current in the more direct line, or again, since the weight of a piece of metal filling the space between two hyperbolas is infinite, that the solution for the difficulty thus put forward is not correct, and that any further calculations based upon this result will not be trustworthy. The answer evidently is that, if the axial spike be removed altogether, as shown in fig. 5, the actual resistance and moment of inertia would not be so much as 1 per cent. different from the value $(\frac{1}{2})$ already found, and that, as these calculations are merely a guide to direct the design of a practical instrument, an assumption which is not more than 1 or 2 per cent. in error in what is after all a small fraction of the total resistance and moment of inertia—one which, in fact, is so true that neither the power of the instrument maker nor our knowledge of the constants will prevent our introducing in practice variations of far greater magnitude—may be allowed to pass, especially as without something equivalent it would be impossible to find expressions which would apply to the more complicated conditions which will be considered later.

Fig. 5.



The same considerations apply exactly to the sensitive plate, which, when the greatest efficacy with respect to moment of inertia is required, should no longer be made prismatic, but should become more narrow towards the ends, as shown in fig. 1. I may mention that in the original instrument, which I made before I had arrived at any of these results, I did, as a matter of fact, make the active bar of the shape shown, because I felt that the lower corners were doing more harm by their moment of inertia than they were doing good by their conductivity.

Since the active plate is taken as one of the invariables of the circuit, any proportions that may be desired may be taken for it, and the resulting resistance and moment of inertia made use of in calculating numerical values. I have in the arithmetical work which follows taken the active plate to be of the original shape and size, that is, a rectangular prism, $10 \times 5 \times \cdot 25$ mm. I have further not made any allowance for excess of metal for soldering, *i.e.*, p has been taken as the coefficient of resistance and moment of inertia of the upper bar only, which has been found to be equal to $\frac{1}{2}$, just as, when the conditions with respect to weight were considered, p was taken as equal to 1.

As in the rest of this paper the best conditions with respect to weight will no more be considered, but only those relating to moment of inertia, it will be well here to specify the meaning of the several symbols that will be used. The arithmetical values of the fixed quantities are, as before, included within parentheses.

$(9{\cdot}49 \times 10^{-3})$	K.	The moment of inertia of plate, mirror, and stem, the dead moment of inertia.
	k.	The moment of inertia of the copper hoop, the variable moment of inertia.
$(6{\cdot}742 \times 10^{6})$	C.	The resistance of the plate, the dead resistance.
	r.	The resistance of the copper hoop, the variable resistance.
	l.	The length of the rectangle.
	n.	The number of turns of wire.
$(2{\cdot}2375 \times 10^{-3})$	u.	The moment of inertia of the unit piece of copper $(1 \times {\cdot}1 \times {\cdot}01)$ at 5 mm. from the axis.
$(1{\cdot}642 \times 10^{6})$	v.	The resistance of the unit piece of copper.
	a.	The sectional area of the wire (${\cdot}1 \times {\cdot}01$ being considered unity).

Since the formulæ already given are now equally true with respect to moment of inertia, it will be unnecessary to do more than barely state them here for future reference.

For best sectional area (n turns) $$a = \sqrt{\frac{\mathrm{K}v}{u\mathrm{C}}} \quad \ldots \ldots \quad (7),$$

for best length (n turns) $$2nl = (2n - 1)p + \sqrt{\frac{\mathrm{KC}}{uv}} \quad \ldots \ldots \quad (8),$$

for best r (one turn) $$r = \mathrm{C}\left(1 + 2p\sqrt{\frac{uv}{\mathrm{CK}}}\right) \quad \ldots \ldots \quad (9),$$

for best k (one turn) $$k = \mathrm{K}\left(1 + 2p\sqrt{\frac{uv}{\mathrm{CK}}}\right) \quad \ldots \ldots \quad (10),$$

for best E_k (one turn) $$\mathrm{E}_k = \frac{1}{8\,\{\sqrt{(\mathrm{CK}\,uv)} + puv\}} \quad \ldots \ldots \quad (11).$$

$p = \frac{1}{2} +$ any excess allowed for soldering.

The following arithmetical results are obtained from some of these expressions, making $p = \frac{1}{2}$:—

n.	Best l.	E_k.	Best a.
1	2·3366	$7·1863 \times 10^{-6}$	1·0174.
2	1·4183	3·4222 ,,	
3	1·1122	2·1208 ,,	
4	·9591	1·5014 ,,	
5	·8673	1·1500 ,,	

Thus, the best length and the best sectional area with respect to moment of inertia are each just over half the values found with respect to weight. One turn, of course, gives a greater efficacy than any other number, just as it did before, and for the same reason, but in this case one turn is more than twice as good as two, whereas it was only about one-fifth in excess.

The result at present obtained, then, is this: provided a torsion fibre of convenient length can be obtained which shall in every case produce a certain pre-determined period, say 10 seconds, which, with my process for quartz, is possible even when the hanging body is far lighter and smaller than any galvanometer mirror, then the moment of torsion and the moment of inertia will always have the same ratio; therefore, the circuit which produces the greatest couple for its moment of inertia will also produce the greatest couple compared with the torsion, and will, therefore, give the greatest deflection. This circuit has been found above to have a length of 2·3366 cm., and to be made of copper, having a sectional area of ·0010174 sq. cm. at the sides.

Though this circuit has the best length and the best sectional area under certain magnetic conditions, it is not necessarily the best when these are varied.

It has, of course, been understood that in the comparison between long and short circuits the magnetic field is in both cases the same, and is uniform throughout the whole of the area enclosed by the circuit. This may or may not be true when an internal pole piece is arranged as shown in figs. 2 and 3. There must be some clearance above and below; thus, the internal drum is always less than the actual length. It is therefore probable that the effective area is proportional to the length, less a small constant quantity. If this defect is equal to the sum of the clearances above and below the drum, then the length of the drum must be called l in the preceding formulæ, and four times the clearance added to p. It is not possible to say exactly what is the error thus introduced into the calculations, but in no case can it be important. This, however, is a mere detail compared with the effect of very strong magnetism.

When a closed circuit oscillates under the conditions in which that in the instrument is placed, induced currents are formed which oppose the motion, and thus, if the field is strong enough, or the conductivity great enough, the oscillatory character ceases, and the circuit slowly moves towards its resting place, more slowly as it approaches it. It is a great advantage in an instrument that it should in this way be dead beat; but, if the field is more than strong enough, the extra resistance to the motion so increases the time of coming to the resting place that the loss of time more than counterbalances any advantage given by the increased ultimate sensibility. If the ultimate sensibility is required to be made a maximum, then the expressions found are the best, and, the stronger the field, the better; but, if the best combination which is dead beat (and no more than dead beat) is required—and this is what any one who has used both would require—then the length and the sectional area already found are not the best, unless the strongest field which can conveniently be employed is not strong enough to make the motion dead beat. In the particular case it is more than strong enough.

It is necessary, therefore, to introduce the effect of another variable, the strength of the field, the relation between it and the rest of the circuit being such that the motion is just dead beat.

It is well known that motion ceases to be oscillatory when half the coefficient of resistance to the motion is equal to the square root of the acceleration when the angular displacement is unity. In the investigations depending on these relations the following symbols will have the meanings attached to them :—

S. Sensibility, *i.e.*, efficacy $\times$ enclosed magnetic field.

A. Area enclosed by the circuit.

ω. Angular velocity.

T. Time of a complete undamped oscillation.

G. Conductivity of the whole circuit.

α. Angle included between the plane of the circuit and the direction of the lines of force, supposed parallel to one another.

κ'. Moment of inertia of the whole circuit, *i.e.*, $K + k$.

H′. The minimum strength of magnetic field for which the motion is dead beat. This will hereafter be called the dead beat magnetic field.

The resistance to the motion of the circuit

$$= G \times H^2 \times A^2 \times \omega \times \cos^2 \alpha \,;$$

half the coefficient of resistance $= GH^2A^2 \cos^2 \alpha/2\kappa'$.

Since the magnetic field is radial and is everywhere cut normally by the side wires of the circuit, the factor $\cos^2 \alpha$ ought to be omitted. Owing to the small possible angular deflection, it could not in any case differ appreciably from 1.

$$\sqrt{}(\text{acceleration at unit angle}) = \frac{2\pi}{T}\cdot$$

Since, when the motion is just dead beat, half the coefficient of resistance is equal to $\sqrt{}$(acceleration at unit angle),

$$\frac{GH'^2A^2}{2\kappa'} = \frac{2\pi}{T},$$

or

$$H' = \frac{2}{A}\sqrt{\frac{\pi}{T}} \cdot \sqrt{\frac{\kappa'}{G}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

$$S = \frac{GAH'}{\kappa'} = 2\sqrt{\frac{\pi}{T}} \cdot \sqrt{\frac{G}{\kappa'}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13).$$

On differentiating the expression for S with respect to a (the sectional area of the wire) and equating to 0, the maximum sensibility will be found when a has the same value $\sqrt{(Kv/uC)}$, which gave the maximum efficacy; on the other hand, the differential coefficient with respect to l (the length of the rectangle of copper) is negative for all positive values of l, that is, however short l may be, provided that the magnetic field may be made strong enough to keep the motion dead beat, a still greater sensibility will be given by a shorter circuit. If the strongest magnetic field conveniently available is not sufficient to make the circuit having maximum efficacy dead beat, then that circuit is still the best. If, however, as will be the case with an ordinary magnet, the field is more than strong enough, then the length of rectangle must be reduced until the motion is deat beat. Taking the particular arrangement already referred to, in which the circuit is 1 cm. wide and is made of wire having the best sectional area (·001017 sq. cm.), the following are the values of the dead beat magnetic field for various lengths of circuit :—

								Best.
Length . .	·2	·4	·6	·8	1·0	1·5	2·0	2·3366
H′ . . .	1722	929	664	532	455	347	294	271·8

It is interesting here, in the expression for H′ (12), to substitute those values of κ' and G which belong to the circuit of greatest efficacy. If this is done, there finally results the equation

$$H'' = 8\sqrt{\frac{\pi}{T}}\sqrt{uv} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12a),$$

where H″ is the dead beat magnetic field for the circuit of greatest efficacy. It thus appears that, no matter what the resistance of the antimony-bismuth bars, or what the moment of inertia of these bars, the mirror, and stem may be, provided that the circuit is so formed as to produce the greatest ultimate sensibility in any given field, the motion will be dead beat for one particular strength which simply depends upon the specific gravity and the specific resistance of the material with which the circuit is completed.

The sensibility obtained by such a combination is

$$S = \sqrt{\frac{\pi}{T}} \cdot \frac{1}{\sqrt{CK} + p\sqrt{uv}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13a).$$

As has already been shown, this is not the best arrangement to make use of. That is best in which the circuit is the shortest which will remain dead beat in the strongest magnetic field available.

In the original instrument, of which figs. 2 and 3 show the pole pieces, a strong compound horseshoe magnet was used. The working field was tested by replacing the active circuit by one composed of 50 turns of 1 sq. cm. each of the finest insulated copper wire. This was mounted so that it could be suddenly twisted through a definite angle by moving an arm between a pair of stops. The ends of the coil were connected with a ballistic galvanometer in a distant room, and the throw observed. The resistance of the whole circuit was measured. Then, in the place of the coil, a condenser of known capacity and a cell of known E.M.F. were arranged with a key so that a definite discharge of electricity could be sent through the galvanometer. The absolute value of that sent by each oscillation of the coil could thus be determined, and hence the value of the field found. In this way it was found that without the keeper a field of 1342 units existed in the working space.

It appears, then, from the table on the previous page, that with such a field the circuit should be about 3 mm. long. I did not actually alter the shape of the circuit, but adjusted the field by means of a sliding armature, until the sensibility, or the resistance to the motion, produced a convenient result. As has been shown, this is not so good a plan as adapting the circuit to the strongest field that is available, though less is lost by reducing the field than might be expected, as will be explained later.

If the breadth b that will give the greatest efficacy with respect to moment of inertia is required, there is no difficulty in finding the best a and l in terms of b, but the best b is involved in an expression of such complexity that it can only be found by arithmetical means.

It is not worth while to give at length the table showing the successive values of the efficacy, as b varies; it is sufficient to state that not only is the efficacy diminished by increasing the breadth beyond that of the active bar, but it is even increased as the breadth diminishes down to 2 mm., and probably far beyond.

The conclusion, then, is obvious, that not only should the rectangle be as narrow as possible, but the junction should be arranged also in a correspondingly narrow form. Further, in consequence of the extreme narrowness of the circuit, the resistance and moment of inertia of the cross wire may practically be neglected in comparison with the now much greater length of the rectangle. I must here remark that I have thus been brought to the adoption of the excessively narrow form which M. D'ARSONVAL has used. I do not know whether he was aware that the narrow form is not only far quicker than a wide form, which is the reason he gives for adopting it, but that it is in addition, when a convenient period is arranged, also more sensitive. I certainly did not expect to find it so.

Making use of the same methods and symbols, but neglecting the now infinitesimal effect of the cross wire, the following equations will be found to hold:—

$$\text{best} \quad a = \frac{1}{b}\sqrt{\frac{\mathrm{K}v}{u\mathrm{C}}} \quad \ldots \ldots \ldots \quad (14),$$

$$\text{best} \quad l = \frac{1}{2b}\sqrt{\frac{\mathrm{KC}}{uv}} \quad \ldots \ldots \ldots \quad (15),$$

$$\text{greatest} \quad \mathrm{E}_k = \frac{1}{8\sqrt{(\mathrm{KC}uv)}} \quad \ldots \ldots \ldots \quad (16).$$

Further, it will be found that

$$\mathrm{C} = \frac{2lv}{a} \quad \text{and} \quad \mathrm{K} = 2\,laub^2,$$

and thus the copper hoop must be so proportioned that its resistance may be equal to the dead resistance, and its moment of inertia to the dead moment of inertia. It is also found that under the supposed circumstances, namely, that the cross wire is of no account, the efficacy is independent of the breadth, and the only effect of an increased breadth is to require a diminished sectional area of wire and a diminished length; thus, during variation of the breadth neither the resistance nor the moment of inertia of the copper wire is changed.

The expression for the dead beat magnetic field, when the circuit of greatest efficacy is used, is, as before,

$$\mathrm{H}'' = 8\sqrt{\frac{\pi}{\mathrm{T}}}\sqrt{uv} \quad \ldots \ldots \ldots \quad (17);$$

and this seems to be very generally true.

The sensibility under these conditions is

$$\mathrm{S} = \sqrt{\frac{\pi}{\mathrm{T}}} \cdot \frac{1}{\sqrt{\mathrm{KC}}} \quad \ldots \ldots \ldots \quad (18),$$

which shows that, no matter what the material of the hoop may be, if the dimensions which give the greatest sensibility in any given magnetic field are employed, and if the field is so strong that the motion is dead beat, the sensibility will always be the same, and this will only depend on the resistance and moment of inertia of the invariable part of the circuit.

Since, as in the case of the wide circuit, the magnetic field that will make the motion of the best narrow circuit dead beat is far less than that which is available, it will, as before, be best to employ a circuit so much smaller than the best as will just make the motion dead beat.

The circuit may be made smaller by reducing either factor, the length l or the breadth b. As has been shown, it is indifferent whether one or other factor is altered, provided that b is small compared with l. Let the product be made N times as small, so that bl becomes bl/N, then the first factor $2/bl$ in the expression for H′, which it is convenient to repeat here in another form,

$$\left(\mathrm{H}' = \frac{2}{bl}\sqrt{\frac{\pi}{\mathrm{T}}}\sqrt{\kappa'\mathrm{R}}\right),$$

will become $2\mathrm{N}/bl$. But when bl is changed, both κ', the total moment of inertia, and R, the total resistance, are changed also. In the circuit of greatest efficacy the resistance and moment of inertia of the hoop are each equal to the fixed or invariable resistance and moment of inertia; and thus, when bl becomes bl/N, one half of each κ' and R also becomes one-N$^{\text{th}}$ of what it was, the other half of each remaining unchanged, and thus the new expression for H′ becomes $\frac{\mathrm{N}+1}{bl}\sqrt{\frac{\pi}{\mathrm{T}}}\sqrt{\kappa'\mathrm{R}}$, that is $\frac{1}{2}(\mathrm{N}+1)$ times what it was. Therefore, if it is desired to make the dead beat magnetic field M times that which has been found for the circuit of greatest efficacy, the product bl must be reduced until it is $2\mathrm{M}-1$ times as small, for $\mathrm{N} = 2\mathrm{M}-1$.

By this process the actual sensibility is increased, and the amount of increase may be found as follows:—The sensibility of any combination varies directly as the magnetic field and as the product bl, and inversely as the total moment of inertia and the total resistance; of these four quantities it has just been shown, that when bl is divided by N, the dead beat magnetic field is multiplied by $\frac{1}{2}(\mathrm{N}+1)$, and at the same time R and κ' are each multiplied by $(\mathrm{N}+1)/2\mathrm{N}$; therefore, the sensibility of the arrangement becomes

$$\frac{\mathrm{N}+1}{2} \times \frac{1}{\mathrm{N}} \times \frac{2\mathrm{N}}{\mathrm{N}+1} \times \frac{2\mathrm{N}}{\mathrm{N}+1}, \quad \text{that is,} \quad \frac{2\mathrm{N}}{\mathrm{N}+1},$$

or $2-(1/\mathrm{M})$ times what it was, so that

$$\mathrm{S} \text{ becomes } \left(2-\frac{1}{\mathrm{M}}\right)\sqrt{\frac{\pi}{\mathrm{T}}}\frac{1}{\sqrt{\mathrm{KC}}}.$$

Since the dead beat magnetic field for the circuit of greatest efficacy is about 272 units, N must be so chosen as to make H′ four or five times as great. Assuming that H′ is to be increased to four times its original value, or that $\mathrm{M} = 4$, the sensibility will only become $1\frac{3}{4}$ times what it was. Even in the case of an infinite field, it cannot be more than double that due to a field of 272 units if the motion is only just dead beat. From this it appears that, as long as the circuit has dimensions which at all approximate to those which theoretically are best, the sensibility obtained by moving the pole pieces until the dead beat conditions or the desired logarithmic decrement are produced is practically the highest which is possible.

Owing to the fact that, with increase in the breadth of the circuit, the cross piece becomes increasingly mischievous, both on account of its moment of inertia and of its resistance, it is clear that the circuit cannot be too narrow until the increased length becomes such that it is inconvenient to provide a magnet and pole pieces which will enclose so great a length. In another way the thick wire which the narrow circuit requires is advantageous, as will appear shortly.

Having thus found the best relations between the variable copper and the arbitrary junction and mirror, it remains to see how these may be modified with advantage.

As, with the narrow form of circuit, the smallest galvanometer mirror has a moment of inertia many times as great as that of the active bars, and since the copper must have a moment of inertia equal to their sum, it is evident that it will be advantageous to reduce the dimensions of the mirror until it again becomes small in comparison. By this reduction the defining power of the mirror, supposed optically perfect, is also reduced, and thus there must be a limit at which as much is lost by the increasing want of definition as is gained by the diminishing moment of inertia.

The defining power of a perfect mirror—and the smaller the mirror the more likely it is to be perfect—varies with its diameter, while the moment of inertia is proportional to the fourth power of the diameter when the thickness is constant, or to the fifth power if the thickness is also proportional to the diameter. To find the best diameter it is necessary to remember that the fixed moment of inertia K is the sum of the moment of inertia of the junction K_j and of the mirror K_m. Thus, the accuracy of observing a deflection $= K_m^{1/n}/2\,(K_m + K_j)$, where $n = 4$ or 5 as the case may be. The best size of mirror then will be such that

$$K_m = \frac{K_j}{3}; \qquad k = \frac{4K_j}{3} = 4K_m, \text{ when } n = 4,$$

or that

$$K_m = \frac{K_j}{4}; \qquad k = \frac{5K_j}{4} = 5K_m, \text{ when } n = 5.$$

Using the thinnest microscope cover glass, about ·1 mm. thick, it will be found that the size of mirror which gives the best result when the antimony-bismuth bars have the dimensions which will be assigned to them hereafter is one having a diameter of $2\frac{3}{4}$ mm.

I have picked out a number of discs sufficiently thin, silvered them, cut pieces of the proper size, and then examined them by reflection. With mirrors as small as this, the eye itself takes the place of the usual telescope, and it is easy to choose those which allow the eye to see by reflection fine distant lines as clearly as if the light came direct. The only difficulty I have had in attaching these mirrors to the stem arises from the bending of the glass under the action of even the smallest quantity of cement. If sealing-wax is used—and this is the least magnetic of all the cements I have examined—a speck less than 1 mm. in diameter will, by its capillarity when melted, so distort the glass, even though it only touches near one edge, as to make a double image clearly visible. All difficulty is overcome by using instead the smallest visible quantity of shellac varnish, and applying heat to a certain extent. Of course the light reflected from so small a mirror is not sufficient when the usual paraffin lamp is employed, but, with oxygen at its present price, there is no reason why a small lime light should not be used. I have found that with even a small supply of oxygen the light on the scale is abundant, and there is no difficulty in observing a deviation of $\frac{1}{4}$ mm. The theoretical defining power upon a scale a metre distant of a mirror $2\frac{3}{4}$ mm. in diameter is about ·23 mm.

The image of the cross wire given by a mirror of this size that was used in the radio-micrometer shown to the Royal Society was a sharp line which could be read with an accuracy of $\frac{1}{10}$ mm.

As the little mirror is plane, I have cemented a plano-convex lens of a convenient focus in the place of the usual plane glass window which must be used to protect the moving parts from currents of air. This is preferable to a double convex lens, because the flat surface is more convenient for cementing, but especially because this surface by reflection also throws an image on the scale which is invariable in position, and which may be used as a reference mark if the scale is moved.

As the definition of the mirror is still so good that the power of reading a deflection in the ordinary way is not materially reduced, no change will be practically necessary in the series of equations 14–18, which are only strictly applicable when the defining power is not affected by change of K.

The junction is the only part of the suspended portion of the instrument which now remains arbitrary. I have provisionally assumed, for the sake of arithmetical results, a pair of bars of antimony and bismuth $5 \times 1 \times \frac{1}{4}$ mm. fixed parallel to one another at a mean distance apart of 1 mm. A less mean distance is impracticable, though it would be an advantage; but the length and sectional area may be modified if found necessary.

Before considering the effect of varying the proportion of the antimony-bismuth bars, it will be convenient at this point to find numerically the value of the combination (see fig. 6) which has thus been developed.

They are as follows :—

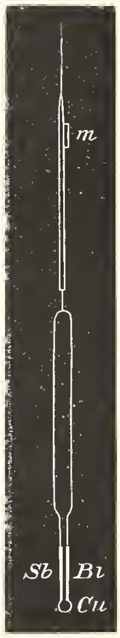
$$\left.\begin{aligned} a &= \frac{1}{b}\sqrt{\frac{\mathrm{K}v}{u\mathrm{C}}} = \cdot000387 \text{ sq. cm.} \\ l &= \frac{1}{2b}\sqrt{\frac{\mathrm{KC}}{uv}} = 3\cdot970 \text{ cm.} \end{aligned}\right\} \text{if } b = 1 \text{ mm.},$$

$$\mathrm{E}_k = \tfrac{1}{8}\frac{1}{\sqrt{\mathrm{KC}uv}} = 4\cdot284 \times 10^{-5},$$

$$\mathrm{H}'' = 8\sqrt{\frac{\pi}{\mathrm{T}}}\sqrt{uv} = 271\cdot8.$$

Now, assuming that the working field is four times that found, then the product bl must be made one-seventh of that stated above, and the product $E_k \times H''$, which

Fig. 6.



represents the available sensibility, must be made $1\frac{3}{4}$ times as great. It is satisfactory to find that the greatest efficacy, and therefore sensibility, is with the narrow circuit about six times as great as that found for the wide circuit first considered.

There is no reason why the exact dimensions assigned to the active bars should be employed; it will be well, therefore, to consider what will be the effect of using bars of other dimensions.

Let the sectional area be supposed increased in the ratio $1 : n$; then K will become nK, and C will become C/n; therefore, the greatest efficacy which depends on the product of these will be unchanged, but this assumes a constant difference of temperature between the ends of the bars. Now, in the case of the increased sectional area, the radiation of heat upon the warm junction will be unchanged, while for a given temperature difference the flow of heat to the cool junction, both on account of ordinary heat conduction and the PELTIER action of the current, will be increased, and, thus, the warm junction will not become so warm; thus, the actual sensibility will be less. The loss of heat by radiation from the bars can only be less in consequence of the warm junction being cooler, and, thus, the conclusion remains true, that there is no limit, except that imposed by the difficulty of working the materials, to the smallness of the sectional area that should be used. I may say here that I have found no great difficulty in making the bars as little as $\frac{1}{8}$ mm. thick, and in making perfect soldered joints where the weight of solder used does not exceed the fifth part of a milligram.

But to do this the ordinary methods must be discarded, and special means and special tools devised, when the difficulty becomes greatly reduced. Though full instructions would be interesting to the few who would ever probably care to make circuits of this extreme fineness, I do not think a detailed account of the manipulation would be suitable for insertion in this paper. I may, however, mention two causes, ordinarily of no great moment, which become, under the peculiar circumstances, of the first importance. One is the apparently instantaneous conduction of heat, and the other the surface tension of melted solder, which, unless provided against, will produce troublesome and unexpected results. There is no need to do more than mention the fusibility of the bismuth in the presence of the melted solder.

If the length of the bars be supposed increased in the ratio of $1:n$, K will become nK, and C will become nC; thus, since $E \propto 1/\sqrt{CK}$, E will become n times as small. But here again less heat will reach the cool junction with the larger bars, both by conduction and by the PELTIER action of the current. Thus, the warm junction will become warmer, and the cool junction colder: now, should the temperature difference become also n times as great, the actual sensibility of the circuit would remain unchanged. If no heat were radiated from the bars, then the temperature difference would be proportional to n, and the actual sensibility independent of n; but, on account of the radiation which must occur, the temperature difference would vary in a less ratio than $1:n$, and therefore the bars could not be too short until the cold junction became sufficiently near the hot junction for it to be impossible to prevent the radiant heat from falling on it also.

On the other hand, on account of the increased flow of heat with the shorter bars, the cool junction would be made warmer, and the whole junction would therefore become warmer, and so there would be an increased loss by radiation from the warm junction. On this account the temperature difference would be less.

I may mention here that, in the narrow pattern of instrument, I have found it advantageous to make a special heat-receiving surface of the thinnest copper, of the size and shape suited to the purpose for which the instrument is made, and to keep the whole of the bars screened from the radiant heat altogether. This an adaptation to the radio-micrometer of the copper-faced thermopile, which Lord ROSSE has found, and which is, obviously, so far preferable to the ordinary construction.

Though it is impossible to find by calculation the exact relative value of the three sources of equalisation of temperature in the circuit—namely, conduction of heat, PELTIER effect, and radiation—it will be some guide to find, as far as data will allow, what their values are. It is most convenient to express them all by giving the time that would elapse before all the heat which is transferred to the cold junction by either of the first two actions, or which escapes in consequence of the third, would be sufficient to raise the pair of bars to the temperature of the warm junction, or, in the case of radiation, to raise it about half as much.

It is easy to show that this time is for the PELTIER effect equal to $\mathrm{JR}s/t\theta$, where

J is JOULE's equivalent ($4 \cdot 2 \times 10^7$),
$\mathrm{R} = 2\mathrm{C}$ is the resistance of the whole circuit ($67 \cdot 42 \times 10^6$),
s is the heat capacity of the two bars (·000798),
t is the mean absolute temperature (taken as 290),
θ the thermo-electric power (taken as 10,000).

The time of equalisation at a supposed constant rate is on this account 77·9 seconds.

This is true of the circuit of greatest efficacy; if the circuit of reduced size is employed, that is, one with a length one-N^{th} of this, then the total resistance R and the time of equalisation will be $(\mathrm{N}+1)/2\mathrm{N}$ times as great. This can never be less than $\frac{1}{2}$.

The corresponding time for the equalisation by conduction may be taken as equal to $sl/\mathrm{D}a$, where

l is the length of the bars,
a their sectional area (separately),
D the sum of the conductivities of antimony and bismuth, *i.e.*, ·0607.

The value of this time is 2·63 seconds.

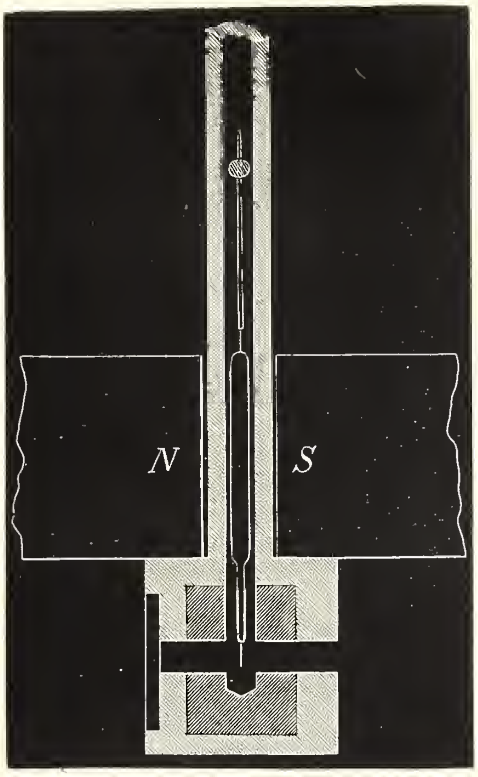
Thus, conduction appears to be far more important than the PELTIER effect, which, practically, may be left out of account. It can only become comparable when so much heat is lost by radiation that the rate of conduction at the cool end is far less than at the hot end, but in this case they would neither be of any practical importance in comparison with the radiation.

The data for finding the time for the escape of half the quantity of heat by radiation, contact of air, &c., are of doubtful value, on account of the very small size of the bars. But taking Professor TAIT's figure, given in LUPTON's Tables, for a black surface, the time would be about 29 seconds. Though no great value must be attached to this figure, it would appear that conduction is the main cause of the equalisation of the temperature of the circuit.

The PELTIER effect is involved in another manner in the action of the instrument. It must make a difference in the value of the least magnetic field which is necessary for the dead beat conditions. Thus, during motion of the circuit, currents are induced which oppose the motion; but these currents set up differences of temperature, which oppose the currents. Therefore, a stronger field may be employed before the dead beat conditions are reached. It is hardly necessary to do more than state that, as the result of calculation, no appreciable change is made in the value of the dead beat magnetic field on this account.

I must here mention the only difficulty which is apt to be found in practice. It arises from the magnetic properties of many materials which, insignificant though they are under ordinary methods of observation, become of serious importance when the extremely feeble force due to the torsion of the fibre is taken into account. I have found it absolutely necessary to sink the antimony and bismuth into a little well made by drilling a hole, no larger than necessary, in a piece of soft iron which is buried in the brasswork. A small lateral hole allows the radiation to fall on the heat-receiving surface. This effectually screens off the part which produces the greatest disturbance (see fig. 7, in which the iron is represented by the darker shading). The

Fig. 7.



copper must be exposed to the magnetism; therefore, it must be carefully examined to see that it is neutral, and it must then be kept away from emery or magnetic cleaning materials.

It is finally necessary to show that advantage is gained by employing the antimony-bismuth-copper combination, instead of the plain pair of wires used by M. D'ARSONVAL. While antimony-bismuth wires would, on account of their great thermo-electric power, be superior to palladium-silver, they would, on account of their magnetic qualities, disturb the natural period of the circuit. By the combination which I have employed, I am able to make use of this great thermo-electric power at the same time that the magnetic disturbance is avoided.

Some of the conclusions enunciated in the preliminary note require modification in view of the more extended investigation described in this paper. In a note, added March 23rd, I had concluded that more than one junction would be advantageous, but this is not the case.

But the point that requires special correction is the estimate formed of the greatest possible sensibility. This was calculated on the assumption that an instrument could be used practically when a particular circuit had a natural vibration period of 20 seconds, and was suspended in a field of 10,000 units, produced by an electro-magnet. Without entering on the question whether an electro-magnet could be used, it is sufficient to say that the resistance to the motion would be so great, the instrument would be so much more than just dead beat, that the time of coming to rest would be enormously prolonged. Thus, though the figure given is correct, the conditions to which it refers would not practically be advantageous.

It is, then, with some satisfaction that I turn to the result given by an instrument of the narrow form, having the best proportions. I have now taken quantities which can not only be separately obtained, but which can be used together, and which I have actually used with success.

Under these conditions, the least difference of temperature that could be observed with certainty, that is, one giving a movement of the light of $\frac{1}{5}$ mm. on the scale, would be due to a temperature difference of less than one two-millionth of a degree Centigrade. This figure is obtained by putting in the values of the quantities in the formula for temperature, which may be expressed in a variety of ways. The following are convenient :—

$$\text{Temperature difference} = \frac{128\pi^2\alpha'\sqrt{\text{KC}uv}}{3\text{T}^2\text{H}\theta},$$

or, if the dead beat magnetic field for such a circuit be employed as well,

$$\text{Temperature difference} = \frac{16}{3}\frac{\alpha'}{\theta}\frac{\pi}{\text{T}}\sqrt{\text{KC}}.$$

α' is the least observable angle of deflection (supposed $\frac{1}{10,000}$),

θ, the thermo-electric power (supposed 10,000),

T, the natural vibration period (supposed 10 seconds),

K, C, u, and v as before.

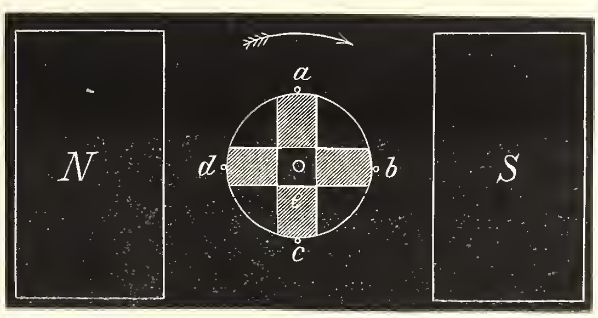
The temperature difference $8{\cdot}06 \times 10^{-7}$, found from the equation above, must be multiplied by $\frac{4}{7}$ to give the corresponding figures for the circuit of reduced length.

Tests made with an instrument of the narrow form, in which it was evident that the magnet still acted to a slight extent on the materials of the suspended portion, show it to be in practice exceedingly sensitive, and, what is of even more importance, the equilibrium of the moving parts remains perfectly stable. The surface which receives the radiant heat is in a particular case a disc only 2 mm. in diameter, and when the scale is 30 inches from the mirror, the hand held about a yard from the instrument produces at once a deflection of 16 cm. A candle flame at 9 feet produced a deviation of 45 mm. every time a small shutter close to the candle was pulled on one side with a piece of cotton, nothing else being allowed to move. Making a strict comparison between this result and those referred to in the preliminary note, this would give 1530 feet as the distance to which a halfpenny might be taken from a candle flame before the heat which it would receive would, if concentrated on the sensitive surface, be too small to produce a deflection of $\frac{1}{4}$ mm. This figure is considerably in excess of that obtained before, even though the fibre is 10 instead of 38 cm. long, the scale is only 30 inches from the screen, and a deflection of $\frac{1}{4}$ instead of $\frac{1}{10}$ mm. is here assumed as the least that could be observed with certainty.

With regard to the rotating pile described at the end of the preliminary note, I ought to say that something very similar is mentioned in NOAD's 'Electricity and Magnetism,' but I was not aware of this at the time of publication. There is, however, a curious difference, which is worth pointing out. So far as I have been able to learn, the wire frames described in NOAD's book only rotate one way when on one pole, and the other way when on the other pole of a magnet. Now, my arrangement will not rotate at all when placed over a pole; it will only rotate when between two poles, and then it will go either way when the heat is applied on one side, but will be prevented from moving when the heat is applied on the other side. Though the matter is of little importance, I may perhaps explain the reason for the peculiar behaviour of my arrangement.

The cross seen in fig. 8 is made with bismuth arms and an antimony centre. At the

Fig. 8.



ends of the arms four copper wires, a, b, c, d, are soldered at right angles to the plane of the cross, and lower down to a ring of copper. The whole is balanced on a point between the poles of a magnet, and is free to turn.

If heat is applied to the point e, an up current in the wire is produced at c, and a down current at a. Hence, the position of the cross is one of unstable equilibrium. Whichever way the cross begins to move, it will be kept moving in this direction. Suppose it to start in the direction of the arrow, that side of the antimony centre which faces the north pole will become the hottest, though it is gaining heat most rapidly when passing e; hence, the up current at d and the down current at b will each be urged on in the same direction. If, however, the cross started in the other direction, the side nearest the south pole would become the hottest, the current would pass the other way, and the reversed motion would still be kept up. Thus, whichever way it moves, it is kept moving in the same direction. If, however, the heat is applied on the opposite side, the first current produced is in a position of stable equilibrium; hence, the circuit shows no disposition to move unless there is want of symmetry, when it moves 45°, and then the equilibrium is absolutely stable. Whichever way the circuit is now made to turn, the direction of the currents will be such as stop the motion, for the same reason that in the previous case they maintained it.

In conclusion, the principal advantages in the instrument the development of which is described in this paper are:—

Extreme quickness and sensibility.

Freedom from extraneous thermal and magnetic influence.

The sensibility can be varied at will.

The instrument may be made dead beat, or its logarithmic decrement may be varied at will.

By the use of the quartz fibre, difficulties caused by the uncertain behaviour of silk under varying conditions of temperature and moisture—difficulties that would be far greater than in the case of a galvanometer—are completely obviated.

On the other hand, a disadvantage inherent in the instrument is that it must, like a galvanometer, be fixed in position; it is inferior to the thermopile or bolometer in the ease with which they can be pointed in any desired direction.

VI. *The Waves on a Rotating Liquid Spheroid of Finite Ellipticity.*

By G. H. Bryan, *B.A.*

Communicated by Professor G. H. Darwin, *F.R.S.*

Received November 6,—Read November 22, 1888.

1. The hydrodynamical problem of finding the waves or oscillations on a gravitating mass of liquid which, when undisturbed, is rotating as if rigid with finite angular velocity, in the form of an ellipsoid or spheroid, was first successfully attacked by M. Poincaré in 1885. In his important memoir, "Sur l'Équilibre d'une Masse Fluide animée d'un Mouvement de Rotation,"* Poincaré has (§ 13) obtained the differential equations for the oscillations of rotating liquid, and shown that, by a transformation of projection, the determination of the oscillations of any particular period is reducible to finding a suitable solution of Laplace's equation. He then applies Lamé's functions to the case of the ellipsoid, showing that the differential equations are satisfied by a series of Lamé's functions referred to a certain auxiliary ellipsoid, the boundary-conditions, however, involving ellipsoidal harmonics, referred to both the auxiliary and actual fluid ellipsoid. At the same time, Poincaré's analysis does not appear to admit of any definite conclusions being formed as to the nature and frequencies of the various periodic free waves.

The present paper contains an application of Poincaré's methods to the simpler case when the fluid ellipsoid is one of revolution (Maclaurin's spheroid). The solution is effected by the use of the ordinary tesseral or zonal harmonics applicable to the fluid spheroid and to the auxiliary spheroid required in solving the differential equation. The problem is thus freed from the difficulties attending the use of Lamé's functions, and is further simplified by the fact that each independent solution contains harmonics of only one particular degree and rank.

By substituting in the conditions to be satisfied at the surface of the spheroid we arrive at a single boundary-equation. If we are treating the forced tides due to a known periodic disturbing force, this equation determines their amplitude and, hence, the elevation of the tide above the mean surface of the spheroid at any point at any time. If there be no disturbing force, it determines the frequencies of the various free waves determined by harmonics of given order and rank. Denoting by κ the ratio of the frequency of the free waves to twice the frequency of rotation of the

* 'Acta Mathematica,' vol. 7.

23.3.89

liquid about its axis, the values of κ are the roots of a rational algebraic equation, and depend only on the eccentricity of the spheroid, as well as the degree and rank of the harmonic, while the number of different free waves depends on the degree of the equation in κ. At any instant the height of the disturbance at any point of the surface is proportional to the corresponding surface harmonic on the spheroid, multiplied by the central perpendicular on the tangent plane, and is of the same form for all waves determined by harmonics of any given degree and rank, whatever be their frequency; but the motions of the fluid particles in the interior will differ in nature in every case.

Taking first the case of zonal harmonics of the n^{th} degree, we find that, according as n is even or odd, there will be $\frac{1}{2}n$ or $\frac{1}{2}(n+1)$ different periodic motions of the liquid. These are essentially oscillatory in character and symmetrical about the axis of the spheroid. In all but one of these the value of κ is essentially less than unity, that is, the period is greater than the time of a semi-revolution of the liquid.

Taking next the tesseral harmonics of degree n and rank s, we find that they determine $n - s + 2$ periodic small motions. These are essentially tidal waves rotating with various angular velocities about the axis of the spheroid, the angular velocities of those rotating in opposite directions being in general different. All but two of the values of κ are numerically less than unity, the periods of the corresponding tides at a point fixed relatively to the liquid being greater than the time of a semi-revolution of the mass. The mean angular velocity of these $n - s + 2$ waves is less than that of rotation of the mass by $2/\{s(n - s + 2)\}$ of the latter.

In the two waves determined by any sectorial harmonic, the relative motion of the liquid particles is irrotational. The harmonics of degree 2 and rank 1 give rise to a kind of precession, of which there are two.

I have calculated the relative frequencies of several of the principal waves on a spheroid whose eccentricity is $\frac{1}{2}\sqrt{2}$.

The question of stability is next dealt with, it being shown that in the present problem, in which the liquid forming the spheroid is supposed perfect, the criteria are entirely different from the conditions of secular stability obtained by POINCARÉ for the case when the liquid possesses any amount of viscosity, and which latter depend on the energy being a minimum. In fact, for a disturbance initially determined by any harmonic (provided that it is symmetrical with respect to the equatorial plane, since for unsymmetrical displacements the spheroid cannot be unstable), the limits of eccentricity consistent with stability are wider for a perfect liquid spheroid than for one possessing any viscosity. If we assume that the disturbed surface initially becomes ellipsoidal, the conditions of stability found by the methods of this paper agree with those of RIEMANN.

The case when the ellipticity and, therefore, the angular velocity are very small is next discussed, it being shown that all but two of the waves, or all but one of the oscillations for any particular harmonic, become unimportant, their periods increasing indefinitely. In the case of those whose periods remain finite for a non-rotating spherical mass, the effect of a small angular velocity ω of the liquid is to cause them to turn round the axis with a velocity less than that of the liquid by ω/n.

Finally, the methods of treating forced tides are further discussed. The general cases of a "semi-diurnal" forced tide, or of permanent deformations due to constant disturbing forces, are mentioned in connection with some peculiarities they present; and these are followed by examples of the determination of the forced tides due to the presence of an attracting mass, first, when the latter moves in any orbit about the spheroid, secondly, when it rotates uniformly about the spheroid in its equatorial plane. The effects of such a body in destroying the equilibrium of the spheroid where the forced tide coincides with one of the free tides form the conclusion of this paper.

POINCARÉ'S *Differential Equations for Waves or Oscillations of Rotating Liquid.*

2. Suppose a mass of gravitating liquid is in relative equilibrium when rotating as if rigid about a fixed axis with angular velocity ω, and that it is required to determine the waves or small oscillations due to a slight disturbance of the mass.

Let the motion be referred to a set of orthogonal moving axes, of which the axis of z is the fixed axis of rotation, while the axes of x, y rotate about it with angular velocity ω. In the steady or undisturbed motion the positions of the fluid particles relative to these axes will remain fixed. In the oscillations, let U, V, W be the small component velocities of the fluid at the point (x, y, z) *relative* to the axes. The actual component velocities referred to axes fixed in space and coinciding with our axes of x, y, z, at the time considered, will be $\mathrm{U} - \omega y$, $\mathrm{V} + \omega x$, W, and the equations of hydrodynamics may be written *

$$\frac{\partial \mathrm{U}}{\partial t} - \omega(\mathrm{V} + \omega x) + \mathrm{U}\frac{\partial \mathrm{U}}{\partial x} + \mathrm{V}\left(\frac{\partial \mathrm{U}}{\partial y} - \omega\right) + \mathrm{W}\frac{\partial \mathrm{U}}{\partial z} = \frac{\partial}{\partial x}\left(\mathrm{V}_1 - \frac{p}{\rho}\right),$$

$$\frac{\partial \mathrm{V}}{\partial t} + \omega(\mathrm{U} - \omega y) + \mathrm{U}\left(\frac{\partial \mathrm{V}}{\partial x} + \omega\right) + \mathrm{V}\frac{\partial \mathrm{V}}{\partial y} + \mathrm{W}\frac{\partial \mathrm{V}}{\partial z} = \frac{\partial}{\partial y}\left(\mathrm{V}_1 - \frac{p}{\rho}\right),$$

$$\frac{\partial \mathrm{W}}{\partial t} + \mathrm{U}\frac{\partial \mathrm{W}}{\partial x} + \mathrm{V}\frac{\partial \mathrm{W}}{\partial y} + \mathrm{W}\frac{\partial \mathrm{W}}{\partial z} = \frac{\partial}{\partial z}\left(\mathrm{V}_1 - \frac{p}{\rho}\right),$$

V_1 being the potential due to the attraction of the liquid and any forces which may act on it, p the pressure, and ρ the density.

For small disturbances we may neglect squares and products of the relative velocities U, V, W (as is usual in wave problems), and, therefore, the above equations reduce to

* BASSET, 'Hydrodynamics,' vol. 1, § 23; or GREENHILL, 'Encyclopædia Britannica,' article "Hydromechanics."

$$\left.\begin{aligned} \frac{\partial \mathrm{U}}{\partial t} - 2\omega \mathrm{V} &= \frac{\partial \psi}{\partial x}, \\ \frac{\partial \mathrm{V}}{\partial t} + 2\omega \mathrm{U} &= \frac{\partial \psi}{\partial y}, \\ \frac{\partial \mathrm{W}}{\partial t} \qquad\quad &= \frac{\partial \psi}{\partial z}, \end{aligned}\right\} \quad \ldots \ldots \ldots \ldots \quad (1),$$

where

$$\psi = \mathrm{V}_1 - \frac{p}{\rho} + \tfrac{1}{2}\omega^2 (x^2 + y^2) \quad \ldots \ldots \ldots \quad (2).$$

We have also the equation of continuity,

$$\frac{\partial \mathrm{U}}{\partial x} + \frac{\partial \mathrm{V}}{\partial y} + \frac{\partial \mathrm{W}}{\partial z} = 0 \quad \ldots \ldots \ldots \ldots \quad (3).$$

Eliminating U, V, W from equations (1), (3), we obtain the differential equation

$$\frac{\partial^2}{\partial t^2} \nabla^2 \psi + 4\omega^2 \frac{\partial^2 \psi}{\partial z^2} = 0 \quad \ldots \ldots \ldots \ldots \quad (4),$$

where, as usual, ∇^2 stands for LAPLACE's operator $\partial^2/\partial x^2 + \partial^2/\partial y^2 + \partial^2/\partial z^2$.

3. Let us now consider separately the simple harmonic oscillations of one particular period. Assume that U, V, W, and ψ all vary as $e^{2\iota\omega\kappa t}$, so that the ratio of the period of oscillation to the time of a complete revolution of the liquid mass about its axis is $1/2\kappa$. The equations (1), (4) reduce to

$$\left.\begin{aligned} 2\omega (\iota\kappa \mathrm{U} - \mathrm{V}) &= \frac{\partial \psi}{\partial x}, \\ 2\omega (\iota\kappa \mathrm{V} + \mathrm{U}) &= \frac{\partial \psi}{\partial y}, \\ 2\omega\iota\kappa \mathrm{W} \qquad &= \frac{\partial \psi}{\partial z}, \end{aligned}\right\} \quad \ldots \ldots \ldots \ldots \quad (5),$$

$$\frac{\partial^2 \psi}{\partial x^2} + \frac{\partial^2 \psi}{\partial y^2} + \left(1 - \frac{1}{\kappa^2}\right) \frac{\partial^2 \psi}{\partial z^2} = 0 \quad \ldots \ldots \ldots \quad (6).$$

Put

$$1 - \frac{1}{\kappa^2} = \tau^2 \quad \ldots \ldots \ldots \ldots \quad (7),$$

and

$$z = \tau z' \quad \ldots \ldots \ldots \ldots \quad (8).$$

Equation (6) now becomes

$$\frac{\partial^2 \psi}{\partial x^2} + \frac{\partial^2 \psi}{\partial y^2} + \frac{\partial^2 \psi}{\partial z'^2} = 0 \quad \ldots \ldots \ldots \ldots \quad (9).$$

If κ be greater than unity, τ and, therefore, also z' will be real. We may take (x, y, z') to be the coordinates of a point corresponding to the point (x, y, z) of the liquid. We thus obtain a new region of points derivable from the original region by homogeneous strain parallel to z or by projection. This region may be called the auxiliary region, and the surface formed by points corresponding to points on the fluid surface, the auxiliary surface. Our problem thus reduces to that of finding a suitable value of ψ satisfying LAPLACE'S equation (9) within the space bounded by the auxiliary surface.

But we must revert to the original system in order to satisfy the boundary-conditions, which must hold at the actual surface of the liquid, not at the auxiliary surface. If the surface of the liquid be free, p must be constant over it, and, therefore, the condition to be satisfied all over the *disturbed* surface of the liquid is

$$\psi = \mathrm{V}_1 + \tfrac{1}{2}\omega^2(x^2 + y^2) + \text{const.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10).$$

In forming the expression for V_1 we must remember that the gravitation potential is due to the disturbed configuration of the liquid mass.

If κ be less than unity, τ will be imaginary, and, therefore, the auxiliary surface will also be imaginary. But the results arrived at by this method in the case where τ is real will still hold good even if τ be imaginary, provided that the expression obtained for ψ is a real function of the coordinates x, y, z. The method breaks down if $\kappa = \pm 1$, when τ vanishes; this must be treated as a limiting case.

Solution for the Spheroid by Spheroidal Harmonics.

4. Let the liquid be in the form of a MACLAURIN'S spheroid the equation of whose surface is

$$\frac{x^2 + y^2}{c^2(\zeta_0^2 + 1)} + \frac{z^2}{c^2\zeta_0^2} \equiv \frac{x^2 + y^2}{c^2 \operatorname{cosec}^2 \alpha} + \frac{z^2}{c^2 \cot^2 \alpha} = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad (11),$$

so that

$$\zeta_0 = \cot \alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

and $\sin \alpha$ is the eccentricity of the spheroid, c being the radius of its focal circle.

The locus of the corresponding point (x, y, z') is the auxiliary quadric

$$\frac{x^2 + y^2}{c^2 \operatorname{cosec}^2 \alpha} + \frac{\tau^2 z'^2}{c^2 \cot^2 \alpha} = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13).$$

This quadric will be a prolate spheroid if τ^2 lies between zero and $\cos^2\alpha$, that is, if κ^2 lies between unity and $\operatorname{cosec}^2\alpha$. If τ^2 is greater than $\cos^2\alpha$, or κ^2 greater than $\operatorname{cosec}^2\alpha$, the spheroid will be oblate. If τ^2 be negative, or κ^2 less than unity, equation (13) represents a hyperboloid of one sheet, but the part corresponding to the liquid surface is the imaginary portion for which $x^2 + y^2$ is less than $c^2 \operatorname{cosec}^2 \alpha$, and z'^2 is negative; this is the imaginary auxiliary spheroid.

We shall take as our standard case that in which equation (13) represents a prolate auxiliary spheroid. Let it be written in the form

$$\frac{x^2 + y^2}{k^2 (\nu_0^2 - 1)} + \frac{z'^2}{k^2 \nu_0^2} = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14),$$

so that

$$k^2 (\nu_0^2 - 1) = c^2 \operatorname{cosec}^2 \alpha,$$

$$k^2 \nu_0^2 = \frac{c^2 \cot^2 \alpha}{\tau^2} = \frac{c^2 \cot^2 \alpha \, . \, \kappa^2}{\kappa^2 - 1}.$$

Solving for ν_0, k, we find

$$\nu_0 = \frac{\kappa \cos \alpha}{\sqrt{(1 - \kappa^2 \sin^2 \alpha)}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15),$$

$$k^2 = c^2 \frac{\operatorname{cosec}^2 \alpha - \kappa^2}{\kappa^2 - 1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16).$$

The solution of the differential equation (9) must be effected by means of spheroidal harmonics applicable to the auxiliary spheroid (14), whilst the expressions for the gravitation potential of the liquid mass and the boundary-conditions will involve spheroidal harmonics referred to the actual liquid spheroid (11). We must, therefore, use two different sets of orthogonal elliptic coordinates for the auxiliary and the actual systems. Let these coordinates be denoted by (μ', ν, ϕ) (μ, ζ, ϕ) respectively, and let them be connected with the rectangular coordinates in the two systems by the relations

$$\left.\begin{array}{ll} x = k\sqrt{(\nu^2 - 1)}\sqrt{(1 - \mu'^2)} \cos \phi & = c\sqrt{(\zeta^2 + 1)}\sqrt{(1 - \mu^2)} \cos \phi \\ y = k\sqrt{(\nu^2 - 1)}\sqrt{(1 - \mu'^2)} \sin \phi & = c\sqrt{(\zeta^2 + 1)}\sqrt{(1 - \mu^2)} \sin \phi \\ z' = k\nu\mu' & = c\zeta\mu/\tau \\ \text{and, therefore,} & \\ z = k\nu\mu'\tau & = c\zeta\mu \end{array}\right\} \quad . \quad (17).$$

The surfaces of the spheroids will be given by the equations

$$\zeta = \zeta_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18),$$

or

$$\nu = \nu_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18\text{A});$$

moreover, all over these surfaces, at corresponding points,

$$\mu = \mu' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19).$$

The angular coordinate ϕ is the same in both systems; but, except over the surfaces, μ will not be equal to μ', nor will any other two of the surfaces $\nu =$ constant and $\zeta =$ constant coincide.

Put $\mu' = \cos\theta$. On transforming to (θ, ν, ϕ), equation (9) becomes

$$\frac{\partial}{\partial \nu}\left\{(\nu^2 - 1)\frac{\partial \psi}{\partial \nu}\right\} + \frac{1}{\sin\theta}\frac{\partial}{\partial\theta}\left(\sin\theta\frac{\partial\psi}{\partial\theta}\right) + \frac{\nu^2 - \cos^2\theta}{(\nu^2-1)\sin^2\theta}\frac{\partial^2\psi}{\partial\phi^2} = 0 \quad . \quad . \quad (20),$$

of which a solution, finite and continuous at all points within the spheroid (14), is

$$\psi = A_n^s T_n^{(s)}(\mu') T_n^s(\nu) e^{\iota s\phi} e^{2\iota\omega\kappa t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21),$$

where A_n^s is any constant, and

$$T_n^{(s)}(\mu') = (1 - \mu'^2)^{s/2}\left(\frac{d}{d\mu'}\right)^s P_n(\mu') \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22),$$

$$T_n^s(\nu) = (\nu^2 - 1)^{s/2}\left(\frac{d}{d\nu}\right)^s P_n(\nu) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23),$$

P_n denoting the zonal harmonic of degree n.

In our standard case ν is real and greater than unity, and in every case μ' lies between the limits $+1$ and -1, and is real. I have adopted the above notation (according to which the functions $T_n^{(s)}$, T_n^s differ in form by the constant factor $(-1)^{s/2}$) in order to avoid introducing imaginary coefficients unnecessarily.*

It is easy to see that the solution (21) is applicable in every case. For, if τ^2 be greater than $\cos^2\alpha$, both k and ν are purely imaginary; whilst, if τ^2 be negative, we may show that k will be imaginary, but ν will be real and less than unity. In any case $T_n^s(\nu)$ will be either real or purely imaginary, so that $A_n^s T_n^{(s)}(\mu) T_n^s(\nu)$ can be always made a *real* function of the coordinates (x, y, z). Moreover, the right-hand side of (21) is finite, single valued, and continuous throughout the liquid spheroid, and satisfies the differential equation (6). It therefore only remains to investigate the boundary-conditions which must be satisfied by ψ at the surface of the liquid.

5. The spheroidal harmonics referred to the liquid spheroid, required for these boundary-conditions, will be formed as follows:—

Let

$$p_n(\zeta) = \frac{1}{2^n n!}\left(\frac{d}{d\zeta}\right)^n (\zeta^2 + 1)^n \equiv (-1)^{n/2} P_n(\iota\zeta) \quad . \quad . \quad . \quad (24),$$

$$t_n^s(\zeta) = (\zeta^2 + 1)^{s/2}\left(\frac{d}{d\zeta}\right)^s p_n(\zeta) \equiv (-1)^{n/2} T_n^s(\iota\zeta) \quad . \quad . \quad . \quad (25),$$

* The tesseral harmonics may be replaced by the associated functions of the first kind of HEINE without any change in the formulæ, the constant coefficients being supposed included in A_n^s.

and let

$$q_n(\zeta) = p_n(\zeta)\int_\zeta^\infty \frac{d\zeta}{(\zeta^2+1)\{p_n(\zeta)\}^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26),$$

$$u_n^s(\zeta) = t_n^s(\zeta)\int_\zeta^\infty \frac{d\zeta}{(\zeta^2+1)\{t_n^s(\zeta)\}^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27).$$

Then the expressions

$$v_1 = \mathrm{B}_n{}^s e^{2\iota\omega\kappa t} e^{\iota s\phi} \mathrm{T}_n{}^{(s)}(\mu)\, t_n{}^s(\zeta)/t_n{}^s(\zeta_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad (28),$$

$$v_0 = \mathrm{B}_n{}^s e^{2\iota\omega\kappa t} e^{\iota s\phi} \mathrm{T}_n{}^{(s)}(\mu)\, u_n{}^s(\zeta)/u_n{}^s(\zeta_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad (29),$$

are solutions of LAPLACE'S equation which are finite and continuous, the former throughout the interior of the spheroid $(\zeta = \zeta_0)$, the latter throughout all space outside the spheroid, and vanishing at infinity; while at the surface $(\zeta = \zeta_0)$ both expressions become equal to

$$[v] = \mathrm{B}_n{}^s e^{2\iota\omega\kappa t} e^{\iota s\phi} \mathrm{T}_n{}^{(s)}(\mu) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30).$$

The Elevation of the Waves on the Surface.

6. Let h be the normal displacement at any point of the liquid surface, *i.e.*, the height of the wave above the level of the undisturbed spheroid.

Let ϖ be the central perpendicular on the tangent plane, and $d\mathrm{N}$ an element of the outward drawn normal to the surface of the *liquid* spheroid (11). We readily find

$$\frac{\partial x}{\partial \zeta} = \frac{\zeta x}{\zeta^2+1}, \qquad \frac{\partial y}{\partial \zeta} = \frac{\zeta y}{\zeta^2+1}, \qquad \frac{\partial z}{\partial \zeta} = \frac{\zeta z}{\zeta^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31),$$

and at the surface, since the element $d\mathrm{N}$ is a tangent to the curve $\mu = \text{const.}$, $\phi = \text{const.}$; therefore,

$$\left(\frac{d\mathrm{N}}{d\zeta}\right)^2 = \left(\frac{\partial x}{\partial \zeta}\right)^2 + \left(\frac{\partial y}{\partial \zeta}\right)^2 + \left(\frac{\partial z}{\partial \zeta}\right)^2;$$

whence,

$$\frac{d\mathrm{N}}{d\zeta} = c\sqrt{\left(\frac{\zeta_0^2+\mu^2}{\zeta_0^2+1}\right)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32),$$

the differentials in (32) being *total*

Also

$$\varpi = c\zeta_0\sqrt{\left(\frac{\zeta_0^2+1}{\zeta_0^2+\mu^2}\right)},$$

so that

$$\varpi\, d\mathrm{N} = c^2\zeta_0\, d\zeta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33).$$

From equations (5) we obtain

$$\left.\begin{aligned} 2\omega(1-\kappa^2)\,\mathrm{U} &= \iota\kappa\frac{\partial\psi}{\partial x}+\frac{\partial\psi}{\partial y} \\ 2\omega(1-\kappa^2)\,\mathrm{V} &= \iota\kappa\frac{\partial\psi}{\partial y}-\frac{\partial\psi}{\partial x} \\ 2\omega(1-\kappa^2)\,\mathrm{W} &= \iota\kappa\tau^2\frac{\partial\psi}{\partial z} \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (34).$$

Multiplying by $\partial x/\partial\mathrm{N}$, $\partial y/\partial\mathrm{N}$, $\partial z/\partial\mathrm{N}$, and adding, we find at the surface

$$2\omega(1-\kappa^2)\left(\mathrm{U}\frac{\partial x}{\partial\mathrm{N}}+\mathrm{V}\frac{\partial y}{\partial\mathrm{N}}+\mathrm{W}\frac{\partial z}{\partial\mathrm{N}}\right)$$
$$=\iota\kappa\left(\frac{\partial\psi}{\partial x}\frac{\partial x}{\partial\mathrm{N}}+\frac{\partial\psi}{\partial y}\frac{\partial y}{\partial\mathrm{N}}+\tau^2\frac{\partial\psi}{\partial z}\frac{\partial x}{\partial\mathrm{N}}\right)+\left(\frac{\partial\psi}{\partial y}\frac{\partial x}{\partial\mathrm{N}}-\frac{\partial\psi}{\partial x}\frac{\partial y}{\partial\mathrm{N}}\right).$$

But $\mathrm{U}\partial x/\partial\mathrm{N}+\mathrm{V}\partial y/\partial\mathrm{N}+\mathrm{W}\partial z/\partial\mathrm{N}$ is the normal velocity of the liquid relative to the moving axes, and is therefore equal to $\partial h/\partial t$ or to $2\iota\omega\kappa h$. We have, therefore,

$$4\omega^2(1-\kappa^2)\kappa h$$
$$=\kappa\frac{d\zeta}{d\mathrm{N}}\left(\frac{\partial\psi}{\partial x}\frac{\partial x}{\partial\zeta}+\frac{\partial\psi}{\partial y}\frac{\partial y}{\partial\zeta}+\tau^2\frac{\partial\psi}{\partial z}\frac{\partial z}{\partial\zeta}\right)-\iota\frac{\partial\zeta}{\partial\mathrm{N}}\left(\frac{\partial\psi}{\partial y}\frac{\partial x}{\partial\zeta}-\frac{\partial\psi}{\partial x}\frac{\partial y}{\partial\zeta}\right)$$
$$=\kappa c^2\zeta_0\frac{d\zeta}{d\mathrm{N}}\left(\frac{x}{c^2(\zeta_0^2+1)}\frac{\partial\psi}{\partial x}+\frac{y}{c^2(\zeta_0^2+1)}\frac{\partial\psi}{\partial y}+\frac{\tau^2 z}{c^2\zeta_0^2}\frac{\partial\psi}{\partial z}\right)-\frac{\iota\zeta_0}{\zeta_0^2+1}\frac{d\zeta}{d\mathrm{N}}\left(x\frac{\partial\psi}{\partial y}-y\frac{\partial\psi}{\partial x}\right)$$
$$=\kappa c^2\zeta_0\frac{d\zeta}{d\mathrm{N}}\left(\frac{x}{k^2(\nu_0^2-1)}\frac{\partial\psi}{\partial x}+\frac{y}{k^2(\nu_0^2-1)}\frac{\partial\psi}{\partial y}+\frac{z'}{k^2\nu_0^2}\frac{\partial\psi}{\partial z'}\right)-\frac{\iota\zeta_0}{\zeta_0^2+1}\frac{d\zeta}{d\mathrm{N}}\frac{\partial\psi}{\partial\phi}$$
$$=\zeta_0\frac{d\zeta}{d\mathrm{N}}\left\{\frac{\kappa c^2}{\nu_0 k^2}\frac{\partial\psi}{\partial\nu}-\frac{\iota}{\zeta_0^2+1}\frac{\partial\psi}{\partial\phi}\right\} \quad \ldots\ldots\ldots \quad (35).$$

Now, taking ψ as given by (21), we have

$$\frac{d\psi}{d\phi}=\iota s\psi \quad \ldots\ldots\ldots \quad (36);$$

moreover, since by (23)

$$\mathrm{T}_n^s(\nu)=(\nu^2-1)^{s/2}\,\mathrm{D}^s\mathrm{P}_n(\nu),$$

where the symbol D stands for differentiation with respect to ν, therefore

$$\mathrm{DT}_n^s(\nu)=(\nu^2-1)^{s/2}\{\mathrm{D}^{s+1}\mathrm{P}_n(\nu)+s\nu/(\nu^2-1)\,.\,\mathrm{D}^s\mathrm{P}_n(\nu)\} \quad \ldots \quad (37).$$

Also at the surface μ' is equal to μ.

Hence, we find

$$4\omega^2(1-\kappa^2)\kappa h = A_n^s \zeta_0 \frac{d\zeta}{dN}\left\{\frac{\kappa c^2}{\nu_0^2 k^2}\nu_0 D^{s+1}P_n(\nu_0) + \frac{s\kappa c^2}{k^2(\nu_0^2-1)} D^s P_n(\nu_0) + \frac{s}{\zeta_0^2+1} D^s P_n(\nu_0)\right\}$$
$$\times (\nu_0^2-1)^{s/2} T_n^{(s)}(\mu) e^{\iota s\phi} e^{2\iota\omega\kappa t}$$
$$= A_n^s \zeta_0 \frac{d\zeta}{dN}\left\{\frac{\kappa^2-1}{\kappa\zeta_0^2}\nu_0 D^{s+1}P_n(\nu_0) + \frac{s(\kappa+1)}{\zeta_0^2+1} D^s P_n(\nu_0)\right\}$$
$$\times (\nu_0^2-1)^{s/2} T_n^{(s)}(\mu) e^{\iota s\phi} e^{2\iota\omega\kappa t}$$

by (15), (16).

Whence

$$h = C_n^s \varpi T_n^{(s)}(\mu) e^{\iota s\phi} e^{2\iota\omega\kappa t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38),$$

where

$$C_n^s = -A_n^s \frac{\tan^2\alpha}{4\omega^2\kappa c^2}\left\{\frac{1}{\kappa}\nu_0 D^{s+1}P_n(\nu_0) + \frac{s\cos^2\alpha}{\kappa-1} D^s P_n(\nu_0)\right\}(\nu_0^2-1)^{s/2} \quad . \quad (39);$$

moreover, the equation of the disturbed surface of the liquid is

$$\zeta = \zeta_0 + \delta\zeta_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (40),$$

where

$$\delta\zeta_0 = h\frac{d\zeta}{dN} = C_n^s \varpi^2 \tan\alpha / c^2 \, T_n^{(s)}(\mu) e^{\iota s\phi} e^{2\iota\omega\kappa t}. \quad . \quad . \quad . \quad . \quad . \quad (41).$$

The Boundary-Conditions.

7. Let V_0 be the potential of a mass of the liquid filling the spheroid

$$\zeta = \zeta_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18),$$

and let v be the potential of a distribution of the liquid of thickness everywhere equal to h over the surface of the spheroid. The combination of these two distributions is equivalent to the liquid mass as disturbed by the waves, so that

$$V_1 = V_0 + v \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42).$$

For the free waves, the boundary-equation (10) requires that

$$\psi = V_0 + v + \tfrac{1}{2}\omega^2(x^2+y^2) + \text{const.} \quad . \quad . \quad . \quad . \quad . \quad . \quad (43),$$

all over the surface (40).

Now ψ, v, $\delta\zeta_0$ are small quantities of the first order. Hence, expanding by TAYLOR's theorem, we have to first order

$$[\psi] = [V_0 + \tfrac{1}{2}\omega^2 (x^2 + y^2)] + [v] + \delta\zeta_0 \frac{d}{d\zeta} \{V_0 + \tfrac{1}{2}\omega^2 (x^2 + y^2)\} + \text{const.} \quad (44),$$

where the square brackets indicate that ζ is to be put equal to ζ_0.

In the case of forced tides due partly to small disturbing forces whose potential at any instant is V_2, and partly to periodic variations of pressure p_2 over the surface of the liquid, the condition at the surface becomes

$$[\psi] = [V_0 + \tfrac{1}{2}\omega^2 (x^2 + y^2)] + [v] + \delta\zeta_0 \frac{d}{d\zeta} \{V_0 + \tfrac{1}{2}\omega^2 (x^2 + y^2)\} + \left[V_2 - \frac{p_2}{\rho}\right] + \text{const.} \quad . \quad (44^*)$$

Equating to zero the non-periodic terms, we obtain the well-known condition for steady motion

$$[V_0 + \tfrac{1}{2}\omega^2 (x^2 + y^2)] + \text{const.} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (45).$$

Here

$$V_0 = \text{const.} - \tfrac{1}{2} \{A (x^2 + y^2) + Cz^2\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (46),$$

where

$$\left.\begin{aligned} A &= 4\pi\rho\gamma\, \zeta_0 (\zeta_0^2 + 1) \int_{\zeta_0}^{\infty} \frac{d\zeta}{(\zeta^2 + 1)^2} = 4\pi\rho\gamma \operatorname{cosec}^2 \alpha \cot \alpha \frac{u_1^1(\zeta_0)}{t_1^1(\zeta_0)} \\ C &= 4\pi\rho\gamma\, \zeta_0 (\zeta_0^2 + 1) \int_{\zeta_0}^{\infty} \frac{d\zeta}{(\zeta^2 + 1)\zeta^2} = 4\pi\rho\gamma \operatorname{cosec}^2 \alpha \cot \alpha \frac{q_1(\zeta_0)}{p_1(\zeta_0)} \end{aligned}\right\} \quad . \quad (47),$$

γ being the constant of gravitation, and being put equal to unity if the density is expressed in astronomical units.

From (45) we have, in the usual manner,

$$(A - \omega^2)(x^2 + y^2) + Cz^2 = C\zeta_0^2 \{(x^2 + y^2)/(\zeta_0^2 + 1) + z^2/\zeta_0^2\} \quad . \quad . \quad (48),$$

whence

$$\begin{aligned} \omega^2 &= A - C\zeta_0^2/(\zeta_0^2 + 1) \\ &= 4\pi\rho\gamma\, \zeta_0 \{t_1^1(\zeta_0)\, u_1^1(\zeta_0) - p_1(\zeta_0)\, q_1(\zeta_0)\} \quad . \quad . \quad . \quad . \quad . \quad (49), \end{aligned}$$

which can also be put in the form

$$\begin{aligned} \omega^2 &= 4\pi\rho\gamma\, \zeta_0 \{\tfrac{1}{2}(3\zeta_0^2 + 1) \cot^{-1} \zeta_0 - \tfrac{3}{2}\zeta_0\} \\ &= 4\pi\rho\gamma\, \zeta_0 q_2(\zeta_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (50). \end{aligned}$$

From (44*), (45) the boundary-condition for the oscillations is

$$[\psi] = [v] + \delta\zeta_0 \frac{\partial}{\partial\zeta} \{V_0 + \tfrac{1}{2}\omega^2 (x^2 + y^2)\} + \left[V_2 - \frac{p_2}{\rho}\right] \quad . \quad . \quad . \quad (51).$$

Now, by (48)

$$\delta\zeta_0 \frac{d}{d\zeta}\{V_0 + \tfrac{1}{2}\omega^2(x^2 + y^2)\}$$

$$= -\tfrac{1}{2}C\zeta_0^2\,\delta\zeta_0\left\{\frac{\partial x}{\partial\zeta}\frac{\partial}{\partial x} + \frac{\partial y}{\partial\zeta}\frac{\partial}{\partial y} + \frac{\partial z}{\partial\zeta}\frac{\partial}{\partial z}\right\}\left(\frac{x^2+y^2}{\zeta_0^2+1} + \frac{z^2}{\zeta_0^2}\right)$$

$$= -4\pi\rho\gamma(\zeta_0^2+1)\,\zeta_0^4\frac{q_1(\zeta_0)}{p_1(\zeta_0)}\left\{\frac{x^2+y^2}{(\zeta_0^2+1)^2} + \frac{z^2}{\zeta_0^4}\right\}\delta\zeta_0$$

$$= -4\pi\rho\gamma(\zeta_0^2+1)\,\zeta_0^2 p_1(\zeta_0)\,q_1(\zeta_0)\,c^4\,\delta\zeta_0/\varpi^2$$

$$= -4\pi\rho\gamma\,C_n^s c^2\cot\alpha\,\mathrm{cosec}^2\alpha\,.\,p_1(\zeta_0)\,q_1(\zeta_0)\,T_n^{(s)}(\mu)\,e^{\iota s\phi}e^{2\iota\omega\kappa t} \quad . \quad . \quad (52).$$

8. To find v.—Suppose that the values of this potential inside and outside the spheroid respectively are given by the formulæ

$$v_1 = B_n^s e^{2\iota\omega\kappa t}e^{\iota s\phi}T_n^{(s)}(\mu)\,t_n^s(\zeta)/t_n^s(\zeta_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28),$$

$$v_0 = B_n^s e^{2\iota\omega\kappa t}e^{\iota s\phi}T_n^{(s)}(\mu)\,u_n^s(\zeta)/u_n^s(\zeta_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29).$$

Since v_1 v_0 are due to a surface distribution of surface density ρh, therefore,

$$-4\pi\rho\gamma h = \left[\frac{\partial V_0}{\partial N}\right] - \left[\frac{\partial V_1}{\partial N}\right] = \frac{d\zeta}{dN}\left[\frac{\partial V_0}{\partial\zeta} - \frac{\partial V_1}{\partial\zeta}\right]_{(\zeta=\zeta_0)}$$

$$= B_n^s\frac{d\zeta}{dN}e^{2\iota\omega\kappa t}e^{\iota s\phi}T_n^{(s)}(\mu)\left\{\frac{Du_n^s(\zeta_0)}{u_n^s(\zeta_0)} - \frac{Dt_n^s(\zeta_0)}{t_n^s(\zeta_0)}\right\}$$

$$= -B_n^s\frac{d\zeta}{dN}e^{2\iota\omega\kappa t}e^{\iota s\phi}T_n^{(s)}(\mu) \div \{(\zeta_0^2+1)\,t_n^s(\zeta_0)\,u_n^s(\zeta_0)\},$$

and, therefore, at the surface, by (30)

$$[v] \equiv B_n^s e^{2\iota\omega\kappa t}e^{\iota s\phi}T_n^{(s)}(\mu)$$

$$= 4\pi\rho\gamma\,C_n^s c^2\cot\alpha\,\mathrm{cosec}^2\alpha\,.\,t_n^s(\zeta_0)\,u_n^s(\zeta_0)\,T_n^{(s)}(\mu)\,e^{\iota s\phi}e^{2\iota\omega\kappa t} \quad . \quad . \quad . \quad . \quad (53).$$

9. Lastly, in the forced oscillations, whatever be the variable conservative bodily forces or surface tractions producing them, we know that it is always possible to expand the value of $[V_2 - p_2/\rho]$ over the surface of the spheroid and at all times in a series of the form

$$[V_2 - p_2/\rho] = \Sigma_{\kappa=-\infty}^{\kappa=\infty}\Sigma_{n=1}^{n=\infty}\Sigma_{s=0}^{s=n}W_{(n,\kappa)}^s T_n^{(s)}(\mu)\,e^{\iota s\phi}e^{2\iota\omega\kappa t} \quad . \quad . \quad . \quad . \quad (54),$$

where $W_{(n,\kappa)}^s$ is a constant, and the summations may extend to all possible values of κ, but only to integral values of n and s. The effect of each term may be considered separately. To do this, let us take the case when there is a single term only, *i.e.*, take

$$[V_2 - p_2/\rho] = W_{(n,\kappa)}^s T_n^{(s)}(\mu)\,e^{\iota s\phi}e^{2\iota\omega\kappa t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (55).$$

In the waves produced the values of n, s, κ will be the same.

Substituting from (21), (52), (53), (55) in (51), we obtain

$$A_n^s(\nu_0^2-1)^{s/2} D^s P_n(\nu_0) + 4\pi\rho\gamma C_n^s c^2 \cot\alpha \operatorname{cosec}^2\alpha \{p_1(\zeta_0) q_1(\zeta_0) - t_n^s(\zeta_0) u_n^s(\zeta_0)\} = W^s_{(n,\kappa)} \quad . \quad . \quad (56).$$

This equation, combined with (39), suffices to determine the unknown constants A_n^s, C_n^s in terms of the known coefficient $W^s_{(n,\kappa)}$, and thus the amplitude of the forced oscillation is determined in terms of that of the disturbing force.

10. The most interesting point is to determine C_n^s, in order to find the height of the corrugations on the surface. This plan has, moreover, the advantage that, in considering the effect of several disturbing forces of different periods, we may add together the elevations (h) due to the separate forces, whereas, in determining the value of ψ, the terms having different periods are referred to different auxiliary systems. Substituting for A_n^s in terms of C_n^s from (39) in (56), and writing, for brevity,

$$K_n^s(\zeta_0) = p_1(\zeta_0) q_1(\zeta_0) - t_n^s(\zeta_0) u_n^s(\zeta_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (57),$$

$$M = \tfrac{4}{3}\pi\rho c^3 \cot\alpha \operatorname{cosec}^2\alpha = \text{mass of spheroid} \quad . \quad . \quad . \quad (58),$$

we find, after several reductions, the required equation for C_n^s, viz.,

$$\frac{3M\gamma C_n^s}{c}\left\{K_n^s(\zeta_0) - \frac{4\kappa q_2(\zeta_0) D^s P_n(\nu_0)}{s\, D^s P_n(\nu_0)/(\kappa-1) + \sec^2\alpha \,.\, \nu_0 D^{s+1} P_n(\nu_0)/\kappa}\right\} = W^s_{(n,\kappa)} \quad . \quad (59),$$

in which it must be remembered that

$$\nu_0 = \frac{\kappa\cos\alpha}{\sqrt{(1-\kappa^2\sin^2\alpha)}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15).$$

The Period-Equations for Free Waves.

11. If the oscillations of the liquid be free, we must put $W^s_{(n,\kappa)}$ equal to zero in (59), and we therefore obtain

$$K_n^s(\cot\alpha) - \frac{4\kappa q_2(\cot\alpha) D^s P_n(\nu_0)}{s\, D^s P_n(\nu_0)/(\kappa-1) + \sec^2\alpha \,.\, \nu_0 D^{s+1} P_n(\nu_0)/\kappa} = 0 \quad . \quad . \quad (60),$$

which, together with (15), determines the admissible values of κ and ν_0. In reducing (60) to a rational algebraic equation for κ we must distinguish three cases.

I. Let $s = 0$, and let n be even. Then we know that $P_n(\nu_0)$ and $DP_n(\nu_0)$ contain only even and odd powers of ν_0 respectively, and, therefore, that $DP_n(\nu_0)$ is divisible by ν_0. Multiplying (60) by $DP_n(\nu_0)/\nu_0$, we find (writing K_n for K_n^0)

$$K_n(\cot\alpha) DP_n(\nu_0)/\nu_0 - 4q_2(\cot\alpha)(1-\kappa^2\sin^2\alpha) P_n(\nu_0) = 0 \quad . \quad . \quad (61).$$

Expanding $P_n(\nu_0)$ and $DP_n(\nu_0)/\nu_0$ in powers of ν_0, substituting for ν_0 by means of (15), and multiplying the resulting equation throughout by $(1 - \kappa^2 \sin^2 \alpha)^{n/2-1}$, we find

$$4q_2(\cot\alpha)\left\{(\kappa\cos\alpha)^n - \frac{n(n-1)}{2.(2n-1)}(\kappa\cos\alpha)^{n-2}(1-\kappa^2\sin^2\alpha) + \ldots\right\}$$
$$- nK_n(\cot\alpha)\left\{(\kappa\cos\alpha)^{n-2} - \frac{(n-1)(n-2)}{2.(2n-1)}(\kappa\cos\alpha)^{n-4}(1-\kappa^2\sin^2\alpha) + \ldots\right\}$$
$$= 0 \quad . \quad . \quad (62).$$

This is a rational algebraic equation in κ of the n^{th} degree, involving only even powers of κ. It is, therefore, satisfied by n values of κ occurring in pairs corresponding to $\frac{1}{2}n$ values of κ^2.

II. Let $s = 0$, but let n be odd. Then $DP_n(\nu_0)$ is not divisible by ν_0. Hence, we must multiply the equation (60) throughout by $DP_n(\nu_0)$ and obtain

$$K_n(\cot\alpha)\, DP_n(\nu_0) - 4q_2(\cot\alpha)(1-\kappa^2\sin^2\alpha)\,\nu_0 P_n(\nu_0) = 0 \quad . \quad . \quad (63).$$

If this be developed in the same manner as in the preceding case, we shall obtain

$$4q_2(\cot\alpha)\left\{(\kappa\cos\alpha)^{n+1} - \frac{n(n-1)}{2.(2n-1)}(\kappa\cos\alpha)^{n-1}(1-\kappa^2\sin^2\alpha) + \ldots\right\}$$
$$- nK_n(\cot\alpha)\left\{(\kappa\cos\alpha)^{n-1} - \frac{(n-1)(n-2)}{2.(2n-1)}(\kappa\cos\alpha)^{n-3}(1-\kappa^2\sin^2\alpha) + \ldots\right\}$$
$$= 0 \quad . \quad . \quad (64).$$

This is satisfied by $n + 1$ values of κ, but, as before, the positive and negative roots are numerically equal, so that there will only be $\frac{1}{2}(n + 1)$ different values of κ^2.

III. Let s be different from zero. Multiplying by the expression

$$sD^sP_n(\nu_0) + \sec^2\alpha\,.\,(\kappa - 1)/\kappa\,.\,\nu_0 D^{s+1}P_n(\nu_0),$$

we find

$$\sec^2\alpha\, K_n^s(\cot\alpha)(\kappa-1)/\kappa\,.\,\nu_0 D^{s+1}P_n(\nu_0)$$
$$-\{4q_2(\cot\alpha)\kappa(\kappa-1) - sK_n^s(\cot\alpha)\}\,D^sP_n(\nu_0) = 0 \quad . \quad . \quad (65),$$

which reduces to the following equation in κ—

$$\{4q_2(\cot\alpha)\kappa(\kappa-1) - sK_n^s(\cot\alpha)\}\left\{(\kappa\cos\alpha)^{n-s}\right.$$
$$\left. - \frac{(n-s)(n-s-1)}{2.(2n-1)}(\kappa\cos\alpha)^{n-s-2}(1-\kappa^2\sin^2\alpha) + \ldots\right\}$$
$$-(n-s)\sec\alpha\,.\,K_n^s(\cot\alpha)(\kappa-1)\left\{(\kappa\cos\alpha)^{n-s-1}\right.$$
$$\left. - \frac{(n-s-1)(n-s-2)}{2.(2n-1)}(\kappa\cos\alpha)^{n-s-3}(1-\kappa^2\sin^2\alpha) + \ldots\right\} = 0 \quad . \quad (66).$$

This equation is of the degree $n-s+2$, and involves both odd and even powers of κ. It therefore has $n-s+2$ roots, but in the present case these roots do not occur in pairs of equal and opposite values.

Equations (62), (64), (66) are the period-equations of the various free harmonic waves or oscillations of the liquid spheroid. Their roots depend on the value of α or the eccentricity ($\sin \alpha$) alone. The periods of the waves are the corresponding values of $\pi/\omega\kappa$ and depend also on ω.

Nature of the Real Oscillations and Waves.

12. The periodic movements determined by zonal harmonics ($s = 0$) and those determined by tesseral harmonics differ in character considerably.

The former are symmetrical about the axis. Taking the solution

$$\begin{aligned} \psi &= \mathrm{A}_n \mathrm{P}_n(\mu') \, \mathrm{P}_n(\nu) \, e^{2\iota\omega\kappa t}, \\ h &= \mathrm{C}_n \varpi \, \mathrm{P}_n(\mu) \, e^{2\iota\omega\kappa t}, \end{aligned}$$

another solution got by changing the sign of κ is given by

$$\begin{aligned} \psi &= \mathrm{A}_n \mathrm{P}_n(\mu') \, \mathrm{P}_n(\nu) \, e^{-2\iota\omega\kappa t}, \\ h &= \mathrm{C}_n \varpi \, \mathrm{P}_n(\mu) \, e^{-2\iota\omega\kappa t}. \end{aligned}$$

Compounding these, we get the real motions of the liquid determined by

$$\left.\begin{aligned} \psi &= \mathrm{A}_n \mathrm{P}_n(\mu') \, \mathrm{P}_n(\nu) \sin(2\omega\kappa t - \epsilon_n) \\ h &= \mathrm{C}_n \varpi \, \mathrm{P}_n(\mu) \sin(2\omega\kappa t - \epsilon_n) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (67),$$

ϵ_n being any constant.

These are *stationary oscillations* of the liquid about the spheroidal form. By what has already been shown, there are either $\frac{1}{2}n$ or $\frac{1}{2}(n+1)$ such free oscillations, according as n is even or odd. In all of these oscillations the expression for h is of the same form, that is, the corrugations produced on the surface are similar in each. But this will not be the case with the values of ψ, because the auxiliary systems of spheroidal coordinates to which they are referred are different for each different value of κ. Thus, the motions of the fluid particles in the interior of the mass are different for each of the oscillations.

13. Taking next the case when s is different from zero, let us change the sign of $\sqrt{-1}$ *everywhere* that it occurs in our investigations. The results will still hold good when this is done. Hence, for every root of (66) we get two solutions of the equations of oscillation, giving respectively

$$\psi = A_n^s T_n^{(s)}(\mu') T_n^s(\nu) e^{\iota(s\phi + 2\omega\kappa t)},$$
$$h = C_n^s \varpi T_n^{(s)}(\mu) e^{\iota(s\phi + 2\omega\kappa t)},$$

and also

$$\psi = A_n^s T_n^{(s)}(\mu') T_n^s(\nu) e^{-\iota(s\phi + 2\omega\kappa t)},$$
$$h = C_n^s \varpi T_n^{(s)}(\mu) e^{-\iota(s\phi + 2\omega\kappa t)},$$

which combine to give the real motions

$$\left.\begin{aligned} \psi &= A_n^s T_n^{(s)}(\mu') T_n^s(\nu) \sin(s\phi + 2\omega\kappa t - \epsilon_n^s) \\ h &= C_n^s \varpi T_n^{(s)}(\mu) \sin(s\phi + 2\omega\kappa t - \epsilon_n^s) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (68).$$

These represent a system of *waves* travelling round the axis of the spheroid with *relative* angular velocity $-2\omega\kappa/s$. But it must be remembered that the coordinate axes to which we have referred the wave motion are themselves rotating with angular velocity ω. Hence, the angular velocity of the waves in space is $\omega(1 - 2\kappa/s)$. According to our convention, positive values of κ give waves rotating more slowly than the liquid, and *vice versâ*.

There are $n - s + 2$ such waves determined by harmonics of degree n and rank s, and, since the values of κ are not equal and opposite in pairs, these waves do not combine into oscillations fixed relatively to the moving axes. As in the symmetrical oscillations, the form of the corrugations is the same for all the waves, but the motion of the fluid particles different in each.

14. If $\kappa_1, \kappa_2, \ldots, \kappa_{n-s+2}$ be the roots of (66), it is obvious that

$$\kappa_1 + \kappa_2 + \ldots + \kappa_{n-s+2} = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (69).$$

Hence, the *mean* relative angular velocity of *all* the different harmonic waves of degree n and rank s is

$$\frac{2\omega}{(n - s + 2)s},$$

in direction opposite to that of rotation of the liquid, whilst their mean actual angular velocity in space is

$$\omega\left\{1 - \frac{2}{(n - s + 2)s}\right\}.$$

Analysis of the Period-Equations.

15. From POINCARÉ'S investigations it appears that the spheroid will be secularly stable, even if the liquid be viscous, provided that the coefficients which are here denoted by $K_n^s(\zeta_0)$ or

$$p_1(\zeta)\, q_1(\zeta) - t_n^s(\zeta)\, u_n^s(\zeta)$$

are positive for all values of n greater than unity.* From our equations (49), (50) we have

$$-K_1^1(\zeta) = q_2(\zeta) = \omega^2/(4\pi\rho\gamma\,\zeta),$$

so that $K_1^1(\zeta)$ is essentially negative and $q_2(\zeta)$ positive.

In accordance with this, we shall now show that, if $K_n^s(\zeta)$ and $q_2(\zeta)$ be both positive, the roots of the period-equation for harmonic waves of degree n and rank s are all real, and we shall find their situations.

In the first place, let us suppose s is different from zero. The period-equation (66), as it stands, may be written

$$F(\kappa) = 0,$$

where

$$NF(\kappa) \equiv (1-\kappa^2\sin^2\alpha)^{(n-s)/2}\{4q_2(\cot\alpha)\,\kappa(\kappa-1) - sK_n^s(\cot\alpha)\}\,D^sP_n(\nu)$$
$$-(1-\kappa^2\sin^2\alpha)^{(n-s-1)/2}\sec\alpha\,K_n^s(\cot\alpha)\,(\kappa-1)\,D^sP_n(\nu),$$

if we write for brevity

$$N \equiv (2n)!/\{2^n n!\,(n-s)!\}.$$

We know that the roots of the equation

$$D^sP_n(\nu) = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (70)$$

are all real, and lie between $+1$ and -1; also they are separated by those of

$$D^{s+1}P_n(\nu) = 0.$$

Let $\kappa_1, \kappa_2, \ldots \kappa_{n-s}$ be the values of κ (taken in descending order of magnitude) corresponding to the roots of (70). These values of κ all lie between $+1$ and -1, also ν decreases as κ decreases. Moreover, if κ is put in turn equal to $\kappa_1, \kappa_2, \ldots \kappa_{n-s}$, the corresponding values of $D^{s+1}P_n(\nu)$ are alternately positive and negative.

We are now in a position to trace the changes in $F(\kappa)$ as κ decreases from $+\infty$ to $-\infty$.

When κ is greater than cosec α, ν is imaginary. But $F(\kappa)$ when written in the form of the left-hand side of (66) is obviously real; also when $\kappa = \infty$ the sign of $F(\kappa)$ is that of the coefficient of κ^{n-s+2}. It is therefore *positive*.

When κ passes through the value cosec α, ν becomes infinite and then becomes real, but $F(\kappa)$ does not in general change sign.

When $\kappa = 1$, $\nu = 1$, $D^sP_n(\nu)$ is positive, and $F(\kappa)$ is negative.

When $\kappa = \kappa_1$, $F(\kappa)$ is positive.

When $\kappa = \kappa_2$, $F(\kappa)$ is negative.

When $\kappa = \kappa_3$, $F(\kappa)$ is positive.

and so on; thus, when $\kappa = \kappa_{n-s}$, $F(\kappa)$ has the same sign as $\overline{-1}|^{n-s+1}$.

* We shall in future leave out the suffixes in ζ_0 and ν_0, using ζ, ν to denote the surface values, as these surface values alone occur in the remainder of our investigations.

In general $F(\kappa)$ does not change sign when $\kappa = -\operatorname{cosec}\alpha$, but when $\kappa = -\infty$, $F(\kappa)$ has the same sign as $\overline{-1}|^{n-s+2}$.

Hence, the equation $F(\kappa) = 0$ must have one real root between each of the following values :—

$$\infty,\ 1,\ \kappa_1,\ \kappa_2,\ \kappa_3,\ \ldots\ \kappa_{n-s},\ -\infty.$$

Thus, if $K_n^s(\zeta)$ is positive, all the roots of (66) are real. Let us now examine what happens when $K_n^s(\zeta)$ vanishes and becomes negative. POINCARÉ proves* that, if $t_n^s(\zeta)$ is divisible by ζ, the equation

$$K_n^s(\zeta) = 0$$

has no real root ; we must, therefore, have $n - s$ even, so that $t_n^s(\zeta)$ is not divisible by ζ.

When $K_n^s(\zeta)$ vanishes, the equation

$$F(\kappa) = 0$$

reduces to

$$\kappa(\kappa - 1)(1 - \kappa^2 \sin^2\alpha)^{\frac{1}{2}(n-s)} D^s P_n\{\kappa\cos\alpha(1 - \kappa^2\sin^2\alpha)^{-\frac{1}{2}}\} = 0,$$

of which the roots are

$$0,\ 1,\ \kappa_1,\ \kappa_2,\ \ldots,\ \kappa_{n-s}.$$

Since $n - s$ is even, the equation

$$D^s P_n(\nu) = 0$$

has not zero for one of its roots. Thus, the roots of the period-equation are all real and different. Therefore, when $K_n^s(\zeta)$ changes sign and becomes negative, the period-equation must, at any rate at first, continue to have all its roots real. If it have a pair of complex roots, the ratio of $K_n^s(\zeta) : q_2(\zeta)$ must not only be negative, but numerically greater than some *finite* limit.

16. The roots of the period-equations for the oscillations that are symmetrical about the axis of the spheroid are to be separated in exactly the same way. It will be sufficient to state the results here. We suppose $\kappa_1, \kappa_2, \kappa_3, \ldots, \kappa_n$ are the n values of κ which make

$$P_n(\nu) \equiv P_n\{\kappa\cos\alpha(1 - \kappa^2\sin^2\alpha)^{-\frac{1}{2}}\} = 0 \quad \ldots \quad (71).$$

Let n be odd. One of the above values, viz., $\kappa_{\frac{1}{2}(n+1)}$ will be zero, whilst $\kappa_n = -\kappa_1$, $\kappa_{n-1} = -\kappa_2$, and so on. Also, the ratio $K_n(\zeta) : q_2(\zeta)$ *must* be positive. It will be found that the period-equation (64) has one root between each of the following values of κ ;

$$\infty,\ \kappa_1,\ \kappa_2,\ \kappa_3,\ \ldots,\ \kappa_{\frac{1}{2}(n-1)},\ 0,\ \kappa_{\frac{1}{2}(n+3)},\ \ldots,\ \kappa_n,\ -\infty.$$

* 'Acta Mathematica,' vol. 7, p. 326.

If n be even, the least positive and negative roots of (71) are $\kappa_{\frac{1}{2}n}$ and $\kappa_{\frac{1}{2}n+1}$, also $\kappa_{\frac{1}{2}n+1} = -\kappa_{\frac{1}{2}n}$. If the ratio $K_n(\zeta) : q_2(\zeta)$ be positive, we find that the positive roots of the period-equation (62) are situated between the following values :—

$$\infty, \kappa_1, \kappa_2, \ldots, \kappa_{\frac{1}{2}n},$$

while the negative ones which are equal and opposite to them are situated in the intervals between

$$-\infty, \kappa_n, \kappa_{n-1}, \ldots, \kappa_{\frac{1}{2}n+1},$$

there being no roots between $\kappa_{\frac{1}{2}n}$ and $\kappa_{\frac{1}{2}n+1}$.

When $K_n(\zeta)$ vanishes, the roots are the n quantities

$$\kappa_1, \kappa_2, \ldots, \kappa_n,$$

none of which is equal to zero. If the ratio $K_n(\zeta) : q_2(\zeta)$ now become negative, the roots of (62) will at first continue to be real, being situated between the values

$$\kappa_1, \kappa_2, \ldots, \kappa_{\frac{1}{2}n}, 0, \kappa_{\frac{1}{2}n+1}, \ldots, \kappa_n.$$

This will be the case until we arrive at a value of ζ for which the period-equation has a pair of equal roots, each equal to zero. When this is so, we have

$$\frac{K_n(\zeta)}{4q_2(\zeta)} = \mathrm{L}^t_{\nu=0} \frac{\nu P_n(\nu)}{DP_n(\nu)} = -\frac{1}{n(n+1)},$$

whence,

$$K_n(\zeta) = -\frac{4}{n(n+1)} q_2(\zeta) = \frac{4}{n(n+1)} K_1^1(\zeta) \quad . \quad . \quad . \quad . \quad (72),$$

or

$$p_1(\zeta)\, q_1(\zeta) - p_n(\zeta)\, q_n(\zeta) = \frac{4}{n(n+1)} \{p_1(\zeta)\, q_1(\zeta) - t_1^1(\zeta)\, u_1^1(\zeta)\}.$$

When the ratio $K_n(\zeta) : -q_2(\zeta)$ or $K_n(\zeta) : K_1^1(\zeta)$ becomes greater than $4/\{n(n+1)\}$, two of the roots of the period-equation will become imaginary.

In *every* case there must be at least one positive root between each of the quantities

$$\kappa_1, \kappa_2, \ldots, \kappa_{\frac{1}{2}n}$$

and corresponding negative roots, so that under no circumstances can equation (62) have *more* than one pair of imaginary roots.

Numerical Solutions of the Period-Equations.

17. For a spheroid of given eccentricity, α, and therefore ζ, are known. Now, the functions $p_n(\zeta)$, $t_n^s(\zeta)$ can be expanded in finite terms of ζ in exactly the same way

as the ordinary spherical harmonics, while $q_n(\zeta)$, $u_n^s(\zeta)$ can be expressed in finite terms of ζ, $\cot^{-1}\zeta$, *i.e.*, of ζ, α; hence, the function $K_n^s(\zeta)$ can be calculated for any value of ζ.* By HORNER's method we may then approximate to the values of the roots of the equation in κ in the simpler cases. The periods of the waves are the corresponding values of $\pi/\kappa\omega$, while ω is expressed in terms of ρ by equation (50).†

To obtain some idea of the relative frequencies of the various waves, I have tabulated the values of κ thus calculated for harmonics of the second, third, and fourth degrees for a spheroid in which $\zeta = 1$ or $\alpha = \pi/4$, the eccentricity being, therefore, $\frac{1}{2}\sqrt{2}$. The results are embodied in the accompanying Table. As already stated, the positive roots correspond to waves rotating more slowly than the liquid, or relatively in the direction opposite to that of rotation of the mass, while those having the double sign correspond to symmetrical oscillations of the liquid.

TABLES of the Values of $\kappa \left(= \dfrac{\lambda}{2\omega}\right)$ for Waves on a Spheroid whose Eccentricity $= \sin\dfrac{\pi}{4} = \dfrac{\sqrt{2}}{2}$.

Rank of Harmonic.					
	I. Harmonics of the Second Degree.				
2 (sectorial)	1·2126108,	− 0·2126108.			
1	1·280776,	− 0·780776.			
0 (zonal) Oscillatory waves	± 1·128465.				
	II. Harmonics of the Third Degree.				
3 (sectorial)	1·569830,	− 0·569830.			
2	1·677377,	0·423263,	− 1·100640.		
1	1·6008928,	0·8267846,	− 0·0733654,	− 1·3543120.	
0 (zonal) Oscillatory waves	± 1·5178954,	± 0·5122368.			
	III. Harmonics of the Fourth Degree.				
4 (sectorial)	1·852560,	− 0·852560.			
3	1·806374,	0·366650,	− 1·173024.		
2	1·921878,	0·730910,	− 0·107365,	− 1·545423.	
1	1·924662,	0·890193,	0·585670,	− 0·654623,	− 1·745902.
0 (zonal)	± 1·994751,	± 0·685895.			

I find that the period of the symmetrical or zonal harmonic oscillation of the second degree (in which the surface remains spheroidal) is, in this spheroid, 0·8599258

* "On the Expression of Spherical Harmonics of the Second Kind in a Finite Form," 'Cambridge Philosophical Proceedings,' December, 1888.

† See THOMSON and TAIT's 'Natural Philosophy,' vol. 2, § 772.

of the corresponding time of oscillation in a non-rotating spherical mass of liquid of the same density.

Sectorial Harmonic Waves.

18. When $s = n$, $D^s P_n(\nu)$ is numerical, and $D^{s+1} P_n(\nu)$ is zero; thus, the period-equation reduces to

$$4\kappa(\kappa - 1) = n K_n^n(\zeta)/q_2(\zeta) \quad \ldots \ldots \ldots \quad (73),$$

of which the roots are given by

$$\kappa = \tfrac{1}{2}\{1 \pm \surd(1 + n K_n^n(\zeta)/q_2(\zeta))\} \quad \ldots \ldots \ldots \quad (74).$$

The condition that these roots may be real is that

$$q_2(\zeta) + n K_n^n(\zeta) > 0 \quad \ldots \ldots \ldots \quad (75),$$

that is

$$p_1(\zeta)\, q_1(\zeta) - t_n^n(\zeta)\, u_n^n(\zeta) - \frac{1}{n}\{p_1(\zeta)\, q_1(\zeta) - t_1^1(\zeta)\, u_1^1(\zeta)\}$$

must be positive.

These results have been obtained previously by POINCARÉ in the special case in which $n = 2$, but in his investigation an extraneous factor has been introduced into the period-equation, giving a third root ($\kappa = 1$) which does not properly belong to it.

The expression for ψ in (21) is here proportional to

$$(1 - \mu'^2)^{\frac{1}{2}n} (\nu^2 - 1)^{\frac{1}{2}n} e^{\iota(n\phi + 2\omega\kappa t)},$$

that is, in Cartesian coordinates, to

$$(x + \iota y)^n e^{2\iota\omega\kappa t},$$

and is independent of z.

Thus, the motion of the liquid is "two dimensional," and takes place in planes parallel to the equatorial plane of the spheroid. By the laws of vortex motion the molecular rotation or spin of the actual motion of the liquid is therefore everywhere constant and equal to ω, being that due to the rotation of the liquid. In other words, the wave motion of the liquid *relative* to the rotating axes is irrotational.

Small Free Precession of the Spheroid.

19. Another case of some interest is when the harmonics determining the small periodic relative motions are of the second degree and first rank. Putting $n = 2$, $s = 1$, the period-equation (66) reduces to

$$4\kappa^2(\kappa - 1)\, q_2(\cot\alpha) - [(\kappa - 1)\sec^2\alpha + \kappa]\, K_2^1(\cot\alpha) = 0 \quad \ldots \quad (76).$$

Now, whatever the value of α may be, $\kappa = \frac{1}{2}$ is always a root of this equation. For, if we put $\kappa = \frac{1}{2}$ in the left-hand side, it becomes

$$
\begin{aligned}
&= -\tfrac{1}{2}q_2(\cot\alpha) + \tfrac{1}{2}\tan^2\alpha\, \mathrm{K}_2{}^1(\cot\alpha) \\
&= \tfrac{1}{2}\{\mathrm{K}_1{}^1(\zeta) + \mathrm{K}_2{}^1(\zeta)/\zeta^2\} \\
&= \tfrac{1}{2}\{(\zeta^2+1)p_1(\zeta)\,q_1(\zeta)/\zeta^2 - t_1{}^1(\zeta)\,u_1{}^1(\zeta) - t_2{}^1(\zeta)\,u_2{}^1(\zeta)/\zeta^2\} \\
&= \tfrac{1}{2}(\zeta^2+1)\int_\zeta^\infty \left\{\frac{1}{\zeta^2} - \frac{1}{\zeta^2+1} - \frac{1}{\zeta^2(\zeta^2+1)}\right\}\frac{d\zeta}{\zeta^2+1} = 0 \quad . \quad . \quad . \quad . \quad (77),
\end{aligned}
$$

as was to be proved.

Substituting from the relation just found in (76), the period-equation becomes

$$(4\kappa^3 - 4\kappa^2 + \kappa)\tan^2\alpha - (2\kappa - 1)\sec^2\alpha = 0,$$

or, dividing throughout by $(2\kappa - 1)\tan^2\alpha$, the other two roots are given by

$$2\kappa^2 - \kappa - \operatorname{cosec}^2\alpha = 0,$$

whence

$$
\begin{aligned}
\kappa &= \tfrac{1}{4}\{1 \pm \sqrt{(1 + 8\operatorname{cosec}^2\alpha)}\} \\
&= \tfrac{1}{4}\{1 \pm \sqrt{(9 + 8\zeta^2)}\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (78).
\end{aligned}
$$

The expression for ψ is proportional to

$$(1 - \mu'^2)^{\frac{1}{2}}\,\mu'(\nu^2 - 1)^{\frac{1}{2}}\,\nu\sin(\phi + 2\omega\kappa t - \epsilon),$$

that is, to

$$z\{x\sin(2\omega\kappa t - \epsilon) + y\cos(2\omega\kappa t - \epsilon)\},$$

while the height of the displacement of the surface is proportional to

$$\varpi z\{x\sin(2\omega\kappa t - \epsilon) + y\cos(2\omega\kappa t - \epsilon)\}.$$

Remembering that this displacement is so small that its square may be neglected, it can be readily shown by the usual methods of analytical geometry that, if, as is here supposed, the ellipticity of the spheroid be finite, the displaced surface is a spheroid of the same form and dimensions as the original spheroid, and can be obtained by turning the latter through a small angle about the line

$$x\sin(2\omega\kappa t - \epsilon) + y\cos(2\omega\kappa t - \epsilon) = 0, \qquad z = 0.$$

This will, however, no longer be true if the ellipticity of the spheroid is a small quantity comparable with the height of the small displacement, or the surface is spherical or nearly spherical. In such cases it will be found that the displaced surface is an ellipsoid, differing in form from the original spheroid by small quantities of the first order, whose axes make finite, not small, angles with those of the spheroid.

Suppose the liquid spheroid is rotating steadily about its axis of figure with angular velocity ω, and that this axis does not quite coincide with our fixed axis of z, but is inclined to it at a small angle, while the axes of x, y rotate about the axis of z with angular velocity ω. The coordinates of the fluid particles will now no longer be constant, but will undergo small periodic changes. In the time $2\pi/\omega$ both the fluid particles and the axes will come round to their original positions; thus, the period of the *apparent* relative oscillations is $2\pi/\omega$, although the liquid is in reality rotating steadily. This accounts for the root $\kappa = \frac{1}{2}$, which occurs in the period-equation, a result which may be completely verified by rigorous analytical methods.

The movements corresponding to the other two roots are somewhat similar to precession, the axis of figure of the spheroid turning about the axis of z, to which it is inclined at a small angle.

Stability of the Spheroid.

20. We have already alluded to POINCARÉ's investigations of the condition that the spheroid, if viscous, may be secularly stable, which requires that the energy of the system for the given angular momentum must be a minimum in the spheroidal form. The greatest eccentricity corresponds to the least value of ζ which causes any one of the coefficients $K_n^s(\zeta)$ to vanish, and this is shown to be that given by $K_2^2(\zeta) = 0$, whence, as in THOMSON and TAIT (§ 772),

$$1/\zeta = \tan \alpha = f = 1{\cdot}39457.$$

If the liquid be perfectly inviscid, the criteria are very different. So long as the roots of the period-equations for the various waves and oscillations are all real, the spheroid cannot be unstable. It will, however, become unstable if for any harmonic the equation in κ has a pair of complex or imaginary roots. For, calling these roots $l \pm m\iota$, we get the possible surface displacements

$$h = C_n^s \varpi T_n^{(s)}(\mu)\, e^{\iota s\phi} e^{2\omega(\iota l + m)t},$$
$$h = C_n^s \varpi T_n^{(s)}(\mu)\, e^{-\iota s\phi} e^{2\omega(-\iota l + m)t},$$

compounding into the displacement

$$h = C_n^s \varpi T_n^{(s)}(\mu) \cos(s\phi + 2\omega lt)\, e^{2\omega mt},$$

which increases indefinitely with the time.

Let us imagine that our spheroid is subject to constraints such as to freely allow of its surface undergoing harmonic displacements of degree n and rank s, but which allow of no other displacements (such constraints are, of course, purely theoretical). The spheroid, if at all viscous, will be secularly stable or unstable according as

$$K_n^s(\zeta) > 0 \quad \text{or} \quad < 0,$$

and we have seen that the latter condition can only hold if $n - s$ be even, that is, if the displacement be one symmetrical with respect to the equatorial plane. The critical form is that in which

$$K_n^s(\zeta) = 0.$$

But since, when $K_n^s(\zeta)$ changes sign, the roots of the period-equation at first continue real, the limits of eccentricity for which the perfect spheroid is "ordinarily" stable are in every case wider than those consistent with secular stability if the liquid be viscid. The critical form is determined by the condition that the period-equation must have a pair of equal roots.

In my paper on "The Waves on a Viscous Rotating Cylinder"* I have endeavoured to further elucidate the difference between "ordinary" and "secular" stability. Assuming the displacement from relative equilibrium to be proportional to $e^{-\alpha_1 t}$, α_1 is always complex for viscous liquid, and the condition that the disturbance may not increase with the time is that the real part of α_1 must be positive. Both real and imaginary parts change sign when $\alpha_1 = 0$, the corrugations becoming relatively fixed and the liquid figure becoming a form of "bifurcation." Relative equilibrium is then critical. But, if there be no viscosity, α_1 may be purely imaginary, as in the present case, when κ is real, and the waves will neither increase nor diminish in amplitude with the time; thus, a change in the sign of one of the roots of the period-equation merely implies a change in the relative direction of the wave.

Moreover, it appears that the criteria of ordinary and secular stability will be different only if the angular velocities of the waves be different in the two opposite directions, and this can only be the case if the liquid be rotating.

Reverting to the perfect liquid spheroid, the determination of the greatest eccentricity consistent with ordinary stability involves the question, if ζ be gradually diminished, what is the harmonic displacement for which the period-equation of the waves first commences to have complex roots? It appears probable that this happens for $n = 2$, $s = 2$. With this assumption, we see, by (75), that the critical value of ζ is given by the equation

$$2\{p_1(\zeta)\,q_1(\zeta) - t_2^2(\zeta)\,u_2^2(\zeta)\} + t_1^1(\zeta)\,u_1^1(\zeta) - p_1(\zeta)\,q_1(\zeta) = 0\,;$$

this leads to

$$(3\zeta^4 + 8\zeta^2 + 1)\cot^{-1}\zeta - (3\zeta^3 + 7\zeta) = 0,$$

* 'Cambridge Philosophical Proceedings,' 1888.

whence I find by trial and error that

$$1/\zeta = \tan\alpha = f = 3{\cdot}1414567 \ldots .$$

This result agrees with that found by RIEMANN, who treated the problem as a special case of the general motion of a liquid ellipsoid.

It does not, however, seem possible to justify the above assumption as to the nature of the displacements by a perfectly general rigorous proof. The condition that the period-equation should have a pair of equal roots is far too complicated to allow of this point being fully proved in the way that POINCARÉ has done for secular stability. It is certain that the spheroid will be unstable for all values of $\tan\alpha$ greater than $3{\cdot}1414567$; it is probable, but not certain, that it will be stable for all values less than this limit.

Spheroids of Small Ellipticity.

21. If the eccentricity of the spheroid, and, therefore also, its angular velocity, be small, the period-equations for the waves are much modified. The value of ζ will become very great, and we shall suppose it to be so great that ζ^{-2} is a small quantity that can be neglected. Since α is small, we may put $\cos\alpha = 1$, $\sin\alpha = \alpha = 1/\zeta$.

The function $t_n^s(\zeta)$ is proportional to ζ^n since the other terms in it involve only ζ^{n-2} and lower powers of ζ. Hence, to this approximation,

$$t_n^s(\zeta)\, u_n^s(\zeta) = \zeta^{2n}\int_\zeta^\infty \frac{d\zeta}{\zeta^{2n+2}} = \frac{\zeta^{-1}}{2n+1} \quad \ldots\ldots \quad (79),$$

and

$$\mathrm{K}_n^s(\zeta) = \zeta^{-1}\left(\tfrac{1}{3} - \frac{1}{2n+1}\right) = \frac{2\,(n-1)}{3\,(2n+1)}\,\zeta^{-1} \quad \ldots\ldots \quad (80);$$

moreover,

$$q_2(\zeta) = -\,\mathrm{K}_1^1(\zeta) = \tfrac{2}{15}\,\zeta^{-3} \quad \ldots\ldots \quad (81),$$

whence, as in THOMSON and TAIT (§ 771),

$$\omega^2 = \tfrac{8}{15}\,\pi\rho\gamma\,\zeta^{-2} \quad \ldots\ldots \quad (82).$$

Firstly, suppose that the values of κ remain finite in the limit. Then $\nu = \kappa$ ultimately, and, since $q_2(\zeta)$ is negligible in comparison with $\mathrm{K}_n^s(\zeta)$, equation (65) gives

$$s\mathrm{D}^s\mathrm{P}_n(\kappa) + (\kappa - 1)\,\mathrm{D}^{s+1}\mathrm{P}_n(\kappa) = 0 \quad \ldots\ldots \quad (83),$$

having $n - s$ real roots between 1 and -1.

In the case of the oscillations symmetrical about the axis ($s = 0$) the equation for κ is ultimately

$$\mathrm{DP}_n(\kappa) = 0 \qquad \text{or} \qquad \frac{1}{\kappa}\,\mathrm{DP}_n(\kappa) = 0 \quad \ldots\ldots \quad (84),$$

according as n is odd or even.

The frequencies of these waves or oscillations are proportional to the angular velocity of the liquid. As the latter is diminished without limit they become relatively unimportant, and finally cease to exist, for the limiting case of a mass of liquid without rotation oscillating about the spherical form.

Secondly, suppose the periods of the waves remain finite in the limit. Put $2\omega\kappa = \lambda$, so that $2\pi/\lambda$ is the period. Since λ remains finite, κ will increase without limit as ω diminishes, and, therefore, equation (66) gives, to the first order of small quantities,

$$\frac{2\zeta^{-2}}{15}\frac{\lambda(\lambda-2\omega)}{\omega^2} - \frac{2}{3}\frac{(n-1)}{(2n+1)}s - \frac{2}{3}\frac{(n-1)}{(2n+1)}(n-s)\frac{\lambda-2\omega}{\lambda} = 0,$$

whence, by (82),

$$\lambda(\lambda-2\omega) = \frac{8n(n-1)}{3(2n+1)}\pi\rho\gamma\left\{1 - \frac{n-s}{n}\cdot\frac{2\omega}{\lambda}\right\} \quad . \quad . \quad . \quad . \quad (85).$$

If we put $\omega = 0$, we get

$$\lambda^2 = \frac{8n(n-1)}{3(2n+1)}\pi\rho\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (86),$$

the well-known result for the oscillations of a liquid sphere. Denoting by Λ^2 the expression

$$\frac{8}{3}\frac{n(n-1)}{(2n+1)}\pi\rho\gamma,$$

we find

$$\lambda^2 - \Lambda^2 = \frac{2\omega}{\lambda}\left(\lambda^2 - \frac{n-s}{n}\right),$$

whence, substituting $\lambda = \pm\Lambda$ in the small terms, we get

$$\lambda = \pm\Lambda + \frac{s}{n}\omega \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (87).$$

Remembering the expressions found in § 13 for the relative and actual angular velocities of the corresponding waves, this result may be stated as follows :—The effect of communicating a small angular velocity ω to a spherical mass of gravitating liquid will be to add an angular velocity $(n-1)\,\omega/n$ to the angular velocities of all the free waves which are determined by harmonics of degree n.

The symmetrical oscillations will be unaffected by rotation to this order of approximation. If we proceed to a higher approximation by taking into account small quantities of the second order, the equations become much more complicated. But, for a spheroid similar to the Earth, the above approximation would be practically sufficient.

Forced Tides.

22. We now revert to the applications of the methods of this paper to the investigation of the tides produced on the surface of the spheroid by the influence of periodic variations of pressure over the surface of the liquid, or by the attractions of disturbing bodies in the neighbourhood of the spheroid.

In this connexion, the equations found in §§ 9, 10 will be required, viz., if at the surface

$$V_2 - p_2/\rho = \Sigma\Sigma\Sigma W^s_{(n,\kappa)} T_n^{(s)}(\mu) \sin(s\phi + 2\omega\kappa t - \epsilon_{n\kappa}^{\ s}) \quad . \quad . \quad . \quad (88),$$

and

$$h = \varpi\, \Sigma\Sigma\Sigma C^s_{(n,\kappa)} T_n^{(s)}(\mu) \sin(s\phi + 2\omega\kappa t - \epsilon_{n\kappa}^{\ s}) \quad . \quad . \quad . \quad . \quad (89),$$

then $C^s_{(n,\kappa)}$ will be given in terms of $W^s_{(n,\kappa)}$ by the relation

$$3\frac{M\gamma}{c} C^s_{(n,\kappa)} \left\{ K_n^{\ s}(\zeta) - \frac{4\kappa D^s P_n(\nu)}{sD^s P_n(\nu)/(\kappa-1) + \sec^2\alpha \,.\, \nu D^{s+1} P_n(\nu)/\kappa}\, q_2(\zeta) \right\} = W^s_{(n,\kappa)} \quad (90),$$

where, as in (12), (15),

$$\left.\begin{aligned} \nu &= \frac{\kappa\cos\alpha}{\sqrt{(1-\kappa^2\sin^2\alpha)}}, \\ \text{and} \qquad \zeta &= \cot\alpha \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (91).$$

Also, from (39), we have

$$12\frac{M\gamma}{c} C^s_{(n,\kappa)} \frac{4\kappa D^s P_n(\nu)\, q_2(\zeta)}{sD^s P_n(\nu)/(\kappa-1) + \sec^2\alpha \,.\, \nu D^{s+1} P_n(\nu)/\kappa} = -A^s_{(n,\kappa)} T_n^{\ s}(\nu) \quad . \quad (92).$$

The value of ψ at any point of the surface being

$$[\psi] = A^s_{(n,\kappa)} T_n^{\ s}(\nu)\, T_n^{(s)}(\mu) \sin(s\phi + 2\omega\kappa t - \epsilon_{n,\kappa}^{\ s}) \quad . \quad . \quad . \quad . \quad (93),$$

is determined in terms of $C^s_{(n,\kappa)}$ by the last equation (92).

23. An interesting case occurs when $\kappa = 1$. The period of the tides will then be half that of a complete revolution of the liquid, and they may therefore be called "semidiurnal" with reference to the spheroid. Except in the case when $s = 0$, equation (90) gives

$$3\frac{M\gamma}{c} C_{(n)}^{\ s} K_n^{\ s}(\zeta) = W_n^{\ s} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (94);$$

also, from (92), $A_n^{\ s} T_n^{\ s}(\nu) = 0$, and, therefore, $[\psi] = 0$; hence, it is evident that ψ must also vanish throughout the liquid.

The height of the forced tides is therefore the same as we should get by the "equilibrium theory," *i.e.*, by neglecting the small relative motions of the fluid particles entirely. In fact, these relative motions have no effect on the height of the tides. It does not follow that they do not exist; in fact, it is evident, on the contrary, since the tides move relatively to the liquid, that they must exist. But on referring to equations (34) we see that, when $\kappa = 1$, the small relative velocity components U, V, W may be finite, even though ψ vanishes.

The zonal oscillations are, however, given by

$$3\,\frac{\mathrm{M}\gamma}{c}\,\mathrm{C}_n\left\{\mathrm{K}_n(\zeta) - \frac{8\cos^2\alpha\, q_2(\zeta)}{n\,(n+1)}\right\} = \mathrm{W}_n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (95),$$

since $\nu = 1$, and therefore $\mathrm{P}_n(\nu) = 1$, $\mathrm{DP}_n(\nu) = \frac{1}{2}n\,(n+1)$. For these oscillations ψ does not vanish.

24. Another interesting application is to determine the height of the permanent corrugations produced by disturbing forces which remain constant and fixed relatively to the rotating liquid. We now have to take $\kappa = 0$; therefore, $\nu = 0$ and $\nu/\kappa = \cos\alpha$.

If s is different from zero, then, whether $n - s$ be odd or even, equation (90) gives us

$$3\,(\mathrm{M}\gamma/c)\,\mathrm{C}_n^s\mathrm{K}_n^s(\zeta) = \mathrm{W}_n^s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (94),$$

and (92) gives $\mathrm{A}_n^s\mathrm{T}_n^s(\nu) = 0$, whence $[\psi] = 0$; and therefore ψ is everywhere zero.

If $s = 0$ and n is odd, then $\mathrm{P}_n(0) = 0$, $\mathrm{DP}_n(0)$ is finite, and, as before, we find

$$3\,(\mathrm{M}\gamma/c)\,\mathrm{C}_n\mathrm{K}_n(\zeta) = \mathrm{W}_n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (94\text{A}),$$

and

$$\psi = 0.$$

Lastly, let $s = 0$ and let n be even. This is the case of a harmonic disturbance which is symmetrical both about the axis of the spheroid and also with respect to its equatorial plane. Then, as in § 16, $4\nu\mathrm{P}_n(\nu)/\mathrm{DP}_n(\nu)$ approaches the finite limit $-4/\{n\,(n+1)\}$, when ν is diminished indefinitely, and therefore

$$3\,\frac{\mathrm{M}\gamma}{c}\,\mathrm{C}_n\left\{\mathrm{K}_n(\zeta) - \frac{4q_2(\zeta)}{n\,(n+1)}\right\} = \mathrm{W}_n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (96),$$

$$[\psi] = -\,\frac{12}{n\,(n+1)}\,\frac{\mathrm{M}\gamma}{c}\,\mathrm{C}_n q_2(\zeta)\,\mathrm{P}_n(\mu) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (97).$$

In the first two cases the height of the corrugations is given by the "equilibrium theory," and, since $\psi = 0$, it follows from equations (34) that U, V, W are all zero. Thus, the fluid continues to rotate as if rigid in a form differing slightly from the original spheroid, as we should most naturally expect. But in the last case, since ψ is finite, there will be a finite relative motion of the liquid with respect to the moving axes. That this must be the case may be seen as follows :—The spheroid is supposed to be deformed from its original form by the action of the given conservative forces and surface pressures. The displacement does not vanish at the equator ; hence, if we consider the fluid particles at the surface, forming a circle round the equator, the displacement must necessarily increase or diminish the size of this circle. By THOMSON'S circulation theorem, the circulation in this circuit must remain the same as before, since the liquid is supposed perfect ; hence, the angular velocities of the fluid particles in this circle must be altered, and they can no longer continue to rotate about the axis of the spheroid with the original angular velocity ω. Therefore the disturbance must produce permanent relative motions of the liquid, unless there be any viscosity present, in which case the mass will ultimately rotate as if rigid in the deformed figure, and the "equilibrium theory" will again become applicable.

Tides due to Action of a Satellite.

25. We shall conclude by showing how to determine the forced tides due to the presence of a small satellite of mass m revolving in any orbit about the spheroid.

If we take ϕ' to be the longitude of any point on the spheroid, measured from a plane fixed in space, with which the moving plane of (y, z) coincides at time $t=0$, then, ϕ being the longitude measured from the latter plane, we have

$$\phi' = \phi + \omega t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (98).$$

Let $(\mu_1, \zeta_1, \phi'_1)$ be the spheroidal coordinates of the mass m at time t, ϕ'_1 being measured from the fixed initial plane. Then, at any point (μ, ζ, ϕ') whose distance from the mass is R, we have

$$V_2 = m\gamma/R \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (99).$$

Since $\zeta_1 > \zeta$, $1/R$ can be expanded in spheroidal harmonics by the formula

$$1/R = 1/c\, \Sigma_{n=0}^{n=\infty} (2n+1)\big[P_n(\mu)\, p_n(\zeta)\, P_n(\mu_1)\, q_n(\zeta_1)$$
$$+ 2\, \Sigma_{s=1}^{s=n} \{(n-s)!/(n+s)!\}\, T_n^{(s)}(\mu)\, t_n^{\,s}(\zeta)\, T_n^{(s)}(\mu_1)\, u_n^{\,s}(\zeta_1) \cos s(\phi' - \phi'_1)\big] \quad (100).$$

Since the motion of the satellite is supposed known, $(\mu_1, \zeta_1, \phi'_1)$ are known functions of t. In order to complete the solution we must suppose the quantities $P_n(\mu_1)\, q_n(\zeta_1)$, $T_n^{(s)}(\mu_1)\, u_n^{\,s}(\zeta_1) \cos s\phi'_1$, and $T_n^{(s)}(\mu_1)\, u_n^{\,s}(\zeta_1) \sin s\phi'_1$ expanded by FOURIER'S theorem in simple harmonic functions of the time. If the period of the satellite in its orbit be $2\pi/L$, the expansion will only involve circular functions of multiples of Lt. We then write $\phi + \omega t$ for ϕ', and resolve all *products* of sines and cosines involving ϕ or t in terms of circular functions of sums and differences. The expression V_2 will now be a triple series of the same form as in equation (88), and the heights of the tides due to each term of the series may be determined separately by the application of equations (89), (90).

26. Suppose that the attracting mass rotates round the spheroid in the equatorial plane in a circle of radius $c\sqrt{(Z^2+1)}$ with angular velocity $(1-2l)\,\omega$ (referred to fixed axes).

We then have $\mu_1 = 0$, $\zeta_1 = Z$, $\phi'_1 = (1-2l)\,\omega t$, $\phi' - \phi'_1 = \phi + 2l\omega t$. Therefore

$$V_2 = m\gamma/c\,\Sigma_{n=1}^{n=\infty}(2n+1)\left[P_n(\mu)\,p_n(\zeta)\,P_n(0)\,q_n(Z)\right.$$
$$\left. + 2\Sigma_{s=1}^{s=n}\{(n-s)!/(n+s)!\}\,T_n^{(s)}(\mu)\,t_n^s(\zeta)\,T_n^{(s)}(0)\,u_n^s(Z)\cos s(\phi + 2l\omega t)\right] \quad (101).$$

We have

$$P_n(0) = 0, \ (n \text{ odd}),$$
$$P_n(0) = \frac{(-1)^{\frac{1}{2}n}.\,n!}{2^n(\frac{1}{2}n!)^2}, \ (n \text{ even}),$$
$$T_n^{(s)}(0) = 0, \ (n - s \text{ odd}),$$
$$T_n^{(s)}(0) = \frac{(-1)^{\frac{1}{2}(n+s)}(n+s)!}{2^n.\frac{1}{2}(n+s)!.\frac{1}{2}(n-s)!}, \ (n - s \text{ even}),$$

so that the expansion only involves harmonics which are symmetrical with respect to the equatorial plane.

The first term in the above expansion is a harmonic of the first degree and rank, and determines the motion of the spheroid as a whole about the centre of mass of the spheroid and the attracting body. This motion can be taken separately.

Taking, in the usual way,

$$h = \varpi\,\Sigma_{n=2}^{n=\infty}\{C_nP_n(\mu) + \Sigma_{s=1}^{s=n}C_n^sT_n^{(s)}(\mu)\cos s(\phi + 2l\omega t)\} \quad . \quad . \quad . \quad (102),$$

where the summation includes only even values of $n - s$, we find, by the method of § 24,

$$C_n = \frac{2n+1}{3}\,\frac{m}{M}\,\frac{p_n(\zeta)\,P_n(0)\,q_n(Z)}{K_n(\zeta) - 4q_2(\zeta)/\{n(n+1)\}}$$
$$= (-1)^{\frac{1}{2}n}.\tfrac{1}{3}(2n+1)\frac{m}{M}\frac{n!}{2^n(\frac{1}{2}n!)^2}\cdot\frac{p_n(\zeta)\,q_n(Z)}{K_n(\zeta) - 4q_2(\zeta)/\{n(n+1)\}}, \ (n \text{ even}) \quad (103);$$

also, if $n - s$ be even,

$$C_n^s = (-1)^{\frac{1}{2}(n+s)}\tfrac{2}{3}(2n+1)\frac{m}{M}\cdot\frac{n-s!}{2^n.\frac{1}{2}(n+s)!\,\frac{1}{2}(n-s)!}\,t_n^s(\zeta)\,u_n^sZ$$
$$\div\left\{K_n^s(\zeta) - \frac{4ls(ls-1)\,q_2(\zeta)\,D^sP_n(\nu_s)}{sD^sP_n(\nu_s) - (ls-1)\sec^2\alpha\,.\,\nu_sD^{s+1}P_n(\nu_s)/(ls)}\right\} \quad . \quad (104),$$

where, in this case,

$$\nu_s = ls \cos \alpha \, (1 - l^2 s^2 \sin^2 \epsilon)^{-\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (105),$$

the corresponding value of κ being ls.

Equations (102), (103), (104) fully determine the height of the forced tides and deformations on the surface of the spheroid due to the attracting body.

If, instead of one attracting mass m, we suppose two equal masses $\frac{1}{2}m$ on opposite sides of the body, the expression for V_2, and therefore for h, will contain only zonal harmonics, and harmonics of even rank, but these will be the same as before.

Harmonic Tides of the Second Order.

27. If the body be very distant from the spheroid, Z will be great, and the series (101) will converge very rapidly, so that the harmonics of the second degree are the most important.

Taking $n = 2$, $s = 2$, we get

$$C_2^2 = \frac{5}{12} \frac{m}{M} \cdot \frac{t_2^2(\zeta)\, u_2^2(Z)}{K_2^2(\zeta) - 4l(2l-1)\, q_2(\zeta)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (106),$$

and the corresponding term in the height of the forced tide is

$$h_2^2 = \frac{15}{4} \frac{m}{M} \frac{(\zeta^2 + 1)\, u_2^2(Z)}{K_2^2(\zeta) - 4l(2l-1)\, q_2(\zeta)} (1 - \mu^2)\, \varpi \cos 2(\phi + 2m\,\omega t) \quad . \quad (107).$$

If the attracting body be rotating in the same direction as the liquid, but with less angular velocity (as in all cases of astronomical interest), $2l$ lies between 0 and 1. If, in addition, $K_2^2(\zeta)$ is positive, the denominator in the expression for h_2^2 cannot vanish, and the tide produced by this term cannot therefore become very large. In fact, the angular velocities of both the free harmonic tides will lie beyond the above-mentioned limits, and will neither of them coincide with that of the attracting body. If, however, $K_2^2(\zeta) = 0$, and if, in addition, the attracting body be fixed in space, so that $2l = 1$, this forced tide will increase indefinitely. The spheroid will now have a semi-diurnal free tide, which will be fixed in space, and will coincide with the forced tide due to the attracting body. The equilibrium in the spheroidal form will therefore be completely broken up.

If the attracting body be rotating very slowly about the spheroid, the same thing will happen if $K_2^2(\zeta)$ has a certain corresponding small negative value.

Since the spheroid is secularly stable or unstable according as $K_2^2(\zeta)$ is positive or negative, the mode in which its relative equilibrium will be destroyed if $K_2^2(\zeta)$ becomes negative will depend on circumstances. If the liquid possess but little viscosity, the changes due to secular instability will take place very slowly, but the effect of the tide generating force due to an attracting body when the angular velocity of one of the free tides coincides with that of the body will become very great. If, however, the viscosity be considerable, it will prevent the forced tides from becoming large, and will cause the liquid to rapidly assume the form of a JACOBIAN ellipsoid, owing to the spheroidal form being secularly unstable.

Conclusion.

28. The results of the present paper suggest several considerations, which may possibly throw further light on the past history of the Solar system. The criteria of stability applicable to the two cases where the spheroid is formed of perfect and of viscous liquid respectively have been already discussed in § 20. In the last paragraph I have alluded to the possibility that equilibrium in the spheroidal form may be broken up by an attracting body which causes the harmonic tides of the second order to increase indefinitely, in accordance with Professor DARWIN's hypothesis.

The present analysis, however, suggests that the same thing may happen in the case of harmonic tides of higher order than the second, and, moreover, the results arrived at concerning the number and situation of the roots of the frequency-equation render this hypothesis quite admissible. Except for harmonics of the second order, many of the free tides will rotate in the same direction as the liquid, but with less angular velocity, even though the spheroid be secularly stable; and, if an attracting body should be rotating about the spheroid with the same angular velocity as one of these tides, they would certainly rise to an enormous height, and the liquid might, perhaps, ultimately be broken up into two or more detached masses.

Take, for example, the sectorial harmonic waves of order n. If one of these be fixed in space, we must, from § 13, have the corresponding value of $\kappa = \frac{1}{2} n$, and, by (73), this leads to the condition

$$K_n^n(\zeta) = (n - 2)\, q_2(\zeta).$$

If n is greater than 2, this will be satisfied for some secularly stable form of the spheroid. Under these circumstances, the presence of a fixed attracting body near the spheroid would cause the sectorial harmonic waves of the n^{th} order to increase indefinitely. The only obstacle in the way of the present supposition is that when the distance of the attracting body is at all considerable, the harmonic components of tide generating force of the higher orders become very small in comparison.

Another question of astronomical interest is whether a rotating spheroid can be broken up into one or more *rings* of rotating liquid. This can only happen if one of the zonal harmonic oscillations increase indefinitely or become unstable.

Now, as long as the spheroid continues stable for such zonal harmonic displacements when the liquid is supposed perfect, the frequencies of these oscillations will none of them vanish ; hence, their amplitude cannot be increased indefinitely by the attractions of bodies remaining in the equatorial plane of the spheroid ; and the only way in which this can take place is under the influence of the tide generating force due to a satellite whose orbit is inclined at a considerable angle to the equatorial plane of the spheroid, the effect being greatest if their planes be perpendicular. There is still the possibility that, contrary to our hypothesis in § 20, a perfect liquid spheroid may first become unstable for some displacement symmetrical about the axis ; and, unless this question be fully decided, we are not in a position to say that such is not the case, and that LAPLACE'S hypothesis is wholly unfounded.

VII. *On the Magnetisation of Iron and other Magnetic Metals in very Strong Fields.*

By J. A. Ewing, *B.Sc., F.R.S., Professor of Engineering in University College, Dundee, and* William Low.

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§ 1. Early in 1887 we communicated to the Royal Society a short account of experiments made to examine the magnetic behaviour of iron when subjected to strong magnetic force by what we called the "isthmus" method of magnetisation.* Since then the experiments have been continued and extended by applying stronger magnetic forces, and by testing samples of nickel, cobalt, and various steels, as well as wrought iron and cast iron.† It may be well to preface an account of the more recent experiments by a short summary of the results stated in our earlier paper.

§ 2. The method of experiment consisted in turning the piece of metal whose magnetisation was to be examined to the form of a bobbin with a narrow neck or isthmus, and placing that between the pole-pieces of a powerful electromagnet. The sample was furnished with a spreading cone at each end, to facilitate the convergence of the lines of magnetic induction upon the central neck. The magnetisation was measured by means of an induction coil of fine wire wound in a single layer, or, in some cases, in two layers, upon the metal of the neck. Outside of this coil, and at a small definite distance from it, a second induction coil was wound in order to measure the magnetic field in the space between the two coils. This served a double purpose: it enabled a proper deduction to be made from the values of the induction measured within the inner coil, to allow for the space between the surface of the iron neck and the centre of the thickness of the coil; and it gave values of the magnetic force in the space immediately surrounding the iron, from which an inference might be drawn as to the value of the force within the neck itself. As there was no free magnetism on the circumference of the neck, in the medial plane, the force within the metal was continuous there with the force outside, and it will be shown later that when a suitable slope was given to the conical ends of the bobbin the variation of force in the medial plane in directions at right angles to the axis was so small that the external field

* "On the Magnetisation of Iron in Strong Fields," 'Roy. Soc. Proc.,' vol. 42, p. 200.

† Preliminary notices of some of the later results were communicated to Section A of the British Association at Manchester ('Report of the British Association for 1887,' pp. 586 and 587).

must have formed (in such cases) a very approximately accurate measure of the force acting on the metal. In other cases, when the cones were more blunt, the force in the external field was somewhat greater than the mean force within the metal.

§ 3. Figs. 1 and 2, copied from our earlier paper, show the forms of bobbin originally

Fig. 1.

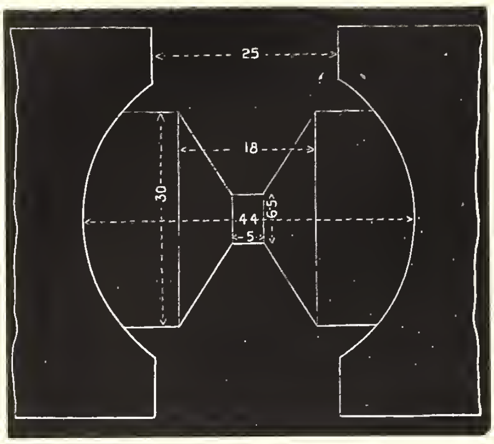
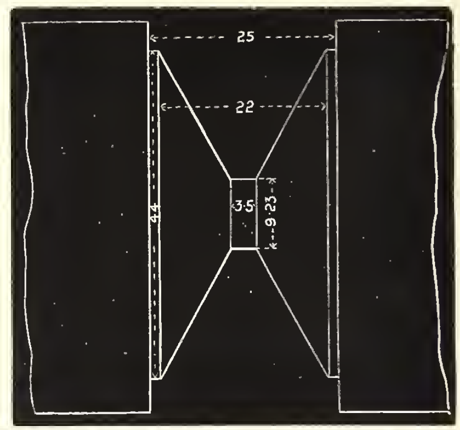


Fig. 2.



used, and the pole-pieces of the magnet by which they were magnetised. With bobbins of the type of fig. 1, the magnetic induction in the neck and the field in the surrounding air were measured by suddenly turning the bobbin round, end for end; in bobbins of the type of fig. 2, the measurements were made by suddenly withdrawing the bobbin from its place between the pole-pieces. In the latter case, the induction measured was the excess of the whole induction ($\mathfrak{B}$) above the residual induction ($\mathfrak{B}_r$) which persisted when the bobbin was drawn out. In iron bobbins the residual magnetisation was found to be sensibly constant from the lowest to the highest value of the inducing field employed in these experiments, but the form of the bobbin made the amount of this residue small. It was measured in bobbins of the type of fig. 1, by comparing the result of turning the bobbin round with the effect of drawing the bobbin out; and, in the first instance, its value in iron bobbins of the type of fig. 2, was estimated to be about the same as in bobbins of the type of fig. 1. In later experiments, when other more retentive metals were being examined, and the residual magnetism consequently formed a more important part of the whole, its value was directly determined by using built-up bobbins which allowed one conical end to be withdrawn; the residual magnetism was then determined after the bobbin had been removed from the field by slipping off (in one operation) one of the conical ends, along with an induction coil which had been wound for this purpose upon a loose ring over the central neck.

Wrought Iron.

§ 4. In the early experiments solid bobbins of the form and dimensions shown in fig. 1, were tested, one of Lowmoor, and another of Swedish wrought iron, with necks 6·5 mm. in diameter and 5 mm. long. The magnetic force was measured in an annular space between the inner and outer induction coils, about 1·3 mm. wide and closely contiguous to the iron neck: for brevity we shall call the magnetic force thus measured in the surrounding air space the "outside field." Tables I. and II. below, which are copied for convenience of reference from our earlier paper, give observed values of the induction $\mathfrak{B}$, and of the outside field for various strengths of current in the coils of the field magnets. They also give values of the quantity ($\mathfrak{B}$ — outside field)/4π, which would be a measure of the intensity of magnetisation $\mathfrak{I}$ if the outside field were fairly representative of the mean magnetic force within the metal of the neck itself (since $\mathfrak{B} = 4\pi\mathfrak{I} + \mathfrak{H}$); and also of the quantity $\mathfrak{B}$/outside field, which on the same proviso would measure the permeability μ. The residual induction $\mathfrak{B}_r$ was 510 in the Lowmoor and 500 in the Swedish sample. The magnetic quantities are stated in c.g.s. units.

Table I.—Lowmoor Wrought Iron.

Current in field magnets, ampères.	Outside field.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\frac{\mathfrak{B}}{\text{outside field}}$.
1·98	3,630	24,700	1680	6·80
4·04	6,680	27,610	1670	4·13
5·81	7,800	28,870	1680	3·70
7·60	8,810	29,350	1630	3·33
11·0	9,500	30,200	1650	3·18
13·5	9,780	30,680	1660	3·14
16·2	10,360	30,830	1630	2·98
21·6	10,840	31,370	1630	2·89
26·8	11,180	31,560	1620	2·82

Table II.—Swedish Wrought Iron.

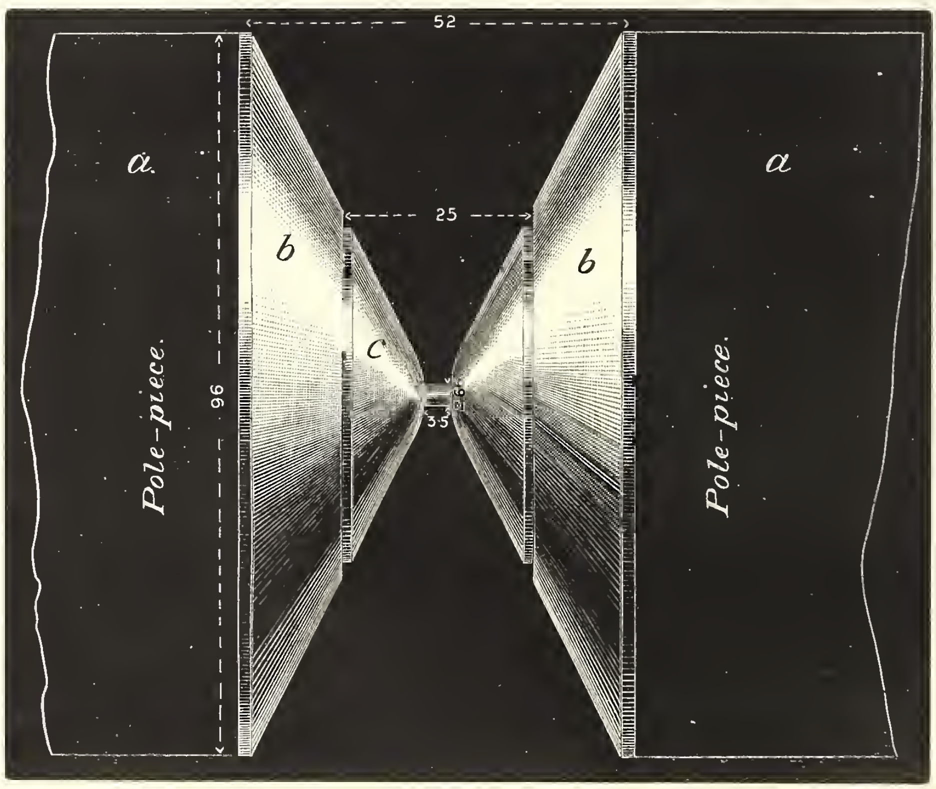
Current in field magnets, ampères.	Outside field.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\frac{\mathfrak{B}}{\text{outside field}}$.
4·08	6,690	27,960	1700	4·18
7·77	8,900	29,730	1660	3·34
10·9	9,510	30,820	1700	3·24
14·2	10,000	31,210	1690	3·12
16·5	10,360	31,630	1700	3·05
18·9	10,810	31,720	1670	2·94
22·9	10,880	32,060	1690	2·95
26·5	11,200	32,360	1690	2·90

Results closely accordant with these were also obtained with samples of the form shown in fig. 2; and, as bobbins with flat ends were most convenient, especially in very strong fields, the subsequent experiments were all made with the flat-ended type.

§ 5. The pole-pieces of the magnet used in the early experiments were only 5·25 cm. square, and it was clear that with a larger magnet a greater concentration of the induction might be produced. Professor TAIT was kind enough to lend us the large magnet of the Edinburgh University Laboratory, and all the subsequent work has been done with it.

The Edinburgh magnet has a pair of vertical limbs about 50 cm. long and 10·7 cm. in diameter. These are united by a horizontal yoke at the bottom, and are furnished on the top with movable pole-pieces in the form of rectangular blocks of soft wrought iron, the cross-section of which is 9·6 cm. square. The limbs are wound along a

Fig. 3.



length of 49 cm. with a number of coils which are grouped in series, making about 1600 turns in all. The currents employed by us ranged up to 40 ampères, and the greatest value of the line-integral of the magnetic force was consequently about 80,000. To allow the old bobbin, of the form of fig. 1, to be effectively used, we furnished the magnet poles with a pair of intermediate conical pieces of soft wrought iron, which virtually formed an extension of the bobbin's ends. Fig. 3 is a full-size sketch of the poles a, a, with the intermediate pieces b, b, and the bobbin c, in place. The figure shows the size to which the neck of the bobbin was finally turned down in experiments which are described below. The dimensions are given in millimetres.

§ 6. In the first instance, however, a Lowmoor bobbin of the dimensions shown in fig. 1, which had been used in the earlier observations, was tested in the Edinburgh magnet without being turned down. The following are the particulars of this experiment and the results :—

Diameter of central neck, 9·23 mm. Length of neck, 3·5 mm.

Diameter to middle of inner induction coil, 9·48 mm.

Diameter to middle of outer induction coil, 10·99 mm.

Inner induction coil, a single layer of twelve turns of silk-covered wire, 0·25 mm. in diameter over the silk.

Outer induction coil, a single layer of seven turns of the same wire.

TABLE III.—Lowmoor Wrought Iron.

Current in field magnets, ampères.	Outside field.	$\mathfrak{B}$.	$\dfrac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\dfrac{\mathfrak{B}}{\text{outside field}}$.
15·0	15,990	36,460	1630	2·28
18·5	18,410	36,970	1480	2·01
28·5	18,380	37,320	1510	2·04
33·0	18,570	37,610	1520	2·03
38·5	18,900	37,990	1520	2·01

A Swedish iron sample of the same shape gave an induction of 37,620 in a field of about the same force, with a current of 38 ampères in the field magnet coils.

§ 7. To push the induction in the Lowmoor sample to a still higher value, the neck of the bobbin was turned down to a diameter of 3·97 mm., the slope of the conical ends being approximately maintained. The inner coil was re-wound with a mean diameter of 4·22 mm., and the outer coil with a mean diameter of 5·7 mm. The following are the results for a magnetising current of about 38 ampères, the mean of several measurements being taken :—

Outside field.	$\mathfrak{B}$.	$\dfrac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\dfrac{\mathfrak{B}}{\text{outside field}}$.
25,620	43,500	1430	1·7

§ 8. A final effort to raise the induction was then made by again turning down the neck of the sample to the size shown in fig. 3. The diameter of the neck was reduced to 2·66 mm., but its length was not reduced in the same proportion: to leave room for a sufficiently long induction coil, a little of the metal of the cones close to the neck was cleared away in the manner shown by the sketch. The section of the neck was now less than $\frac{1}{1500}$ that of the pole-pieces. The bobbin was annealed after being turned down. The inner induction coil was wound in a single layer of ten turns with a mean diameter of 2·93 mm. The outer coil was a single layer of eight turns with a mean diameter of 4·36 mm. With these conditions the induction was forced to the enormous value of 45,350 c.g.s. units, though the outside field between the two coils had a somewhat smaller value than before. This anomaly does not necessarily imply that the measurements were in error, for, as will appear from what follows, the relation of the outside field to the force within the metal is materially affected by the form of the conical ends, and that form had been altered, as has just been said, in the region close to the neck. The excessive smallness of the neck in this case, however, made it more difficult than before to measure the outside field with precision. The following are mean results for the strongest magnetising currents :—

Outside field.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\frac{\mathfrak{B}}{\text{outside field}}$.
24,500	45,350	1660	1·85

§ 9. With regard to the quantity ($\mathfrak{B}$ − outside field)/4π, it will be noticed that, if we exclude the last (somewhat doubtful) case, there is a progressive decrease as the induction rises, within the range covered by these experiments. With a field of 5000 or 6000, the value of this quantity was 1700 in the Swedish sample and 1680 in the Lowmoor sample, and it fell to 1430 as the field was raised to 25,000. This gives great interest to the question, whether the field as measured in the outside space has the same, or nearly the same, value as the magnetic face within the metal; for in that case we should have evidence that the intensity of magnetisation $\mathfrak{I}$ passes a maximum and begins to decrease under the action of very strong fields, and this is a result which WEBER's molecular-current theory of diamagnetism, extended as MAXWELL has extended it to a paramagnetic substance, would lead us to expect.* After a careful examination of this important point, we have concluded, for reasons given below, that the apparent decrease of $\mathfrak{I}$ in the experiments described above is in all probability wholly due to the outside field being greater than the field within the metal, and that, if there is any variation in the real value of $\mathfrak{I}$ in strong fields, it is smaller than our method of experiment can detect.

§ 10. An attempt to investigate the uniformity of the field (in a medial plane along lines radiating from the axis) was made by building up a bobbin over the neck of which four induction coils were wound, one above another, with small annular spaces between. The lowest coil was wound on the iron neck, and the other three on thin

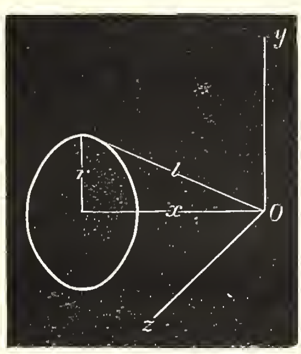
* See MAXWELL's 'Treatise on Electricity and Magnetism,' vol. 2, chap. 22 :—"If it should ever be experimentally proved that the temporary magnetisation of any substance first increases and then diminishes as the magnetising force is continually increased, the evidence of the existence of these molecular currents would, I think, be raised almost to the rank of a demonstration."

brass tubes turned to slip one on another, with flanges at the ends to preserve a definite clearance between them and to keep them concentric. The innermost coil had a mean diameter of 3·2 mm., and the outermost a mean diameter of 7·8 cm. The annular space between them, 2·3 mm. thick, was in this way divided into three parts, in each of which the field was measured. It was found that in these three parts the field decreased progressively with increase of distance from the axis. Thus, in one instance, the field fell from 19,000 in the innermost ring to 17,300 in the outermost ring. It is unnecessary to describe at length these experiments, which were very laborious, and which did not throw any light on the important question of how closely the force within the metal approximated to the force in the air close to the surface of the neck. Moreover, the form of the built-up bobbin used in this case was, as we afterwards recognised, such as to give a much more uniform field than the bobbin formerly used.

Concentration of Magnetic Force by Conical Pole-faces.

§ 11. The magnetic force in the space between the pole-pieces is made up of two parts: (1) the electromagnetic force directly produced by the current in the field-magnet coils; and (2) the force due to free magnetism distributed for the most part over the pole-faces. The first of these was a comparatively small part (less than one-fiftieth) of the whole, and its value must have been sensibly uniform at such small distances from the axis as those with which we are now concerned. In considering the uniformity of the field we need only deal with the force produced by free magnetism distributed over the opposing surfaces of the poles.

Fig. 4.



§ 12. The free magnetism on each pole-face may be treated as made up of a series of co-axial circular rings in planes normal to the axis of magnetisation. Calling M the whole free magnetism of one of these rings (fig. 4) and r its radius, the force F due to it at a point in the axis at a distance x from the plane of the ring is Mx/l^3, where $l = \sqrt{(r^2 + x^2)}$. This force will be a maximum when $dF/dx = 0$, or

$$\frac{1}{l^3} - \frac{3x^2}{l^5} = 0,$$

which gives

$$x = \frac{r}{\sqrt{2}}; \quad \tan\theta = \sqrt{2}; \quad \theta = 54^\circ\ 44'.$$

Hence, a series of co-axial rings will be most advantageously disposed for producing force at a point on the axis if they lie on a cone having its vertex at that point, with a semi-vertical angle of 54° 44′. This conclusion is independent of the distribution of density from ring to ring.

§ 13. The greatest force will be produced when the pole-pieces are themselves saturated, so that $\mathfrak{I}$ reaches its limiting value in all parts of the metal. In that case the distribution of density from ring to ring is uniform. The surface density of free magnetism at any point of a sloping pole-face is $\mathfrak{I}\sin\theta$, where θ is the slope of the face to the axis of magnetisation. The whole quantity in each ring is $\mathfrak{I}$ multiplied by the area of the ring projected upon a plane normal to the axis—a quantity which is independent of the slope of the cone. We have, therefore, the same series of attracting rings to deal with, whatever be the slope of the convergent forces, and whether that slope be uniform or not. Given, then, a certain diameter for the neck of the bobbin to be magnetised, the greatest magnetic force will be produced at the middle of the axis of the neck if we make the expanding ends and pole faces in the form of cones, with a semi-angle of 54° 44′, and with their vertices at the middle of the neck.*

§ 14. This cone of maximum concentrative power is not, however, the form best suited for producing a uniform field. At any point in the axis $d\mathrm{F}/dy$ and $d\mathrm{F}/dz$ are zero, axes of y and z being taken in a plane parallel to the rings, and the condition for a uniform field (uniform, namely, in the neighbourhood of the axis, over a transverse plane) is that $d^2\mathrm{F}/dy^2$ and $d^2\mathrm{F}/dz^2$ shall also be zero. Consider again the attraction of a ring at any point O in the axis. Taking LAPLACE's equation—

$$\frac{d^2\mathrm{V}}{dx^2} + \frac{d^2\mathrm{V}}{dy^2} + \frac{d^2\mathrm{V}}{dz^2} = 0,$$

and differentiating with respect to x, we have

$$\frac{d^2}{dx^2}\frac{d\mathrm{V}}{dx} + \frac{d^2}{dy^2}\frac{d\mathrm{V}}{dx} + \frac{d^2}{dz^2}\frac{d\mathrm{V}}{dx} = 0.$$

By symmetry of the field about the axis of x the second and third terms are equal; hence, writing F for $d\mathrm{V}/dx$,

$$\frac{d^2\mathrm{F}}{dx^2} + \frac{2d^2\mathrm{F}}{dy^2} = 0.$$

* The corresponding proposition for truncated cones, with an air space between them, has lately been stated by Professor STEFAN ('Wien, Akad. Sitzber.,' Feb. 9, 1888; or 'Phil. Mag.,' vol. 25, p. 322).

The condition for a uniform field will therefore be satisfied when $d^2F/dx^2 = 0$; that is, when

$$-\frac{9x}{l^5} + \frac{15x^3}{l^7} = 0,$$

which gives $x = r\sqrt{\frac{3}{2}}$; $\tan\theta = \sqrt{\frac{2}{3}}$; $\theta = 39^\circ\ 14'$. Thus, the condition is satisfied when the pole-faces are cones converging as before upon the middle of the neck, but with a semi-vertical angle of $39^\circ\ 14'$.*

§ 15. With a cone of any semi-angle θ, the surface density of free magnetism being $\mathfrak{I}\sin\theta$, the force at the vertex due to a ring at an axial distance x, of radius r, and of length dl, measured along the slope, is

$$2\pi r\, dl.\ \mathfrak{I}\sin\theta.\ x/l^3, \quad \text{or} \quad 2\pi\mathfrak{I}\sin^2\theta\cos\theta\, dr/r.$$

The whole force at the vertex is

$$2\pi\sin^2\theta\cos\theta\int_a^b \frac{\mathfrak{I}dr}{r},$$

a being the radius of the neck on which the cone converges, and b the radius of the base to which it spreads.

Hence (treating $\mathfrak{I}$ as uniform), with a pair of truncated cones, joined by a neck at the middle of which they have their common vertex, the whole force there is

$$F = 4\pi\mathfrak{I}\sin^2\theta\cos\theta\log_\epsilon\frac{b}{a},$$

which, for convenience of calculation, we shall write

$$F = 28\cdot935\,\mathfrak{I}\sin^2\theta\cos\theta\log_{10}\frac{b}{a}.$$

§ 16. Applying this to the cones of maximum concentrative power (§ 13), in which $\sin\theta = \sqrt{\frac{2}{3}}$ and $\cos\theta = \frac{1}{\sqrt{3}}$,

$$F_{max.} = 11\cdot137\,\mathfrak{I}\log_{10}\frac{b}{a},$$

and the greatest value of the force will be obtained when $\mathfrak{I}$ has the saturation value (of say 1700 c.g.s. units for soft wrought iron), in which case

$$F_{max.} = 18930\log_{10}\frac{b}{a},$$

an expression which measures the greatest possible force which the "isthmus" method of magnetisation can apply at a point in the axis of the bobbin (over and above the small force which is directly produced by the magnet coils). It is

* We are indebted to Mr. A. TANAKADATE for suggesting this calculation of the form of poles suited to give a uniform field.

impracticable to produce quite so great a force as this on account of the difficulty of saturating the magnet poles.

§ 17. With the cones which give the most uniform field, for which $\sin\theta = \sqrt{\frac{2}{5}}$ and $\cos\theta = \sqrt{\frac{3}{5}}$, the value of F is only

$$8{\cdot}965\, \mathfrak{I} \log_{10} \frac{b}{a},$$

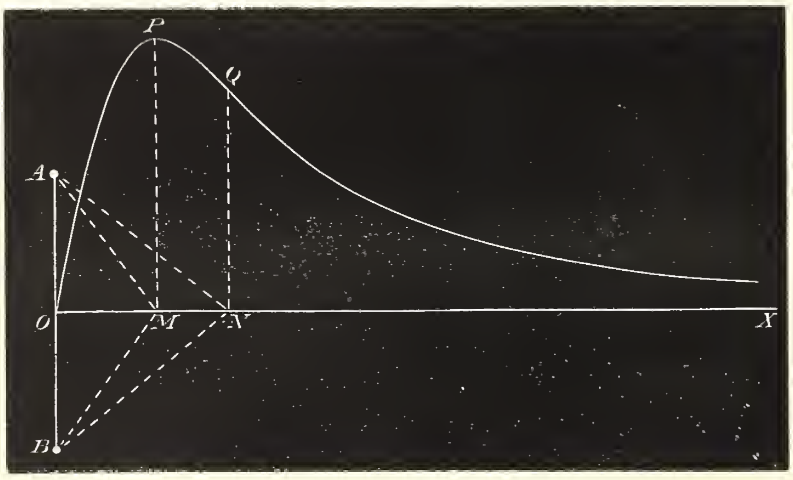
which becomes

$$15240 \log_{10} \frac{b}{a}$$

in the event of the pole-pieces being of soft wrought iron and saturated.

§ 18. The curve, fig. 5, has been drawn to show how the force at the vertex varies when the angle of the cone is altered.

Fig. 5.



The base of the cone being represented by AB, any ordinate PM gives the force when the vertex is at M. In the figure, AMB represents the cone of maximum concentrative power, and ANB represents the cone giving a uniform field in the neighbourhood of the axis, Q being the point of inflection in the curve.

§ 19. In figs. 6 and 7 the same two cases are further illustrated by curves which show the sum of the forces due to two equal and opposite rings (situated on cones with a common vertex) at points along the axis.

By summing up the effects of such pairs, for the whole cone, we may judge how nearly the force is uniform from end to end of the neck of the magnetised bobbin. In a bobbin with cones of semi-angle 39° 14′ the field is sensibly uniform from end to end of the neck, except close to the ends, where it is slightly reduced, and (§ 14) this longitudinal uniformity implies transverse uniformity.

§ 20. When the semi-vertical angle of the cones is greater than 39° 14′, the force at points on the axis has a maximum at the common vertex, and, since $d^2\text{F}/dx^2$ and $d^2\text{F}/dy^2$ have opposite signs (§ 14), the field is stronger at places near the axis than on the axis itself. In a bobbin with a narrow neck this may have the effect of making the field in the closely surrounding air space greater than the mean field within the neck.

§ 21. We may now apply the above conclusions to elucidate the experiments which have been described. The form of bobbin used in them had been chosen, without reference to theory, as one likely to give a strong concentration of magnetic induction, and it chanced to come very near the best form for this purpose. The cones had a semi-angle of 60°, and their vertices were nearly coincident (overlapping very slightly, see fig. 2).

Applying the formula of § 15, we have, for $\theta = 60°$,

$$F = 10{\cdot}85\ \mathfrak{I} \log_{10} \frac{b}{a},$$

which is only two and a half per cent. short of the force attainable by using cones of maximum concentrative power. Moreover, it must be borne in mind that in actual

Fig. 6.

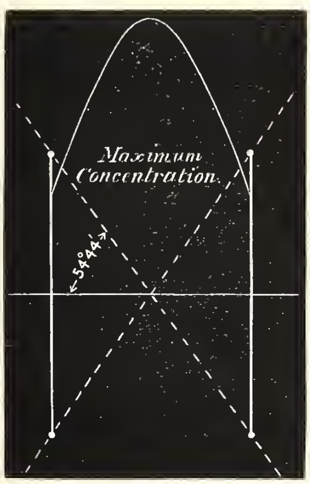
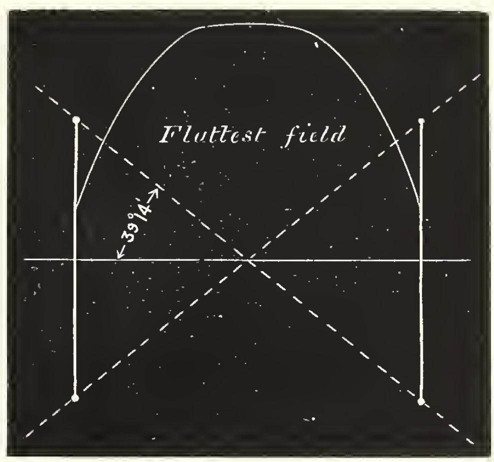


Fig. 7.



use of the isthmus method the strongest induction will be reached when the semi-angle is rather *greater* than 54° 44′, for $\mathfrak{I}$ is itself a function of the angle, decreasing when the angle is decreased, on account of the augmented "resistance" of the whole magnetic circuit. For this reason we probably obtained as much concentration with cones of 60° as we should have obtained with cones of 54° 44′. Further, in the last experiment (§ 8), the neck of the bobbin had been turned down to the smallest size we found it practicable to work with. It is clear, therefore, that no materially higher value of $\mathfrak{B}$ than the value already obtained was possible with the apparatus at our disposal.

§ 22. From the relation of the line integral of magnetic force to the length of iron and mean length of air in the magnetic circuit, we estimate that the mean value of $\mathfrak{I}$

in the magnet cores and pole-pieces cannot have exceeded 1400 when the magnetising current was at its strongest. The distribution of this over the conical pole-faces was not uniform. Close to the neck it reached the saturation value (of, say, 1680 or 1700), being gathered there at the expense of outlying portions. This want of uniformity of $\mathfrak{I}$ in the pole-faces increases the magnetic force in the neck, but when a distribution of $\mathfrak{I}$ is assigned it is easily taken account of in applying the formula of § 15.

§ 23. Taking the experimental case (§ 7), in which the diameter of the neck was $\frac{1}{25}$ of the diameter to which the cones spread, we calculate that the magnetic force at the middle of the axis was probably about 22,500, and at other points of the axis it was less.

Now, the measured value of the field in the air, close to the bobbin's neck, was in this instance 25,620. To produce this force at the axis would require that the value of $\mathfrak{I}$ in the pole-pieces should have been nothing less than 1690 all over, that is to say, it would require that the poles should have been saturated from axis to circumference—a quite impossible supposition. It is clear that the force in the air close to the neck was in this case distinctly greater than the mean force within the neck.

The measured induction $\mathfrak{B}$ within the neck was 43,500. If we accept 22,500 as the mean value of $\mathfrak{H}$ within the neck (remembering that while $\mathfrak{H}$ increases from the axis to the circumference it diminishes from the middle towards the ends), the value of $\mathfrak{I}$ in the neck would be $(43{,}500 - 22{,}500)/4\pi = 1670$, which is, as nearly as may be judged, the same value as was produced by the application of magnetising forces of moderate strength.

§ 24. These considerations establish a very strong presumption that the apparent decrease of $\mathfrak{I}$ in the experiments, that is to say, the observed decrease in the quantity $(\mathfrak{B} - \text{outside field})/4\pi$ under very strong forces is to be explained by the fact that the outside field was stronger than the field within the neck; and that the true value of $\mathfrak{I}$ is sensibly constant throughout the range of magnetic forces examined, namely, from about 4000 to 24,000 c.g.s. units.

Further Experiments on Wrought Iron.

§ 25. To put this matter further to the proof, we continued the experiments with another bobbin, also of Lowmoor wrought iron, the conical ends of which were shaped so as to produce a much more uniform field. The shape which would give the most uniform field was not chosen, for that would have imposed so low an upper limit on the strength of the field that the test would have been rather inconclusive. By way of compromise, a bobbin was turned of the shape and dimensions shown in fig. 8, with cones of semi-angle 45°, as a form which combined high concentrative power with a fair approximation to uniformity of field. The advantage, in respect of uniformity of field, which this bobbin had over the one formerly used may be judged from figs. 9 and 10, which show the longitudinal variation of force due to a pair of rings in the two cases. The length and diameter of the neck were 3·42 mm. The outside field and the induction were measured as before, and it was found that they were decidedly less than in the former instance, chiefly, of course, because of the greater mean thickness of air space now present between the magnet poles, which reduced the

Fig 8.

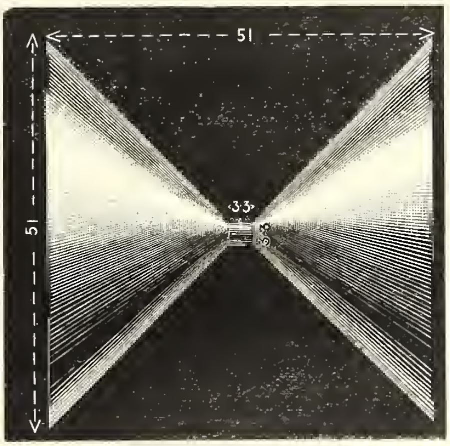


Fig. 9.

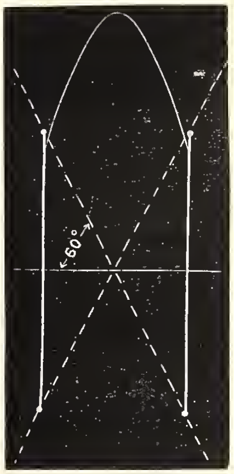
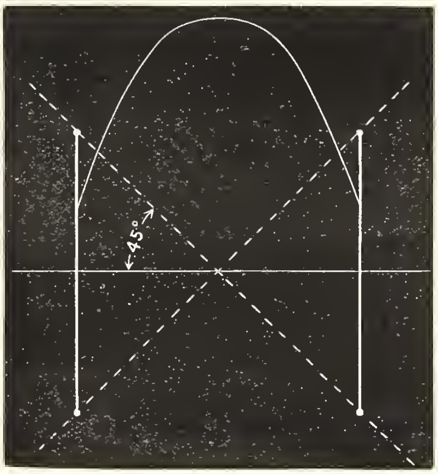


Fig. 10.



mean value of $\mathfrak{I}$ in them. But what is important to our present purpose is to note that now, owing to the greater uniformity of the field, the quantity ($\mathfrak{B}$ — outside field)/4π undergoes no progressive diminution as the force rises. Table IV. gives the results. They confirm the conclusion which was provisionally stated in § 24. Here we may accept the strength of the outside field as closely approximating to the mean force within the neck, so that the first column in the table might have been styled $\mathfrak{H}$, the second last column $\mathfrak{I}$, and the last column μ.

TABLE IV.—Lowmoor Wrought Iron.

Outside field.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\frac{\mathfrak{B}}{\text{outside field}}$.
3,080	24,130	1680	7·83
6,450	28,300	1740	4·39
10,450	32,250	1730	3·09
13,600	35,200	1720	2·59
16,390	36,810	1630	2·25
18,760	39,900	1680	2·13
18,980	40,730	1730	2·15

§ 26. It remains to describe the results of tests of other samples of wrought iron and of cast iron, of various steels, manganese steel, nickel, and cobalt. Messrs JOWITT and SONS, of Sheffield, were kind enough to supply specimens of pure Swedish iron and of steel, with which a number of experiments were made.

The following results were obtained with a bobbin of Swedish iron, described by Messrs. JOWITT as of the " L^{s}Lancash." brand, a good Swedish iron made by the Lancashire hearth process. The form and size of the bobbin are shown in fig. 11.

Fig. 11.

The cones were blunt, and their vertices were at some distance from one another, the general effect being similar to that of sharper cones with a common vertex, giving a fairly uniform, but not excessively strong, field.

TABLE V.—Swedish Iron, " L^{s}Lancash." Brand.

Outside field.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\frac{\mathfrak{B}}{\text{outside field}}$.
1,490	22,650	1680	15·20
3,600	24,650	1680	6·85
6,070	27,130	1680	4·47
8,600	30,270	1720	3·52
18,310	38,960	1640	2·13
19,450	40,820	1700	2·10
19,880	41,140	1700	2·07

It will be noticed that the quantity in the third column, which no doubt approximates very closely to the intensity of magnetisation $\mathfrak{I}$, is practically constant, except for errors of observation. Its mean value is 1685.

§ 27. Another bobbin, described by Messrs. JOWITT as Swedish wrought iron of the celebrated Ⓛ brand, the purest and most expensive iron used in commerce, made by the Walloon process, was also turned to the shape shown in fig. 11, and tested as follows :—

TABLE VI.—Fine Swedish Iron, Ⓛ Brand.

Outside field.	$\mathfrak{B}$.	$\dfrac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\dfrac{\mathfrak{B}}{\text{outside field}}$.
5,310	25,670	1620	4·83
17,680	38,080	1620	2·15
19,240	39,540	1620	2·06

It would seem that the saturation value of $\mathfrak{I}$ is specifically less in this iron than in previous samples.

Cast Iron.

§ 28. Table VII., which is copied from our former paper, gives the results of tests made with a bobbin of cast iron of a form similar to that shown in fig. 1, the magnetisation being measured by reversing the bobbin in the field. The residual induction was also measured by withdrawing the bobbin, and was found to have a nearly constant value of about 400 c.g.s. units.

TABLE VII.—Cast Iron.

Outside field.	$\mathfrak{B}$.	$\dfrac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\dfrac{\mathfrak{B}}{\text{outside field}}$.
3,900	19,660	1250	5·04
6,400	21,930	1240	3·42
7,710	22,830	1200	2·96
8,080	23,520	1230	2·91
9,210	24,580	1220	2·67
9,700	24,900	1210	2·57
10,610	25,600	1190	2·46

§ 29. Table VIII. relates to a later test, made with Professor TAIT's magnet, in which a central spindle of cast iron was used to form the neck of the bobbin, but the conical ends were of wrought iron shrunk on to the ends of the spindle. Fig. 12 shows a section of this bobbin. A similar construction has been adopted in many other cases; the use of the more permeable metal—wrought iron—for the cones has, of course, the advantage of strengthening the induction in the neck. Here the residual magnetism was measured, after the observations were otherwise complete, by slackening one of the cones, so that it might be slipped off the spindle. A suitable induction coil, wound on a ring, was then slipped on; the whole bobbin was magnetised and removed from the field, the loose end and the coil were then slipped off together, and the ballistic effect of this was observed. In these measurements the bobbin was demagnetised by the method of reversals, to get rid of the effect of previous stronger magnetisations. A similar procedure was followed in finding the residual magnetism of steel samples. The residual magnetism is allowed for in the values of $\mathfrak{B}$ given below.

Fig. 12.

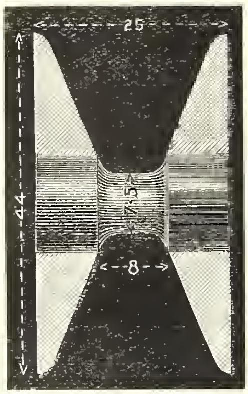


TABLE VIII.—Cast Iron.

Current in field magnets, ampères.	Outside field.	$\mathfrak{B}$.	$\dfrac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\dfrac{\mathfrak{B}}{\text{outside field}}$.
1·57	4,560	20,070	1230	4·40
3·62	9,120	24,630	1230	2·70
5·95	11,770	27,680	1270	2·35
8·1	13,460	28,710	1210	2·13
14·2	14,690	30,160	1230	2·05
23·0	16,200	30,920	1170	1·91
40·0	16,900	31,760	1180	1·88

These two experiments agree in assigning about 1240 as the saturation value of $\mathfrak{I}$ in this cast iron; and the apparent diminution in fields of the greatest strength is, of course, to be set down to the cause which has been fully explained in connection with wrought iron—an excess of the "outside field" over the mean force within the metal, owing to the cones being too blunt to give a very uniform field.

Steel.

§ 30. Of the following experiments, Nos. 1 to 5 were made with samples of steel supplied by Messrs. Jowitt, containing various percentages of carbon. The sample was built, in each case, into a bobbin of the form shown in fig. 13, with wrought iron

Fig. 13.

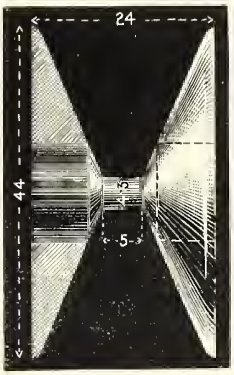
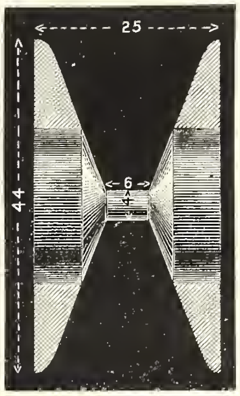


Fig. 14.



cones. No. 6 was made with a specimen of Whitworth's fluid compressed steel, built with wrought iron cones into the bobbin of fig. 14. Observations were made in the strongest fields only.

Table IX.—Steel of Various Qualities.

Description of steel.	Outside field.	$\mathfrak{B}$.	$\dfrac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\dfrac{\mathfrak{B}}{\text{outside field}}$.
1. Bessemer steel, containing about 0·4 per cent. of carbon	17,610	39,880	1770	2·27
2. Siemens-Martin steel, containing about 0·5 per cent. of carbon	18,000	38,860	1660	2·16
3. Crucible steel for making chisels, containing about 0·6 per cent. of carbon	19,470	38,010	1480	1·95
4. Finer quality of crucible steel for chisels, containing about 0·8 per cent. of carbon	18,330	38,190	1580	2·08
5. Crucible steel, containing about 1 per cent. of carbon	19,620	37,690	1440	1·92
6. Whitworth fluid compressed steel	18,700	38,710	1590	2·07

§ 31.—The following tests were made with a piece of Vickers' tool steel, built with wrought iron cones into the bobbin shown in fig. 15. In this case the magnetising field must have been sufficiently uniform to make the first column in the table represent $\mathfrak{H}$, the last μ, and the second last column $\mathfrak{I}$ very nearly. This steel had great coercive force; the residual magnetic induction (entered in the table under $\mathfrak{B}_r$) was scarcely constant, even in fields of over 10,000, and $\mathfrak{I}$ appeared to be still increasing in the strongest field to which the experiment extended.

Fig. 15.

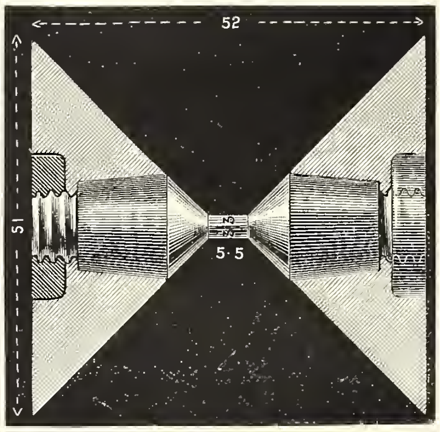


TABLE X.—VICKERS' Tool Steel.

Outside field. ($\mathfrak{H}$.)	$\mathfrak{B}_r$.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$. ($\mathfrak{I}$.)	$\frac{\mathfrak{B}}{\text{outside field}}$. ($\mu$.)
6,210	7350	25,480	1530	4·10
9,970	7670	29,650	1570	2·97
12,170	8000	31,620	1550	2·60
14,660	8030	34,550	1580	2·36
15,530	8030	35,820	1610	2·31

Taken together, the experiments on steel render it probable that there are specific differences in the saturation values of $\mathfrak{I}$ for different steels, smaller values being found in high- than in low-carbon steels. This is to be expected, in view of the decidedly low saturation value of $\mathfrak{I}$ found in cast iron.

Manganese Steel.

§ 32.—At the suggestion of Dr. J. HOPKINSON, we have examined the action of strong magnetic forces upon the remarkable alloy of iron and manganese lately introduced by Mr. R. A. HADFIELD, of Sheffield, which has many peculiar mechanical and electrical properties.* The experiments of HOPKINSON,† BOTTOMLEY,‡ and BARRETT § have shown that this steel is almost wholly destitute of magnetic suscepti-

* See Mr. HADFIELD's paper on Manganese Steel, 'Inst. Civ. Engin. Proc.,' February 28, 1888.

† "Magnetisation of Iron," 'Phil. Trans.,' 1885, p. 462.

‡ 'Report of the British Association for 1885,' p. 903.

§ 'Roy. Dublin Soc. Proc.,' vol. 5, 1886, p. 360.

bility. HOPKINSON found that a force $\mathfrak{H}$ of 244 produced an induction $\mathfrak{B}$ of 310, which makes the permeability only 1·27. Mr. HADFIELD was kind enough to supply us with a bar which contained about 12 per cent. of manganese and 0·8 per cent. of carbon. The metal is excessively hard, but, by raising the bar to a bright red heat and quenching it in water, it was softened sufficiently to allow pieces to be turned, with considerable difficulty, into forms suitable for these experiments.

One piece of the bar was turned into a solid bobbin, of the size and shape shown in fig. 16, and with that the following observations were made :—

Fig. 16.

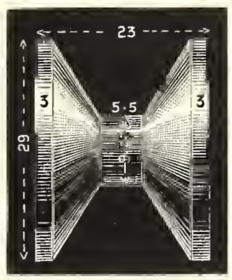


TABLE XI.—HADFIELD'S Manganese Steel.

Outside field.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\frac{\mathfrak{B}}{\text{outside field}}$.
2000	2770	61	1·38
3250	4560	104	1·40
3720	5090	109	1·37
4100	6010	152	1·47
5200	7320	185	1·41

Fig. 17.

§ 33. To push the induction to higher values, another bobbin was built up (fig. 17), with a central spindle cut from the bar of manganese steel, and with cones of wrought iron. The following measurements were made with it :—

TABLE XII.—HADFIELD'S Manganese Steel.

Outside field.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\frac{\mathfrak{B}}{\text{outside field}}$.
1930	2,620	55	1·36
2380	3,430	84	1·44
3350	4,400	84	1·31
5920	7,310	111	1·24
6620	8,970	187	1·35
7890	10,290	191	1·30
8390	11,690	263	1·39
9810	14,790	396	1·51

§ 34. The figures in the last two columns of Tables XI. and XII. show as much regularity as can be expected, when it is borne in mind that they depend upon the small differences between two large quantities which had to be separately measured. The two sets of results agree well. They show that the permeability of manganese steel is, as nearly as may be judged, constant from fields of 2000 to 10,000 units, with a value approximating to 1·4 in this sample. It follows from this that, notwithstanding the excessive resistance which this material opposes to being magnetised, a respectably high intensity of magnetisation will be produced by the application of a sufficiently strong force. In the second experiment $\mathfrak{I}$ was raised to nearly 400. It is very remarkable that scarcely any of this magnetisation remains when the force is withdrawn. One might have expected that a material which resists magnetisation so strongly would possess much coercive force. In fact, however, the residual magnetism (which was determined in the second sample in the usual way, by slipping off one of the iron cones along with an induction coil) was so small that it scarcely admitted of measurement by the apparatus which served to measure the induced magnetism. After applying the strongest field the value of the residual induction $\mathfrak{B}_r$ was only about 30. It is well known that with ordinary iron and steel the magnetisation wholly, or almost wholly, disappears when the magnetising force is withdrawn, provided the force is less than a certain amount. This elastic stage in the process of magnetisation, the limits of which are exceedingly narrow in soft wrought iron, but somewhat wider in hard iron, common steels, and nickel, seems to extend, in manganese steel, up to the strongest force we have been able to apply.

Nickel.

§ 35. Two specimens of nickel, supplied by Messrs. JOHNSON and MATTHEY, have been tested. The first was cut from a bar previously used in examining the permeability of nickel when in a state of compression under the action of ordinarily weak magnetising forces.* It contained about 0·75 per cent. of iron. The bar was annealed and was built into a bobbin with wrought iron cones, and the neck was turned down to a diameter of 4 mm. (fig. 18).

Fig. 18.

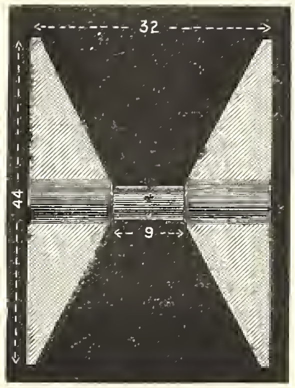


TABLE XIII.—Annealed Nickel (0·75 per cent. of Iron).

Outside field.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\frac{\mathfrak{B}}{\text{outside field}}$.
3,450	9,850	510	2·86
6,420	12,860	510	2·00
8,630	15,260	530	1·77
11,220	17,200	480	1·53
12,780	19,310	520	1·51
13,020	19,800	540	1·52

Here $\mathfrak{I}$ is sensibly constant, with a mean value of 515, of which about 160 was residual.

§ 36. The second sample was a hard-drawn wire, not annealed before testing, which contained less iron than the other (0·56 per cent.). Perhaps for this reason, the value of $\mathfrak{I}$ in it was less. The nickel formed the central spindle of a bobbin with wrought iron cones, and with a neck 5·7 mm. in diameter.

TABLE XIV.—Hard-drawn Nickel (0·56 per cent. of Iron)

Outside field.	$\mathfrak{B}$.	$\frac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\frac{\mathfrak{B}}{\text{outside field}}$.
2,220	7,100	390	3·20
4,440	9,210	380	2·09
7,940	12,970	400	1·63
14,660	19,640	400	1·34
16,000	21,070	400	1·32

* EWING, "Magnetic Qualities of Nickel, Supplementary Paper," 'Phil. Trans.,' A, 1888.

Cobalt.

§ 37. Lastly, a piece of cobalt was tested which was cut from a cast bar supplied by Messrs. JOHNSON and MATTHEY, and turned to form the centre of a bobbin with wrought iron cones (fig. 19), and with a neck 4·48 mm. in diameter. It was found to contain 1·66 per cent. of iron.

Fig. 19.

TABLE XV.—Cobalt.

Outside field.	$\mathfrak{B}$.	$\dfrac{\mathfrak{B} - \text{outside field}}{4\pi}$.	$\dfrac{\mathfrak{B}}{\text{outside field}}$.
1,350	16,000	1260	12·73
4,040	18,870	1280	4·98
8,930	23,890	1290	2·82
14,990	30,210	1310	2·10

It appears from this that the saturation value of $\mathfrak{I}$ for this specimen of cobalt is about 1300, or a little greater than the value we have found for cast iron. In the second, third, and fourth of these observations the residual magnetism was sensibly constant ($\mathfrak{B}_r = 1260$) ; in the first it was a little less.

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§ 38. We may conclude that under sufficiently strong magnetising forces the intensity of magnetisation ($\mathfrak{I}$) reaches a constant, or very nearly constant, value in wrought iron, cast iron, most steels, nickel, and cobalt. The magnetic force at which $\mathfrak{I}$ may be said to become practically constant is less than 2000 c.g.s. units for wrought iron and nickel, and less than 4000 for cast iron and cobalt. In stronger fields, the relation of magnetic induction to magnetic force may be expressed by the formula

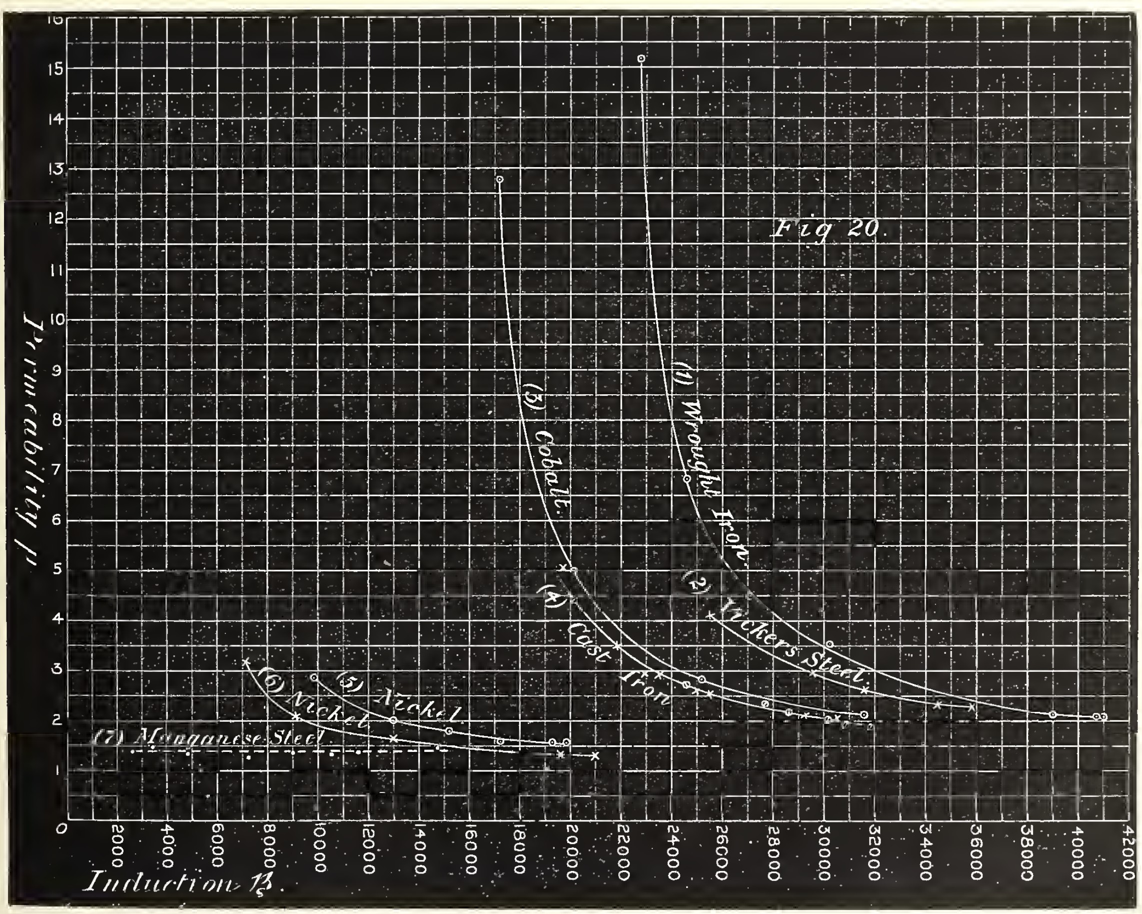
$$\mathfrak{B} = \mathfrak{H} + \text{constant}.$$

For the particular specimens we have tested, the value of this constant ($4\pi\mathfrak{I}$) is about

21,360 in wrought iron, 15,580 in cast iron, 5030 and 6470 in nickel, and 16,300 in cobalt.

The experiments give a definite meaning to the term "saturation," as applied to magnetic state. When magnetisation is measured by the induction $\mathfrak{B}$, the term saturation is inapplicable; there is apparently no limit to the value to which the induction may be raised. But, when we measure magnetisation by the intensity of magnetism $\mathfrak{I}$, we are confronted with a definite limit—a true saturation value, which is reached or closely approached by the application of a comparatively moderate magnetic force. There is nothing to show that the approach to this limit is not asymptotic; but in wrought iron it is practically reached before the magnetic force rises to 2000 c.g.s., and after that a ten-fold increase in the force produces no material change in the intensity of magnetism.

Fig. 20.



§ 39.—The results are further summarised in fig. 20, in which Rowland's curve, showing the relation of the permeability μ to the induction $\mathfrak{B}$, is drawn from the data supplied by the experiments on—

(1) Swedish wrought iron (Table V.).
(2) VICKERS' tool steel (Table X.).
(3) Cobalt (Table XV.).
(4) Cast iron (Tables VII. and VIII. The points taken from Table VII. are marked thus, ×, and those from Table VIII. thus, ⦶.).
(5) Annealed nickel with 0·75 per cent. of iron (Table XIII.).
(6) Hard-drawn nickel with 0·56 per cent. of iron (Table XIV.).
(7) HADFIELD's manganese steel (Table XII.).

If the magnetic force $\mathfrak{H}$, instead of the induction $\mathfrak{B}$, had been taken as abscissa, the curves (with the exception of those relating to VICKERS' steel and manganese steel) would have sensibly lain upon rectangular hyperbolas with $\mathfrak{H} = 0$ and $\mu = 1$ for asymptotes.

VIII. *Some Observations on the Amount of Light Reflected and Transmitted by Certain Kinds of Glass.*

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[Plate 8.]

Introduction.

Although for both theoretical and practical purposes it is important to know the amount of light reflected from the surface of glass, and the loss which light suffers in passing through glass, but few accurate experiments appear to have been made on this subject.

Dr. Robinson, in the report on the Melbourne telescope ('Phil. Trans.,' 1869, p. 127), gives an account of experiments made by Lord Rosse, Mr. Grubb, and himself to determine the amount of light transmitted by telescopic object glasses, and through various kinds of flint and crown glass; he states that Lord Rosse's and Mr. Grubb's experiments were made with a Bunsen's photometer, and his own with a Zöllner's photometer. Dr. Robinson assumed the truth of Fresnel's formulæ, and then calculated the values of the extinction coefficients ϵ^{-nt} from the expression $n = (\log \rho^2 - \log \mathrm{I})/(t \times \text{modulus})$, where I is the intensity of the emergent light, t the thickness of the glass, and ρ^2 the coefficient giving the amount which escapes reflection at the two surfaces.

Mr. Rood published in the 'American Journal of Science' for 1870 (vol. 50, p. 1) an account of some observations he had made, with a modification of Bunsen's photometer, of the loss which light suffers in passing through crown glass. He states that the formulæ for reflection were originally given by Young, and obtained subsequently by Poisson and Fresnel, but believes that no accurate experiments have ever been made to test their truth. He used two plates of glass, ·15 mm. and 1·677 mm. thick, and assumed that with such thin plates the loss of light was practically entirely due to reflection. He found that with one plate, of which the index was 1·5236, and which therefore should have transmitted 91·736 per cent. of the incident light, 91·440 per cent. actually passed through; and with the other, of which the index was 1·5225, 91·155 was transmitted, instead of 91·763.

VOGEL published in the 'Berliner Monatsberichte' for 1877 (p. 138) some determinations of the absorption of crown, and of two kinds of flint, glass for different parts of the spectrum, but states that, as the three glasses were not equally well polished, no conclusion can be drawn as to their relative merits.

M. ALLARD ("L'Intensité des Phares," 'Annales des Ponts et Chaussées,' 1876, p. 31) states that the absorption of light by the glass which it passes through is given "par une formule exponentielle, mais on peut sans grande erreur la supposer proportionelle à l'épaisseur et l'évaluer à raison de 0·03 par centimètre de verre traversé," but does not give an account of any experiments made to determine the amount absorbed.

Since the experiments described in this paper were commenced Lord RAYLEIGH has published ('Roy. Soc. Proc.,' vol. 41, p. 275) an account of some determinations he has made of the intensity of the light reflected from glass at a nearly perpendicular incidence; he came to the conclusion that "recently polished glass surfaces have a reflecting power differing not more than 1 or 2 per-cent. from that given by FRESNEL'S formula; but that after some months or years the reflection may fall off from 10 to 30 per cent., and that without any apparent tarnish."

The experiments of which I have the honour of presenting an account to the Royal Society were commenced in order to determine, if possible, the amount of light lost by transmission through glass, without assuming the truth of the formulæ for reflection, and also to determine experimentally the amount reflected from the surface of the glass.

It was thought that one way in which this might be effected would be by taking plates of the same kind of glass, but of different thicknesses, and observing the amount of light which passed through; the reflection from the first surface would be the same in all cases, whilst that from the second would only differ slightly in amount, as, owing to the increased absorption of the thicker pieces, less light would reach the second surface; but, the absorption being small, and photometric methods not very exact, it was thought that this would hardly produce any sensible difference in the results.

It was also hoped that it might be found possible to determine the reflected light directly, since, as Lord RAYLEIGH has pointed out (*loc. cit.*) with reference to Professor ROOD'S experiments, any error in the measured amount of the light transmitted would give rise to a very much greater error in the estimated amount of the reflected light. This was subsequently accomplished by measuring the relative intensities of the illumination produced by two Argand gas flames, when the light from both fell directly on the photometric surfaces, and when the light from one fell directly, whilst that from the other reached the photometer after reflection from the surface of a piece of glass.

Experiments were also made to ascertain whether repolishing altered in any way the reflective power of the glass; and the polarising angles before, and after, repolishing were also determined.

The observations were made with five plates of Messrs. CHANCE'S "lighthouse" glass, for which the author is indebted to Mr. KENWARD, the plates being 6·5 mm., 11·5 mm., 15 mm., 18·5 mm., and 24·3 mm. thick respectively, and with different thicknesses of Messrs. FIELD'S "ordinary dense flint." A block of this glass was procured from Mr. HILGER, by whom a slice 7 mm. thick was cut off and polished, and then the remainder of the piece formed into a rectangular block measuring 91·3 mm. × 69·5 mm. × 49 mm., and the six faces carefully polished.

Some measurements were also made with a piece of ordinary plate glass 6 mm. thick.

The ordinary plate glass was green when seen edgewise, Messrs. CHANCE'S glass was slightly green when viewed in the same manner, and the flint glass was distinctly yellow by daylight.

Mr. KENWARD states that "the lighthouse glass is of a special mixture, varying slightly from time to time; it is of the nature of hard crown glass." The average refractive index for the sodium line of Messrs. CHANCE'S hard crown is stated by them to be 1·5172, and of their soft crown 1·5146; and in a letter which accompanied the glass its index was said to be "about 1·51 or 1·52."

The refractive indices for the sodium line of the crown glass, the flint glass, and the plate glass used in these experiments were determined in the ordinary way with small prisms made of each kind of glass by Mr. HILGER; the values found were :—

Crown glass	1·5145,
Flint glass	1·6330,
Plate glass	1·5274.

The indices of the crown and plate glass were also determined with the wedges of these two kinds of glass used for the reflection experiments; that for the crown was found to be 1·5137, and of the flint 1·6385; the refracting angles of these two wedges being only 9° 39′ 45″ and 9° 51′ 19″, whilst those of the small prisms were 59° 45′ 19″ and 59° 45′ 17″, it seemed probable that the values obtained with the latter were the most accurate, and they were therefore taken as the true values of the indices.

The account of the experiments is given in Part I., and the results deduced from them in Part II., the account of the experiments being divided into six sections :— 1. Amount of light transmitted. 2. Amount of light reflected at a nearly perpendicular incidence. 3. Amount of light reflected at a nearly perpendicular incidence after repolishing. 4. Amount of light reflected at various incidences between 0° and 90° by the crown glass before and after repolishing. 5. Amount of light transmitted after repolishing. 6. Values of the polarising angles before and after repolishing.

Part I.

Section I.—Determination of the Amount of Light Transmitted.

A photometric arrangement similar to the one described in Pickering's 'Physical Manipulations,' vol. 1, p. 132, was used for the greater number of these determinations (see Plate 8, fig. 1). Two similar pieces of looking glass 125 mm. × 125 mm. were fixed vertically at the ends of a horizontal board rather more than 2 metres long and 27 cm. wide, the mirrors being 2 metres apart; a small Argand gas burner, giving a flame 15 mm. in diameter, was fixed opposite the middle point of the line joining the centres of the mirrors, and at a horizontal distance of 20 cm. from the line; the mirrors were so adjusted that they reflected the light of the lamp towards each other.

A block of wood, resting on four small metal rollers and guided by two strips of wood fixed 7·8 cm. apart on the upper surface of the horizontal board, carried the photometer; an index was fixed to the wood block, and a scale, divided into millimetres, to the board, the zero being at one of the mirrors.

The photometer of which a description was given in 'Roy. Soc. Proc.,' vol. 35, p. 27, and 'Phil. Mag.,' series 5, vol. 15, p. 423, was used (Plate 8, fig. 1a); it consisted essentially of two pieces of white paper so placed that, whilst each was illuminated by one only of the two lights to be compared, both were visible to the observer. Three pieces of wood were screwed to the block, and between these the photometer was placed; this arrangement, whilst permitting the photometer to be reversed, so that the light from each of the two mirrors could be made to fall first on the one and then on the other paper, ensured its always being replaced in the same position.

The whole arrangement was optically equivalent to two exactly similar sources of light which always retained the same relative intensity, and were at a distance apart of a little more than twice the distance between the two mirrors.

A wooden screen was placed between the lamp and the observer, and, in order to cut off stray light reflected from the horizontal board, screens, with apertures in them rather larger than the apertures in the photometer box, were placed on either side of it, and between it and the mirrors; the edges of the apertures in the screens being formed of metal filed to a feather edge, and then, together with all the wood work, painted a dead black.

Assuming that the two sides of the Argand flame were equally bright, that the reflective powers of the two looking glasses were equal, and that there was no stray light, or at least that there was an equal amount from either side, then at a point midway between the two mirrors the intensities of the light reflected from the mirrors would necessarily have been equal, and, in order to determine the amount of light which passed through any transparent substance, it would merely have been necessary to interpose the substance in question between one of the mirrors and the photometer, and determine, by means of the photometer, the new position of equality.

But, as such assumptions might be erroneous, the experiments were actually made in the following manner :—First, the glass plate was fixed on one side of the photometer, whose position was altered until both papers appeared equally bright; six readings having been thus made, the glass was then placed on the other side, and six more readings made. The glass was then replaced in its original position, and six more readings made, and so on. After thirty-six readings the photometer was reversed, and another set of thirty-six readings made in the same manner. In making the readings, the photometer was first placed too much to the right, and then moved to the left till the point at which the illumination was equal was reached; it was then pushed to the left, and gradually moved back to the right, till the papers appeared equally bright, being thus brought to the position of equality alternately from one side and from the other.

In order to prevent the possibility of any light reflected from the edges of the plates reaching the photometer, the plates were placed either close to it or close to one of the screens. For two sets of readings the glass was placed against the opening in the box enclosing the photometer, and for one set against the opening in the screen, except in the case of the thick piece of flint glass, which was too large and heavy to be carried by the block on which the photometer rested, and which was therefore always placed against one of the screens. The position of the glass made no difference in the measurements.

The percentage transmitted of light falling upon the glass was calculated in the following manner:—Half the difference between the means of each set of six observations with the plate on either side of the photometer was taken; half the distance between the two mirrors, plus or minus this quantity, together with the distance between the lamp and the mirrors, gave the two distances from the lamp at which there was equality of illumination. The mirrors being 200 cm. apart and the lamp at a horizontal distance of 20 cm. from the middle of the line which joined their centres, the half difference was added to or subtracted from 202.*

Calling these distances a and b, a being the lesser, a correction x had to be made in the value of a for the optical shortening of the path of the light due to its passage through the glass; this was calculated by the ordinary formula $x = e(1 - 1/n)$, where e is the thickness of the plate, and n its refractive index, the distance of the lamp from the glass being sufficiently great to allow the formula for perpendicular incidence to be used without introducing any sensible error.

The percentage amount of light transmitted was given by the expression $100(a - x)^2/b^2$.

* Strictly speaking, the geometrical, and not the arithmetical, mean of the readings should have been taken, as the intensity of light varies inversely as the square of the distance. Calling the two sources of light, to which the lamp and mirrors were equivalent, m and n, and the distance between them x, the

The surfaces of the glass plates were always cleaned with a wash-leather immediately before the plates were used, but, in order to ascertain whether this was sufficient, a number of readings were made with a piece of plate glass treated in this way, and then the plate was cleaned with strong nitric acid, washed with water, with alcohol, and again with water, dried with a clean cloth, and finally rubbed with a wash-leather. The means of the twelve readings made immediately before, and immediately after, the plate had been so treated were identical.

Table I. gives the readings made with the 24·3 mm. plate of crown glass. The observations made with the other pieces of glass were about as concordant.

two positions of the photometer in which there is equality x_1 and x_2, and the coefficient of transparency for the particular piece of glass k, then

$$\frac{mk}{x_1^2} = \frac{n}{(x - x_1)^2}, \qquad \text{and} \qquad \frac{nk}{(x - x_2)^2} = \frac{m}{x_2^2},$$

then

$$k = \frac{nx_1^2}{m(x - x_1)^2}, \qquad \text{and } k = \frac{m(x - x_2)^2}{nx_2^2},$$

$$k^2 = \frac{x_1^2 (x - x_2)^2}{x_2^2 (x - x_1)^2},$$

$$k = \frac{x_1 (x - x_2)}{x_2 (x - x_1)}.$$

The difference, however, between the values of k obtained by taking the quotient of the squares of the geometrical means of the readings, and by taking the quotient of the squares of the arithmetical means,

$$\left(\frac{\frac{x_1 + (x - x_2)}{2}}{\frac{x_2 + (x - x_1)}{2}}\right)^2,$$

was very small, and, as the readings themselves could not be made with any very great degree of accuracy, the simpler process was used for calculating out the results.

Table I.

A.					
cm.	cm.	cm.	cm.	cm.	cm.
97·1	110·8	96·1	110·4	96·1	109·6
97·3	111·1	96·8	110·2	96·8	110·0
96·0	110·4	96·7	109·6	96·5	109·3
96·3	111·2	96·4	109·8	96·5	110·6
96·3	109·8	95·9	109·3	95·8	109·5
96·7	110·7	96·5	110·0	95·8	109·7
Mean 96·6	110·7	96·4	109·9	96·2	109·8
B.					
94·3	106·3	93·5	107·2	94·6	107·1
94·2	107·3	94·2	106·4	94·5	106·7
93·6	106·5	93·7	106·5	94·0	107·1
94·2	106·8	93·6	106·9	94·3	107·5
94·0	106·8	94·1	106·8	94·4	106·8
94·4	106·9	94·8	106·1	94·4	107·1
Mean 94·1	106·7	94·0	106·3	94·4	107·0

Table II. gives the mean readings (1st column), their half differences (2nd), the values of $a - x$ and b (3rd and 4th), and the percentage amount of light transmitted (5th), A and B being the readings made in the two positions of the photometer.

TABLE II.

Crown Glass. 6·5 mm. plate. $x = 2$ mm.

Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B
A.					
cm.	cm.				
97·7–106·1	4·2	197·6	206·2	91·84	
97·1–106·3	4·6	197·2	206·6	91·11	
96·7–105·4	4·3	197·5	206·3	91·65	
				91·53	
B.					
94·7–103·4	4·3	197·5	206·3	91·65	
95·1–104·0	4·4	197·4	206·4	91·47	
95·1–104·1	4·5	197·3	206·5	91·29	
				91·47	91·50

Crown Glass. 11·5 mm. plate. $x = 4$ mm.

Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B.
A.					
cm.	cm.				
96·2–106·3	5·0	196·6	207·0	90·20	
96·3–106·4	5·0	196·6	207·0	90·20	
96·3–106·4	5·0	196·6	207·0	90·20	
96·4–106·5	5·0	196·6	207·0	90·20	
				90·20	
B.					
94·1–104·9	5·4	196·2	207·4	89·49	
94·4–104·6	5·1	196·5	207·1	90·02	
94·4–105·1	5·3	196·3	207·3	89·67	
95·3–104·9	4·8	196·8	206·8	90·56	
				89·93	90·07

Crown Glass. 15 mm. plate. $x = 5$ mm.

Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B.
A.					
cm.	cm.				
95·5–107·1	5·8	195·7	207·8	88·69	
95·8–106·9	5·5	196·0	207·5	89·22	
95·6–106·6	5·5	196·0	207·5	89·22	
				89·04	
B.					
94·3–104·9	5·3	196·2	207·3	89·58	
94·0–105·2	5·6	195·9	207·6	89·04	
94·2–105·4	5·6	195·9	207·6	89·04	
				89·22	89·13

Crown Glass. 18·5 mm. plate. $x = 6$ mm.

Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B.
A.					
cm.	cm.				
97·1–108·7	5·8	195·6	207·8	88·60	
96·7–109·0	6·1	195·3	208·1	88·08	
96·2–108·1	5·9	195·5	207·9	88·43	
				88·37	
B.					
94·7–105·8	5·5	195·9	207·5	89·13	
94·6–106·4	5·9	195·5	207·9	88·43	
94·6–106·4	5·9	195·5	207·9	88·43	
				88·66	88·51

Crown Glass. 24·3 mm. plate. $x = 8$ mm.

Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B.
A.					
cm.	cm.				
96·6–110·7	7·0	194·2	209·0	86·34	
96·4–109·9	6·7	194·5	208·7	86·86	
96·2–109·8	6·8	194·4	208·8	86·68	
				86·63	
B.					
94·1–106·7	6·3	194·9	208·3	87·55	
94·0–106·3	6·1	195·1	208·1	87·90	
94·4–107·0	6·3	194·9	208·3	87·55	
				87·70	87·16

Flint Glass. 7 mm. plate. $x = 3$ mm.

Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B.
A.					
cm.	cm.				
95·8–108·4	6·3	195·4	208·3	88·00	
96·2–108·1	6·0	195·7	208·0	88·52	
96·1–107·1	5·5	196·2	207·5	89·40	
96·5–108·1	5·8	195·9	207·8	88·87	
				88·70	
B.					
93·5–105·0	5·7	196·0	207·7	89·05	
94·0–105·4	5·7	196·0	207·7	89·05	
94·2–105·5	5·7	196·0	207·7	89·05	
94·3–106·1	5·9	195·8	207·9	88·70	
				88·94	88·83

Flint Glass. 49 mm. thick. $x = 19$ mm.

Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B.
A.					
cm.	cm.				
94·5–109·2	7·3	192·8	209·3	84·85	
94·5–109·0	7·2	192·9	209·2	85 02	
94·9–109·0	7·0	193·1	209·0	85·36	
95·3–109·2	6·9	193·2	208·9	85·53	
				85·19	
B.					
93·1–106·8	6·8	193·3	208·8	85·70	
93·3–107·4	7·0	193·1	209·0	85·36	
93·1–107·0	6·9	193·2	208·9	85·53	
93·9–107·3	6·7	193·4	208·7	85·88	
				85·62	85·40

Flint Glass. 69·5 mm. thick. $x = 27$ mm.

Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B.
A.					
cm.	cm.				
93·1–110·3	8·6	190·7	210·6	81·99	
93·5–110·3	8·4	190·9	210·4	82·32	
93·6–110·4	8·4	190·9	210·4	82·32	
				82·21	
B.					
92·5–108·9	8·2	191·1	210·2	82·65	
92·9–108·9	8·0	191·3	210·0	82·98	
93·1–109 0	7·9	191 4	209·9	83·15	
				82·93	82·57

Flint Glass. 91·3 mm. thick. $x = 35$ mm.

A.					
Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B.
cm.	cm.				
92·7–110·6	8·9	189·6	210·9	80·82	
92·9–110·9	9·0	189·5	211·0	80·66	
93·1–110·7	8·8	189·7	210·8	80·98	
				80·82	
B.					
91·6–110·1	9·2	189·3	211·2	80·34	
92·0–110·2	9·1	189·4	211·1	80·50	
92·3–109·8	8·7	189·8	210·7	81·15	
				80·66	80·74

Three plates of 6 mm. Crown Glass. Cemented together. $x = 6$ mm.

A.				
Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.
cm.	cm.			
96·2–108·6	6·2	195·2	208·2	87·90
96·3–108·4	6·0	195·4	208·0	88·25
96·3–108·6	6·1	195·3	208·1	88·08
				88·08

Ordinary Plate Glass. 6 mm. thick.

Readings.	Half differences.	$a - x$.	b.	Percentage of light transmitted.	Percentage of light transmitted. Mean of A and B.
A.					
cm.	cm.				
94·9–108·4	6·7	195·1	208·7	87·39	
94·9–108·4	6·7	195·1	208·7	87·39	
94·9–108·6	6·8	195·0	208·8	87·22	
				87·33	
B.					
94·4–107·3	6·4	195·4	208·4	87·91	
94·4–107·0	6·3	195·7	208·3	87·91	
94·1–106·9	6·4	195·4	208·4	88·27	
				88·02	87·68

These determinations were carefully made, and the results are fairly concordant; as, however, this agreement was not inconsistent with the existence of a constant source of error, it appeared desirable to repeat some of them by a different method.

A polarising photometer was, therefore, set up (Plate 8, fig. 2), consisting of two Nicols and a right-angled prism; the Nicols, which were furnished with divided circles and verniers reading to 1°, were placed in the same straight line, and about 30 cm. apart, the right-angled prism being between them, and so placed that the field of view of the analysing Nicol was bisected vertically by the edge of the prism.

Two pieces of white paper were fixed in vertical planes at right angles to one another, both being illuminated by a small Argand gas burner; one was seen directly through the two Nicols, and the other, through the analyser only, by reflection in the prism. A blackened diaphragm was fixed between the prism and the second piece of paper, the aperture being of the same apparent size as the circular diaphragm of the polarising Nicol.

On looking through the analysing Nicol a circular white field was seen, bisected vertically, the two halves being usually of unequal brightness. On rotating the analyser one half of the field (that due to the reflection of the second paper in the prism), remained unchanged, whilst the other varied in brightness, being quite dark in two positions (when the Nicols were "crossed"). There were, of course, four positions of the analyser in which the two halves of the field appeared equally illuminated.

The light reflected by the right-angled prism was examined with a double-image prism and a plate of selenite, and was found to be completely unpolarised.

The observations were made by first determining the position of the analyser in which the field appeared equally bright throughout; then the plate of glass to be examined was placed between the right-angled prism and the white surface, and the new position of the analyser, in which there was equality of illumination, observed. As the intensity of the light which traversed the two Nicols varied as the square of the cosine of the angle between their principal sections, the percentage amount transmitted by the glass was given by $100 \times \cos^2 \alpha' / \cos^2 \alpha$, where α is the angle between the principal sections of the Nicol, when the field was uniformly bright without the glass, and α' when it was interposed.

The analysing Nicol was first rotated "clockwise," and readings made in each of the four quadrants of the position in which the two halves of the field appeared equally illuminated; the Nicol was then rotated "counter-clockwise," and four similar readings made, the mean of the eight readings being taken as the true position. The glass was then interposed between the diaphragm and the reflecting prism, and eight readings of the new position of the analyser, in which there was equality, made in the same way.

To determine the light transmitted by each piece of glass, four sets of eight observations were made without the glass, and four sets with it. Table III. gives the first set of each for the 6·5 mm. plate of crown glass; the other sets were about as concordant.

TABLE III.

Without Glass.

Readings of analyser.		Mean.*	α.
42° 25′	42° 45′	42° 35′	47° 25′
137 0	139 30	138 15	48 15
222 25	223 15	222 50	47 10
317 35	318 30	318 0	48 0
		Mean . . .	47 42

* As the intensity of the light varies as the square of the cosine, the geometrical, and not the arithmetical, is the true mean; but the observations did not appear to be sufficiently concordant to make it worth while to employ the longer process.

With 6·5 mm. plate of Crown Glass.

Readings of analyser.		Mean.*	α.
40° 15′	38° 0′	39° 07′	50° 53′
138 30	139 05	138 47	48 47
222 0	223 45	222 52	47 08
319 30	320 20	319 55	49 55
		Mean . . .	49 11

Table IV. gives the values of α determined in this way for the 6·5 mm., 11·5 mm., and 15 mm. plates of crown glass, and the percentage amount of light transmitted, as calculated from these numbers.

Table IV.

Crown glass. 6·5 mm. plate.

α.		Percentage amount of light transmitted.
Without glass.	With glass.	
47° 42′	49° 11′	94·32
45 44	49 36	86·22
46 16	48 37	91·45
46 54	48 37	93·61
		Mean 91·40

11·5 mm. plate.

α.		Percentage amount of light transmitted.
Without glass.	With glass.	
42° 03′	44° 53′	91·05
40 14	43 45	89·54
41 16	44 37	89·68
40 09	42 54	91·85
		Mean 90·53

15 mm. plate.

α.		Percentage amount of light transmitted.
Without glass.	With glass.	
41° 54′	45° 29′	88·73
40 23	43 42	90·08
40 17	43 44	89·82
		Mean 89·54

This method is clearly incapable of giving very accurate results. It is difficult to judge of the equality of the illumination in the two halves of the field, and also the angle through which the Nicol has to be turned to make the comparison is small, and, therefore, a slight error in the determination of the value of α makes a very considerable one in the result.

The measurements with the polarising photometer not being entirely satisfactory, another form of photometer was devised (Plate 8, figs. 3 and 3A). It consisted essentially of two white surfaces illuminated by the same lamp, the light falling very nearly perpendicularly on both. One surface was at a constant distance from the lamp, whilst the other could be brought nearer to, and moved further from it ; one surface was seen directly, and the other through the glass to be examined, and then the distance of the movable surface from the lamp altered till both surfaces appeared equally bright.

One surface was fixed at a distance of 78 cm. from the lamp and in the same horizontal plane, whilst the second was fixed to a vertical screen, which could be moved backwards and forwards by means of a pulley and catgut band along a board with a divided scale. The surfaces consisted of a double thickness of white paper, as it was found by taking a double thickness the apparent illumination was increased, a portion of the light which passed through the first paper being reflected back by the second. In front of the lamp two right-angled prisms were placed. They were held in position by two pieces of wood, through which a screw was passed, one prism being slightly in advance of the other, and overlapping it to a small extent. To adjust the prisms they were placed on a smooth table, the one resting directly on the table, and the other on a thin piece of card, and, after being adjusted, fixed in position by means of the screw. When so placed, the line dividing the two fields of view was much narrower than when the front surfaces of the prisms were in the same vertical plane and their edges in contact. A screen was placed between the prisms and the lamp, which were only 8 cm. apart.

The light of the lamp fell nearly perpendicularly on the white paper, and the direction of that reflected back to the prism was also nearly normal to the paper, so that the distance of the paper from the lamp could be altered without altering to any considerable extent the angle at which the light fell upon it, or the angle under which it was seen.

The distance of the moveable surface from the lamp, when its reflection in the prism and that of the fixed surface appeared equally bright, was determined (1) without any glass being interposed, (2) with a plate of glass between the fixed surface and the prisms, and (3) between the moveable surface and the prisms, six readings being made of each of these positions, and then the prisms reversed so that the surface which had been seen by reflection in the one was seen by reflection in the other, and six more readings made.

Calling the apparent brightness of the fixed surface C, the distance of the moveable surface from the lamp without the glass x, with the glass between the fixed surface and the prisms x', and with the glass between the moveable surface and the prisms x'', and the coefficient of transparency of the particular piece of glass k, then the two surfaces will appear equally bright when

$$C = \frac{1}{x^2}; \qquad Ck = \frac{1}{(x')^2}; \qquad \text{and } C = k\frac{1}{(x'')^2};$$

whence

$$k = \left(\frac{x}{x'}\right)^2, \quad \text{or} \quad \left(\frac{x''}{x}\right)^2, \quad \text{or more simply} \quad \left(\frac{x''}{x'}\right).$$

The percentage amount of light transmitted by the 6·5 mm. and the 24·3 mm. plates of crown glass was determined in this way. Table V. gives the results.

TABLE V.

6·5 mm. plate.

Readings.		Percentage of light transmitted.
x''.	x'.	
cm.	cm.	
78·0	84·4	92 42
77·8	84·9	91 64
		Mean 92·03

24·3 mm. plate.

Readings.		Percentage of light transmitted.
x''.	x'.	
cm.	cm.	
78·0	87·0	89·65
74·1	85·9	86·26
		Mean 87·95

In order to have obtained really accurate results with this method it would have been necessary to have made a large number of observations, and taken their mean; but, as the results obtained with it, and with the polarising photometer, agreed fairly well with one another and with the far larger number of observations made with the first method, it was thought unnecessary to continue the observations, the agreement between those obtained by all three methods being sufficient to show that there was but slight, if any, probability of a constant error due to the photometer itself used in the first series.

Table VI. gives the results of the three methods and the probable error of each determination calculated by the ordinary formula, $0·674 \sqrt{(\Sigma e^2)} . /n$.

TABLE VI.—Percentage Amount of Light Transmitted.

	First method.	Second method.	Third method.
Crown glass—			
6·5 mm. plate	91·50 ± ·06	91·40 ± 1·07	92·03 ± ·18
11·5 ,, ...	90·07 ,, ·07	90·53 ,, ·32	
15·0 ,, ...	89·13 ,, ·07	89·54 ,, ·22	
18·5 ,, ...	88·51 ,, ·08		
24·3 ,, ...	87·16 ,, ·15	. .	87·95 ,, ·81
Flint glass—			
7·0 mm. thick	88·83 ,, ·09		
49·0 ,, ...	85·40 ,, ·07		
69·5 ,, ...	82·57 ,, ·11		
91·3 ,, ...	80·74 ,, ·07		
Common plate glass—			
6 mm. thick	87·68 ,, ·10		
3 plates of 6 mm. crown glass .	88·08 ,, ·05		

Section II.—Amount of Light Reflected at a nearly Perpendicular Incidence.

The small percentage of light reflected by glass at a perpendicular incidence rendered its direct determination difficult; a fairly satisfactory method of measurement was, however, at length devised. The principle of the method was the obvious one of comparing the amount of light which reached the photometer when it came direct from the lamp with that which reached it after reflection from the glass.

It was found necessary to use two lamps, as no single-lamp apparatus, such as had been used for the transmission experiments, could be employed.

One of the lamps, a small Argand gas burner, was placed at the end of the photometer board (Plate 8, fig. 1), and a similar gas burner attached to the arm of a goniometer fixed at a short distance from the other end of the photometer board, the vertical axis of the goniometer being in the prolongation of the median line of the board. The glass of which the reflective power was to be determined could be placed with its surface vertical, and in the axis of the goniometer.

The experiments were made by first comparing the illumination produced by the two lamps when the light from both fell directly on the "photometer," the arm of the goniometer carrying the lamp being in the prolongation of the line joining the fixed lamp and the axis of the goniometer; the glass plate was then attached to the goniometer with its surface vertical and nearly normal to the line joining the fixed lamp and the axis of the goniometer, the arm of the goniometer rotated until the light again fell on the photometer after reflection from the glass plate, and the position of equality determined.

It was not necessary that the illuminating power of the two lamps should be equal: it was necessary that the ratio between their illuminating powers should remain as nearly constant as possible. The gas for the two burners came from the same supply pipe, and was passed through a bell-and-valve regulator; in spite of the regulator, it was found impossible to get satisfactory measurements, except in fairly still weather; the slight flickerings in the flames which occurred whenever there was much wind prevented the position of equality of illumination being determined with any degree of accuracy.

The measurements were always made in a dark room, a "detector" gas burner being used to read the position of the index on the scale; and, in order to reduce the stray light as much as possible, cylindrical metal chimneys, 5·5 cm. in diameter and 18 cm. in height, blackened externally and internally, were placed round the glass chimneys of the burners, a rectangular aperture being cut in each at the level of the flame. Two black wood screens with square openings, similar to those used in the transmission experiments (p. 248), were fixed at either end of the photometer board; there were black cloth screens behind and above the board, and the walls and ceiling of the room were painted a dead black; the metal clamp by which the glass plate was attached to the goniometer was made as small as possible and blackened, and the goniometer itself was covered with a black cloth whilst the observations were being made.

In order to measure the intensity of the light reflected by the glass both when incident normally and at various angles, that is, to compare the intensity of light which under certain circumstances would be partially polarised with the intensity of unpolarised light, it was necessary that the photometric surface should be normal both to the incident light and to the line of sight; hence, the photometer which had been used in the transmission experiments clearly could not be used, nor indeed could a Bunsen's disk or any of its modifications. A new form of photometer was, therefore, devised. Two wooden screens were fixed to the sides of a block 10 cm. across, similar to the one which had carried the photometer in the first set of experiments; in these rectangular apertures were cut, 3 cm. by 2 cm., and "parchment" paper fastened over them; the two right-angled glass prisms which had been used in the third method for determining the amount of the transmitted light (Plate 8, fig. 3A, p. 260) were placed between the screens, and in a line with the apertures. The two papers, each illuminated by the light of one of the lamps, were seen by reflection in the prisms, and by moving the block, to which an index was fixed, along the photometer board a position could be found in which the two images appeared equally bright.

Glass only reflecting from 4 to 5 per cent. of the light incident normally upon its surface, and the photometer scale being only 2 metres long, it was impossible to compare the intensities of the direct and reflected light when the two lamps had the same illuminating power. During the first set of experiments, those marked A, the necessary difference was obtained by keeping the flame of the comparison lamp turned down rather low. Subsequently, in the determinations marked B, the same result was obtained by different sized apertures in the metal chimneys; that in the chimney of the goniometer lamp was 10 mm. by 18 mm., whilst the one in the chimney of the comparison or fixed lamp was only 10 mm. by 6 mm.; the gas flames were so regulated that the apertures appeared completely filled with a uniformly bright flame. When the gas pressure changes slightly, the size of a flame, and not its intrinsic brilliancy, is mainly what alters; and, therefore, by limiting the visible portion of the flame in this way a greater constancy in the ratio between the illumination produced by the two lamps was obtained; but, as the tables show, the measurements made when the whole flame, and those in which the central portions only were used, agree satisfactorily.

This method for determining the reflective power possesses the obvious defect that, owing to the necessary alteration in the course of the light, the direct and reflected light cannot be interchanged, and, therefore, a constant source of error may easily exist. It seems probable that, to a small extent, such was the case, and that the measured amounts of the reflected light were slightly too high.

When the lamp attached to the goniometer was so placed that the light fell directly on the photometer, the whole beam of light which passed through the aperture in the chimney fell solely on the screen at the end of the photometer board, and no light diffused from the lamp reached the photometer. When, however, the lamp was so placed that the light was reflected from the glass, part of the light fell on the wall of the room, and, although this was at a distance of about 2 metres and painted black, some light must have been diffused from it towards the photometer; the metal clamp also in which the glass was held, although made as small as possible and blackened, certainly reflected some light. In order to obtain some idea of the amount of light which reached the photometer from these sources, the clamp was fixed to the goniometer without any glass; the left half of the field was not absolutely dark, but the amount of illumination was far too slight to be measurable, in fact it was almost imperceptible when the light from the comparison lamp illuminated the other half of the field; hence, the error from this cause can only be small.

In order to eliminate as far as possible any error due to a change in the relative amount of illumination produced by the lamps, four readings were first made of the position of the photometer when the two halves of the field were equally bright, the light of both lamps falling directly on it; four readings of the position when the light was reflected from the glass were then made; and then four more with the direct light: the mean of the eight readings being taken as the true position of the photometer when the light reached it directly.

Calling the two sources of light m and n, the distance between them x, the two positions of the photometer in which there is equality of illumination x_1 and x_2, and K the coefficient of reflection for the particular plate of glass, then

$$\frac{m}{x_1^2} = \frac{n}{(x-x_1)^2} \quad \text{and} \quad \frac{\mathrm{K}m}{x_2^2} = \frac{n}{(x-x_2)^2},$$

$$m = \frac{nx_1^2}{(x-x_1)^2} \quad \text{and} \quad \mathrm{K}m = \frac{nx_2^2}{(x-x_2)^2};$$

therefore

$$\mathrm{K} = \frac{x_2^2\,(x-x_1)^2}{x_1^2\,(x-x_2)^2} \quad \text{or} \quad \left\{\frac{x_2\,(x-x_1)}{x_1\,(x-x_2)}\right\}^2.$$

The lamps not being placed at the ends of the divided scale and the two translucent screens of the photometer being necessarily at some distance apart, in order to obtain x_1 and x_2 the distance between the lamp and the zero of the scale had to be added, and the distance between the translucent screen and the index subtracted from the scale reading; thus, the distance from the axis of the lamp carried by the goniometer to the zero of the scale being 45·6 cm., in the first series of experiments, and the index of the photometer being at a distance of 5·7 cm. from the paper, the scale readings, plus 39·9, gave the value of x_1 and x_2. Similarly, 16·9 cm. added to the difference between the readings and 200 gave the value of $(x - x_1)$ and $(x - x_2)$.

In the first set of experiments the distances from the lamps were measured from the axes of the flames, which were about 15 mm. in diameter; in all the others, for which portions only of the flames were used, the distances were measured from the apertures in the chimneys.

The same glass was used for these experiments that had been employed in the transmission experiments already described. In order to prevent the light reflected from the second surface of the glass reaching the photometer the measurements were made with prisms cut from the 18·5 mm. plate of crown glass, and from the flint glass block; one face of each of these prisms was formed by one of the original surfaces of the glass, and these faces, which had not been repolished, were used for the reflection experiments.

Table VII. gives some of the actual measurements made with the prism of crown glass. The first four columns contain measurements made with the light from the whole surface of the flames; the others with light from the central portions only. In the first set the light was incident upon the glass at an angle of 6° 17′, and in the second at 6° 47′.

Table VII.

A.				B.			
Direct light.	Mean with direct light.	Reflected light.	Mean with reflected light.	Direct light.	Mean with direct light.	Reflected light.	Mean with reflected light.
cm.	cm.	cm.	cm.	cm.	cm.	cm.	cm.
128·8				117·0			
129 8				118·9			
127·8				118·1			
130·1		28·8		119·3		24·0	
	128·8	30·1	28·7		118·9	24·2	24·1
127·8		28·0		118·6		23·8	
129·4		28·0		120·1		24·3	
127·6				119·3			
129·1		29·2		120·2		23·7	
	127·9	28·7	28·8		119·4	24·2	24·2
126·3		27·7		118·3		24·1	
128·0		29·6		120·3		24·7	
126·7				119·2			
128·1		28·5		119·6		24·1	
	127·2	28·7	28·4		118·8	25 0	24·4
126·1		28·0		118·3		24·5	
128·0		28·6		118·1		24·2	
126·3				118·2			
127·9		27·8		118·6		23·5	
	127·0	28·9	28·2		118·3	24·3	24·1
126·9		27·6		118·1		24·2	
128·0		28·4		117·5		24·3	
125·8				118·5			
127·3				119·6			

From the mean results the values of x_1, x_2, $(x - x_1)$ and $(x - x_2)$ were obtained, and from these the percentage amount of light reflected by the glass calculated by the formula $K = \left\{\frac{x_2(x - x_1)}{x_1(x - x_2)}\right\}^2$. These values, and others obtained from similar sets of measurements, are contained in Table VIII.

TABLE VIII.

Crown Glass.

Angle of incidence.	x_1.	$(x - x_1)$.	x_2.	$(x - x_2)$.	Per cent. of incident light reflected.
A. 6° 17′	168·7	88·1	68·6	188·2	3·62
	167·8	89·0	68·7	188·1	3·75
	167·1	89·7	68·3	188·5	3·78
	166·9	89·9	68·1	188·7	3·78
6° 17′	171·5	85·3	71·7	185·1	3·71
	173·6	83·2	74·5	182·3	3·84
	174·3	82·5	74·0	182·8	3·67
	175·6	81·2	76·4	180·4	3·83
B. 6° 47′	156·1	95·2	61·3	190·0	3·87
	156·6	94·7	61·4	189·9	3·82
	156·0	95·3	61·6	189·7	3·93
	155·5	95·8	61·3	190·0	3·95
7° 30′	158·4	92·5	62·2	188·7	3·70
	157·8	93·1	61·7	189·2	3·70
	157·8	93·1	62·4	188·5	3·81
	157·7	93·2	62·3	188·6	3·81
	157·5	93·4	61·8	189·1	3·76
				Mean	3·78

Similar measurements were made with the flint glass wedge. The results are given in Table IX.

TABLE IX.

Flint Glass.

Angle of incidence.	x_1.	$(x - x_1)$.	x_2.	$(x - x_2)$.	Per cent. of incident light reflected.
10°	156·2	94·7	68·2	182·7	5·12
	155·9	95·0	67·8	183·1	5·09
	155·1	95·8	68·7	182·2	5·42
	155·6	95·3	69·1	181·8	5·42
10°	157·6	93·3	68·9	182·0	5·02
	157·6	93·3	69·4	181·5	5·12
	157·2	93·7	69·4	181·5	5·20
				Mean	5·20

Section III.—Amount of Light Reflected at a nearly Perpendicular Incidence after Repolishing.

Lord RAYLEIGH found ('Roy. Soc. Proc.,' vol. 41, p. 389) that, although the glass surfaces he examined were free from any apparent tarnish, the amount of light they reflected was largely increased by repolishing. The wedge of crown glass was, therefore, repolished on December 21, 1887, by means of a disk of wood charged with putty powder and mounted in a lathe (the same method that Lord RAYLEIGH had used), and its reflective power redetermined immediately; it was found to reflect 4·29, instead of 3·78, per cent. The glass was again examined on January 5, 1888, and it then reflected 4·20 per cent.

Two days later the glass was repolished a second time with fine rouge, and again examined; it reflected 4·22 per cent. of the incident light.

After an interval of five months, on June 13, this piece of glass, the surface of which had become considerably tarnished, was rubbed with wash-leather until the moisture deposited on the glass by breathing gently on it evaporated quite uniformly; it was then examined, and found to reflect 4·42 per cent. of the incident light. The next day it was repolished for the third time, and examined immediately; it reflected 4·30 per cent.

An attempt was made to repolish the flint glass wedge on February 28, 1888, with both putty and rouge, but a surface free from scratches could not be obtained. Its reflective power, however, was increased, and it reflected 6·20 per cent., instead of 5·20, after this imperfect polishing.

The surface not being satisfactory, the glass was sent to Mr. HILGER to be repolished. It was received back on the evening of March 2, and examined on the 3rd; it reflected 6·06 per cent.

After three months the glass was again examined, the film which had formed on its surface having been previously removed by rubbing with a wash-leather; it only reflected 5·71 per cent., although the surface appeared perfectly polished. On June 11 it was repolished with very fine washed rouge, and was found to reflect 6·25 per cent. of the incident light. Two days later, on June 13, it only reflected 5·73 per cent.

The polishing with putty powder was effected by means of a soft wood disk, mounted in a lathe, the disk being kept moist. For the rouge a polisher was formed by cementing a piece of silk to a sheet of plate glass fastened to a table, and charging this with carefully washed rouge: the glass was held against the rapidly rotating disk in the one case, and rubbed over the fixed surface in the other. The relative velocities of glass and polisher were very different in the two cases, and with the disk the friction was sufficiently great for the glass to become sensibly warm.

Tables X. and XI. give the details of these experiments ; with the crown glass the angle of incidence was 7° 30′, and with the flint glass 10°.

Table X.

Crown Glass.

	x_1.	$(x - x_1.)$	x_2.	$(x - x_2.)$	Per cent. of incident light reflected.
December 21, 1887.—Re-polished with putty	157·8	93·1	65·0	185·9	4·25
	157·0	93·9	64·8	186·1	4·34
				Mean	4·29
January 5, 1888	157·8	93·1	64·3	186·6	4·13
	156·6	94·3	63·9	187·0	4 23
	156·0	94·9	63·5	187·4	4·25
				Mean	4·20
January 7, 1888.—Repolished with rouge	155·7	95·2	63·0	187·9	4·20
	154·9	96·0	62·6	188·3	4·24
	155·5	95·4	63·3	187·6	4·28
	156·2	94·7	63·2	187·7	4·17
				Mean	4·22
June 13	159·6	91·3	67·5	183·4	4·43
	159·6	91·3	67·3	183·6	4·40
	159·8	91·1	67·7	183·2	4·44
				Mean	4·42
June 14.—Repolished with rouge	160·8	90·1	68·1	182·8	4·36
	160·9	90·0	67·6	183·3	4·25
	160·9	90·0	67·8	183·1	4·29
				Mean	4·30

TABLE XI.

Flint Glass.

	x_1.	$(x - x_1.)$	x_2.	$(x - x_2.)$	Per cent. of incident light reflected.
February 28, 1888.—Repolished with rouge and putty	154·6	96·3	71·2	179·7	6·09
	153·4	97·5	70·8	180·1	6·24
	152·9	98·0	70·5	180·4	6·27
				Mean	6·20
March 3, 1888, 5 P.M. — Repolished by Mr. HILGER.	156·7	94·2	73·6	177·3	6·23
	157·4	93·5	73·0	177·9	5·94
	157·4	93·5	73·8	177·1	6·13
	157·1	93·8	73·6	177·3	6·14
				Mean	6·11
March 3, 1888, 9 P.M.	156·1	94·8	71·6	179·3	5·88
	155·4	95·5	71·5	179·4	6·00
	155·4	95·5	71·6	179·3	6·02
	155·2	95·7	72·1	178·8	6·17
				Mean	6·02
June 5, 1888	159·9	91·0	74·1	176·8	5·69
	159·9	91·0	74·7	176·2	5·82
	160·2	90·7	74·9	176·0	5·82
	160·4	90·5	73·6	177·3	5·51
				Mean	5·71
June 7, 1888	160·2	90·7	75·7	175·2	5·98
	160·8	90·1	74·4	176·5	5·58
	160·6	90·3	75·3	175·6	5·81
	159·7	91·2	75·3	175·6	6·00
				Mean	5·84
June 11, 1888	160·5	90·4	74·0	176·9	5·55
	160·5	90·4	74·3	176·6	5·61
	160·5	90·4	74·6	176·3	5·68
	161·0	89·9	74·6	176·3	5·58
				Mean	5·60
June 11, 1888.—Repolished with rouge	166·0	84·9	81·5	169·4	6·05
	165·7	85·2	82·5	168·4	6·34
	165·8	85·1	82·6	168·3	6·35
				Mean	6·25
June 13, 1888	160·1	90·8	74·5	176·4	5·74
	160·4	90·5	74·4	176·5	5·66
	160·5	90·4	75·1	175·8	5·79
				Mean	5·73

Section IV.—Amount of Light Reflected at Various Incidences between 0° *and* 90° *by the Crown Glass before and after Repolishing.*

In the experiments already described the light incident on the glass consisted of a divergent beam, or rather of a complex of rays, the mean incidence of which, if such a term may be used, was as nearly normal as the construction of the apparatus permitted.

Measurements were also made with the crown glass both before and after it was repolished, when the light was incident upon its surface at various angles, the angle between the axis of the beam of light and the normal to the surface being considered as the angle of incidence. Table XII. gives these results.

TABLE XII.—Crown Glass.

Angle of incidence.	Before repolishing.					After repolishing.				
	x_1.	$(x-x_1)$.	x_2.	$(x-x_2)$.	Per cent. of light reflected.	x_1.	$(x-x_1)$.	x_2.	$(x-x_2)$.	Per cent. of light reflected.
10° B.	156·5	94·6	61·4	189·7	3·83	155·5	95·4	63·1	187·8	4·25
	156·7	94·4	61·5	189·6	3·82	150·0	95·9	63·1	187·6	4·36
	156·8	94·3	61·2	189·9	3·76	154·4	96·5	63·5	187·4	4·48
						154·0	96·9	63·6	187·3	4·56
				Mean	3·80				Mean	4·41
20° B.	157·2	93·9	61·1	190·0	3·69	154·9	96·0	66·1	184·8	4·91
	156·2	94·9	60·9	190·2	3·78	154·7	96·2	65·3	185·6	4·79
	155·9	95·2	60·7	190·4	3·79	154·7	96·2	65·1	185·8	4·75
	155·7	95·4	60·7	190·4	3·81	154·4	96·5	65·2	185·7	4·81
				Mean	3·77				Mean	4·81
30° A.	171·8	85·0	73·8	183·0	3·98					
	170·4	86·4	74·1	182·7	4·23					
	170·6	86·2	73·1	183·7	4·04					
	170·6	86·2	73·1	183·7	4·04					
B.	155·0	96·1	59·3	191·8	3·67	153·6	97·3	63·0	187·9	4·51
	154·4	96·7	59·9	191·2	3·86	152·9	98·0	62·8	188·1	4·58
	153·8	97·3	59·7	191·4	3·89	153·0	97·9	62·9	188·0	4·58
	154·2	96·9	58·7	192·7	3·68	152·8	98·1	62·5	188·4	4·53
				Mean	3·92				Mean	4·55
40° B.	156·4	94·7	64·7	186·4	4·42	158·3	92·6	72·0	178·9	5·54
	156·5	94·6	65·6	185·5	4·57	158·1	92·8	71·3	179·6	5·43
	156·3	94·8	65·8	185·3	4·64	158·2	92·7	70·4	180·5	5·22
	155·7	95·4	64·4	186·7	4·47	157·4	93·5	69·3	181·6	5·14
						156·6	94·3	68·9	182·0	5·19
				Mean	4·52				Mean	5·30

Table XII.—continued.

Angle of incidence.	Before repolishing.					After repolishing.				
	x_1.	$(x-x_1)$.	x_2.	$(x-x_2)$.	Per cent. of light reflected.	x_1.	$(x-x_1)$.	x_2.	$(x-x_2)$.	Per cent. of light reflected.
50° A.	162·2	94·6	72·5	184·3	5·26					
	161·3	95·5	71·9	184·9	5·30					
	161·2	95·6	72·2	184·6	5·38					
	161·2	95·6	72·4	184·4	5·42					
B.	154·3	96·8	68·6	182·5	5·56	158·6	92·3	75·6	175·3	6·19
	152·8	98·3	67·8	183·3	5·66	158·3	92·6	76·2	175·3	6·29
	152·4	98·7	68·3	182·8	5·85	158·3	92·6	74·7	176·2	6·46
	151·9	99·2	67·7	183·4	5·81	157·9	93·0	74·5	176·4	6·15
				Mean	5·53				Mean	6·27
56° 34′ B.	156·4	94·7	77·3	173·8	7·25	155·3	95·6	79·7	171·2	8·21
	158·7	92·4	78·4	172·7	6·99	155·5	95·4	79·9	171·0	8·22
	159·5	91·6	79·2	171·9	7·00	155·2	95·7	79·9	171·0	8·30
	157·7	93·4	79·9	171·2	7·64	154·9	96·0	79·4	171·5	8·23
				Mean	7·22				Mean	8·24
60° A.	163·1	93·7	85·6	171·2	8·25					
	163·2	93·6	84·9	171·9	8·02					
	161·9	94·9	85·3	171·5	8·50					
	161·8	95·0	84·1	172·7	8·17					
B.	154·4	96·7	80·2	170·9	8·64	155·1	95·8	84·5	166·4	9·84
	153·4	97·7	80·7	170·4	9·10	154·5	96·4	83·5	167·4	9·68
	152·2	98·9	79·5	171·6	8·88	153·9	97·0	83·7	167·2	9·95
	151·9	99·2	78·3	172·8	8·76	153·7	97·2	83·8	167·1	10·06
				Mean	8·54				Mean	9·88
65° A.	166·8	90·0	97·0	159·8	10·73					
	166·8	90·0	97·5	159·3	10·90					
	167·4	89·4	98·6	158·2	11·08					
	168·2	88·6	97·8	159·0	10·50					
B.	154·1	97·0	87·1	164·0	11·18	156·4	94·5	91·9	159·0	12·20
	153·3	97·8	87·1	164·0	11·48	155·8	95·1	91·5	159·4	12·28
	152·1	99·0	86·6	164·5	11·74	155·8	95·1	91·6	159·3	12·32
	151·5	99·6	85·9	165·2	11·66	155·4	95·5	91·2	159·7	12·31
				Mean	11·16				Mean	12·28
70° A.	167·9	88·9	109·8	147·0	15·64					
	167·6	89·2	109·8	147·0	15·80					
	168·0	88·8	110·0	146 8	15·69					
	169·5	87·3	111·6	145·2	15·67					

TABLE XII.—continued.

Angle of incidence.	Before repolishing.					After repolishing.				
	x_1.	$(x-x_1)$.	x_2.	$(x-x_2)$	Per cent. of light reflected.	x_1.	$(x-x_1)$.	x_2.	$(x-x_2)$.	Per cent. of light reflected.
70° A.	169·2	87·6	109·1	147·7	14·62	158·7	92·2	105·8	145·1	17·94
	167·3	89·5	109·0	147·8	15·57	157·7	93·2	105·2	145·7	18·21
	166·0	90·8	106·8	150·0	15·17	157·0	93·9	104·4	146·5	18·16
	165·3	91·5	107·4	149·4	15·83	157·0	93·9	105·5	145·4	18·83
									Mean	18·28
B.	156·9	94·2	99·3	151·8	15·42					
				Mean	15·49					
75° B.						160·1	90·8	119·4	131·5	26·52
						159·5	91·4	118·9	132·0	26·64
						158·7	92·2	116·7	134·2	25·52
						157·8	93·1	117·3	133·6	26·83
									Mean	26·33

Section V.—Amount of Light Transmitted after Repolishing.

The effect of repolishing on the amount of light transmitted by the glass was examined. The first observations, those with the 24·3 mm. plate of crown glass (except the last) were made with the photometer used in the reflection experiments, the arm of the goniometer carrying the lamp being clamped in the prolongation of the line joining the fixed lamp and the axis of the goniometer. The small slot in front of the flame of the fixed lamp was subsequently replaced by one of the same size as that in front of the lamp carried by the goniometer, and most of the observations made with this arrangement, in which there were two lamps of nearly equal illuminating power. Finally, a photometer with a single lamp, two mirrors, and inclined paper surfaces, like that used for the original transmission experiments, was fitted up, and this was used for all measurements made in August and September, 1888, that is, those with the flint glass and the four last determinations of the light transmitted by the crown glass.

The measurements were made as described in Section I., but only four readings were taken in each position of the glass, and the results were calculated out by the expression $k = \frac{x_1(x - x_2)}{x_2(x - x_1)}$ (see p. 250), where k is the coefficient of transparency, x the distance between the two lights, and x_1 and x_2 the two positions of the photometer in which there is equality of illumination, the optical shortening of the path of the light due to its passage through the glass being of course allowed for.

The results are contained in Tables XIII. and XIV.

Table XIII.

Crown Glass.

	x_1.	$(x - x_1)$.	x_2.	$(x - x_2)$.	Per cent. of incident light transmitted.
6·5 mm. plate. July 6, 1888.—Cleaned, but not repolished.	127·2	135·1	133·1	129·2	91·39
	126·9	135 4	132·7	129·6	91·53
	127·7	134 6	133·4	128·9	91·67
				Mean	91·53
July 13.—"Ground grey and repolished" by Mr. Hilger	125·9	136·4	132·3	130·0	90·70
	127·2	135·1	133·3	129·0	91·11
	126 0	136·3	133·1	129·2	89·73
				Mean	90·31
July 17.—Re-examined	125·8	136·5	132·2	130·1	90·70
	126·0	136·3	132·0	130·3	91·25
	125·9	136·4	132·1	130·2	90·98
				Mean	90·90
11·5 mm. plate. August 13.—Cleaned, but not repolished .	187·8	196·8	198·0	186·6	89·93
	187·9	196·7	197·5	187·1	90·50
	187·8	196·8	197·5	187·1	90·40
				Mean	90·28
11·5 mm. plate *bis*. July 13.—"Ground grey and repolished" by Mr. Hilger	126·1	136·0	132·5	129·6	90·69
	126·1	136·0	133·6	128·5	89·18
	125·7	136·4	132·4	129·7	90·28
				Mean	90·05
July 16.—Re-examined	125·6	136·5	132·8	129·3	89·59
	125·6	136·5	133·0	129·1	89·32
	125·5	136·6	132·9	129·5	89·52
				Mean	89·48
July 17.—Re-examined	124·9	137·2	131·7	130·4	90·14
	124·5	137·6	132·0	130·1	89·18
	124·8	137·3	131·7	130·4	90·00
				Mean	89·77

TABLE XIII.—continued.

	x_1.	$(x - x_1)$.	x_2.	$(x - x_2)$.	Per cent. of incident light transmitted.
11·5 mm. plate *bis* (continued).					
August 13.—Repolished with fine rouge on silk, and subsequently (11/8/88) with putty powder on wood disk	187·4 187·9 188·7	197·2 196·7 196·9	198·2 198·2 198·3	186·4 186·4 186·3	89·37 89·84 90·03
				Mean	89·75
15 mm. plate.					
July 6.—Cleaned, but not repolished . . .	125·6 126·1 126·2	136·4 135·9 135·8	133·2 133·6 133·3	128·8 128·4 128·7	89·04 89·18 89·72
				Mean	89·31
July 13.—" Ground grey and repolished " by Mr. HILGER	125·3 125·6 126·1	136·7 136·4 135·9	134·1 133·6 133·9	127·9 128·4 128·1	87·42 88·50 88·77
				Mean	88·23
July 17.—Re-examined	124·8 124·8 124·9	137·2 137·2 137·1	132·2 132·5 132·7	129·8 129·5 129·3	89·31 88·90 88·77
				Mean	88·99
August 17.—Repolished with putty powder on wood disk	185·4 185·5 185·6	199·1 199·0 198·9	197·1 197·0 196·9	187·4 187·5 187·6	88·54 88·72 88·90
				Mean	88·72
18·5 mm. plate.					
July 16.—Cleaned, but not repolished . .	125·4 124·9	136·5 137·0	133·3 133·1	128·6 128·8	88·63 88·22
				Mean	88·41
July 17.—Re-examined	123·5 123·6 124·4	138·4 138·3 137·5	131·6 131·6 131·8	130·3 130·3 130·7	88·35 88·49 89·72
				Mean	88·85
August 17.—Re-examined	184·6 185·7 185·2	199·8 198·7 199·2	197·2 196·5 197·4	187·2 187·9 187·0	87·71 89·37 88·07
				Mean	88·38

TABLE XIII.—continued.

	x_1.	$(x - x_1)$.	x_2.	$(x - x_2)$.	Per cent. of incident light transmitted.
24·3 mm. plate.					
June 15.—Cleaned, but not repolished	84·7	165·4	92·4	157·7	87·40
	84·8	165·3	92·5	157·6	87·41
	84·9	165·2	93·9	156·2	85·49
				Mean	86·90
June 16.—Re-examined	86·7	163·4	95·6	154·5	85·75
	87·7	162·4	95·3	154·8	87·72
	88·0	162·1	95·9	154·2	87·29
				Mean	86·92
June 16.—Polished with rouge on silk	84·8	165·3	92·9	157·2	86·81
	85·5	164·6	93·3	156·8	87·30
	85·6	164·5	93·8	156·3	86·71
				Mean	86·93
June 23.—Repolished by Mr. HILGER; the polish was defective	84·6	165·5	94·6	155·5	84·03
	84·4	165·7	94·3	155·8	84·15
	84·2	165·9	94·3	155·8	83·85
	84·1	166·0	94·2	155·9	83·85
				Mean	83·97
June 30.—Repolished a second time by Mr. HILGER	85·5	164·6	92·9	157·2	87·90
	85·6	164·5	92·6	157·5	88·50
	84·6	165·5	92·5	157·6	87·09
				Mean	87·84
June 30.—Re-examined	86·4	163·3	94·0	156·1	87·86
	86·3	163·8	93·7	156·4	87·94
	86·7	163·4	94·3	155·8	87·67
				Mean	87·82
July 17.—Re-examined	123·0	138·7	132·0	129·7	87·14
	123·7	138·0	132·4	129·8	87·88
	123·3	138·4	131·7	130·0	87·94
				Mean	87·65

TABLE XIV.

Flint Glass.

	x_1.	$(x - x_1)$.	x_2.	$(x - x_2)$.	Per cent. of incident light transmitted.
7 mm. thick.					
September 30, 1888.—Cleaned, but not repolished	187·6	197·1	194·8	189·9	92·78
	187·7	197·0	194·7	190·0	92·98
	187·5	197·2	195·1	189·6	92·40
	187·5	197·2	195·0	189·7	92·50
				Mean	92·66
49 mm. thick.					
August 15.—Surface repolished with putty powder on August 14	183·0	200·1	198·4	184·7	85·14
	182·5	200·6	197·8	185·3	85·23
	182·9	200·2	198·1	185·0	85·32
	183·1	200·0	198·5	184·6	85·14
				Mean	85·21
August 16.—Re-examined	182·8	200·3	198·7	184·4	84·70
	183·2	199·9	198·0	185·1	85·68
	182·7	200·4	198·1	185·0	85·15
				Mean	85·18
August 16.—Repolished with putty powder and examined immediately	183·1	200·0	198·1	185·0	85·50
	182·3	200·8	198·9	184·2	84·08
	182·5	200·6	199·1	184·0	84·08
				Mean	84·55
August 18.—Re-examined	183·0	200·1	198·8	184·3	84·78
	182·7	200·4	198·7	184·4	84·61
	182·7	200·4	198·6	184·5	84·70
				Mean	84·70
69·5 mm. thick.					
August 15.—Cleaned, but not repolished .	182·1	200·2	198·5	183·8	84·22
	181·9	200·4	198·4	183·9	84·13
	182·1	200·2	198·0	184·3	84·67
	182·4	199·9	198·6	183·7	84·40
				Mean	84·35

Section VI.—Values of the Polarising Angles before and after Repolishing.

In order to determine the angles of polarisation, a form of apparatus essentially similar to that employed by SEEBECK ('POGGENDORFF, Annalen,' vol. 20, 1830, p. 27) was used. It consisted of a goniometer with a horizontal circle reading to 20″; the slit and lens of the collimator were removed and the observing telescope replaced by a tube to one end of which a vertical divided circle was fixed. A Nicol was contained in an inner tube, and by means of a vernier the position of its principal section could be read on the vertical circle to 5′. The stage of the goniometer and the arm carrying the Nicol were geared together by means of toothed wheels working into a double pinion, the number of teeth in the wheels and pinion being such that on moving the arm of the goniometer carrying the Nicol the angular velocity of the tube was twice that of the stage.

A small gas flame was placed close to the end of the collimator tube, the flame being surrounded by a blackened metal chimney with a small aperture in it, and the glass surface whose angle of polarisation was to be observed clamped to the vertical stage with its surface in the prolongation of the vertical axis of the goniometer, the stage turned till the image of the flame was seen through the Nicol, and then, by means of the pinion (the axis of which was fixed to a sliding piece), the stage and Nicol geared together.

The Nicol having been clamped with its short diagonal horizontal, the arm was moved till the light reflected by the glass was reduced to a minimum.

The end of the collimator tube nearest the lamp was bisected by a vertical thread; a pair of cross threads were placed in the inner end, and a diaphragm with a small aperture at the eye end, of the Nicol tube; and in making the observations care was taken that the image of the vertical thread should coincide with the point of intersection of the two cross threads as seen through the diaphragm.

The observations were made by moving the Nicol tube alternately towards the right and the left. As the image of the flame always remained in the field of view, and the room was completely dark, the angle at which the light was reduced to a minimum could be observed with a fair amount of accuracy.

The amount of light reflected by glass at a perpendicular incidence being small, and a satisfactory diagonal eyepiece not being available, the position of the stage in which the light was incident perpendicularly on the surface of the glass was determined by an indirect method.

The axis of the Nicol tube was first, by means of the diaphragm and cross threads, brought into the same line as that of the collimator tube, and its position read on the divided circle of the goniometer; the tube was then turned through 90°, the glass surface attached to the vertical stage and adjusted, and the stage rotated until the image of the single thread again coincided with the cross threads of the Nicol tube.

The stage reading gave the position in which the light was incident upon the surface at an angle of 45°. To verify the adjustment, the Nicol tube was clamped, first, at an angle differing by + 90°, and then at one differing by − 90°, from its original position.

The reading of the Nicol, when light polarised in the plane of incidence was cut off, had been carefully determined some years previously, and was again verified. To reduce, as far as possible, the errors due to the Nicol (which was of the ordinary construction, with its terminal faces not perpendicular to its geometrical axis), eight readings were made with the prism in one position, and eight more after it had been turned round through 180°.

The actual readings made with the 6·5 mm. plate of crown glass, which had been repolished by Mr. Hilger, were—

°	′	°	′	°	′	°	′
107	06	107	25	106	59	106	55
107	03	107	08	107	02	107	04
107	20	107	09	107	08	106	56
107	27	107	09	107	04	106	55.

The readings made with the other glass surfaces were about as concordant.

Table XV. gives the angles of polarisation as deduced from the means of these readings.

Table XV.

Angles of Polarisation.

	°	′	″
Crown glass--			
6·5 mm. plate, repolished by Mr. Hilger	56	22	30
11·5 ,, not repolished	56	14	30
11·5 ,, No. 2, repolished by Mr. Hilger and again with rouge	56	28	30
11·5 ,, ,, ,, with putty powder	56	49	0
15·0 ,, ,, by Mr. Hilger	56	42	30
15·0 ,, ,, with putty powder	56	49	30
18·5 ,, not repolished	56	21	30
18·5 ,, not repolished	56	25	30
24·3 ,, repolished by Mr. Hilger	56	37	30
Wedge used for reflection experiments repolished with putty powder, and subsequently with rouge	56	52	0
Flint glass—			
Face of block—not repolished	58	14	0
,, ,, repolished with putty powder and examined immediately	58	48	0
,, ,, repolished with putty powder and examined after two days	58	44	0

PART II.

When light passes through a transparent plate it is diminished by reflection at the two surfaces, and by "obstruction" within the plate, the cause of obstruction being that a part of the light which has entered the plate is absorbed and, unless the plate be absolutely homogeneous, a part scattered.

If r be the ratio of the light reflected by the first surface, and r' by the second surface, to the light incident upon them, α the coefficient of transmission, and t the thickness of the plate, then the intensity of the transmitted beam is given by the expression $i = I\rho\rho'\alpha^t$, where $\rho = (1 - r)$ and $\rho' = (1 - r')$.

I, i, and t being known, by eliminating $\rho\rho'$, α can be readily calculated. The value of i depends in the case of coloured media on the refrangibility of the light, but in the case of the two kinds of glass used in these experiments it may be taken to be the same for light of all wave-lengths.

Table XVI. contains the values of α for a thickness of one millimetre of crown and flint glass, obtained by combining in pairs the five values of i for crown glass, and the four values for flint glass, contained in Table VI.

TABLE XVI.

Values of α.

Crown glass.	Flint glass.
·99685	·99906
·99690	·99884
·99744	·99887
·99729	·99893
·99692	·99837
·99752	·99897
·99750	
·99809	Mean ·99884
·99763	
·99733	
Mean ·99735	

Dr. ROBINSON, in the paper already mentioned ('Phil. Trans.,' 1869, p. 160), gives the values of n in the expression $i = I\rho^2e^{-nt}$, ρ^2 being calculated from FRESNEL'S formula, and t being the thickness in inches. From the values given by Dr. ROBINSON for a cylinder of crown, and a prism of flint, glass, both of Messrs. CHANCE'S manufacture, the values of the coefficient α were calculated for a thickness of one millimetre.

	Index for line E.	n.	α.
Crown glass	1·5200	0·0272	0·99893
Flint glass	1·6216	0·0218	0·99914

From the values of α it would appear that these two specimens of glass absorbed somewhat less light than those used in the experiments of which an account is contained in this paper, but Dr. ROBINSON'S results depend on the value of ρ being "calculated *accurately* from FRESNEL'S formula," and if, as seems probable, glass usually reflects less than the theoretical amount of light, the amount absorbed would necessarily appear less than it really was.

From the mean values of α given in Table XVI. the values of ρ (on the assumption that $\rho = \rho'$) were obtained by calculating the values of α^t for the different thicknesses of the two kinds of glass used in these experiments and then introducing these values into the equation $i = \mathrm{I}\rho\rho'\alpha^t$.

TABLE XVII.

Values of ρ.

Crown glass.		Flint glass.	
6·5 mm. plate.	·9648	7·0 mm. plate.	·9463
11·5 ,,	·9636	49·0 ,,	·9508
15·0 ,,	·9629	69·5 ,,	·9461
18·5 ,,	·9648	91·3 ,,	·9475
24·3 ,,	·9642	Mean . .	·9477
Mean . .	·9641		

The value of r for the crown glass is therefore ·0359, and for the flint glass ·0523.

The amount of light which according to theory should have been reflected by the glass was calculated by the expression $\left(\frac{n-1}{n+1}\right)^2$, where n is the index of refraction. These values, and also the amount of the reflected light, as determined directly (see Tables VIII. and IX.), are given in Table XVIII.

TABLE XVIII.

Percentage amount of Light Reflected.

	Observed.		Calculated.
	By transmission.	By reflexion.	
Crown glass	3·59	3·78	4·187
Flint glass	5·23	5·20	5·780

The observed values of the light reflected by the crown glass do not agree quite so well as those for the flint glass; this may be due to the fact that all the measurements with the flint glass were made with one thick block, whilst several pieces of crown glass were used for the transmission experiments, and these plates may have differed slightly both in their composition and in the polish of their surfaces. Making due allowance for this, and for the approximate character of all photometric measurements, the agreement between the results obtained by two entirely distinct methods is, probably, quite as close as could have been anticipated.

The calculated value for both kinds of glass considerably exceeds the observed. As has already been mentioned, the refractive indices, as determined with the large prisms used for the reflection experiments, and with the small prisms, differed slightly; the differences, however, $-\cdot 0008$ and $+\cdot 0055$, are not sufficiently great to affect the result to any considerable extent. Thus, at a perpendicular incidence the two theoretical values for the reflected light are 4·187 and 4·176 for the crown glass, and 5·780 and 5·856 for the flint glass, or a difference of 0·011 and 0·076 per cent. of the incident light, a quantity which is, of course, quite inappreciable photometrically.

Both the crown glass and the flint glass had been recently polished, the former by Messrs. CHANCE and the latter by Mr. HILGER, both kinds of glass having been ground with emery and polished with rouge; the crown glass was partially machine-polished but finished by hand, the flint glass entirely hand-polished. The glass surfaces were always well cleaned with wash-leather immediately before being used, as after being left in the laboratory for some days they were usually somewhat tarnished; the films, however, were easily removed, and in no case could any deterioration of the surface be detected.

The effect of repolishing the glass was to increase its reflective power, but Tables X. and XI. show that the two kinds of glass behaved somewhat differently. Immediately after repolishing both reflected more than the theoretical amount of light; but, whilst the crown continued to do so, the reflective power of the flint decreased, and after an interval it reflected the theoretical amount.

TABLE XIX.

	Crown glass.	Flint glass.
Percentage of light reflected before repolishing	3·78	5·20
,, ,, ,, after repolishing { Immediately	4·27	6·14
,, ,, ,, after repolishing { After an interval	4·31	5·72
,, ,, ,, calculated from the observed index of refraction	4·19	5·78

A number of measurements were made of the light reflected by the crown glass at various angles before and after repolishing; the means of the results contained in

Table XII., and also those for crown glass from Table XIX., are given in the second and third columns of Table XX. The amount of light which, according to FRESNEL'S theory, should have been reflected by the glass was calculated for the various incidences by means of the formula

$$J_r^2 = \frac{1}{2}\left\{\frac{\sin^2(i-r)}{\sin^2(i+r)} + \frac{\tan^2(i-r)}{\tan^2(i+r)}\right\},$$

the values of r being determined from the observed value of the refracting index of the glass; the fourth column contains the results.

By assuming the truth of the theory, the value of the refractive index could, of course, be readily deduced from the amount of light reflected at a nearly normal incidence, this being equal to $\left(\frac{n-1}{n+1}\right)^2$. Before repolishing, the crown glass reflected 3·78 per cent. of the incident light; hence, the value of n would be 1·4842. Assuming that such was the case, the amount of light reflected by the glass at various angles was calculated, and these numbers are given in the fifth column.

TABLE XX.

Percentage amount of Light Reflected by Crown Glass.

Angle of incidence.	Observed.		Calculated.	
	Before repolishing.	After repolishing.	From observed value of n.	From calculated value of n.
6° 17′, 6 47, 7 30	3·78	4·29	4·19	
10 0	3·80	4·41	4·19	3·81
20 0	3·77	4·81	4·21	3·81
30 0	3·92	4·55	4·34	3·93
40 0	4·52	5·30	4·77	4·34
50 0	5·53	6·27	5·98	5·52
56 34	7·22	8·24	7·62	7·23
60 0	8·54	9·88	9·16	8·63
65 0	11·16	12·28	12·31	11·75
70 0	15·49	18·28	17·37	16·78
75 0	..	26·33	25·58	

The percentage amount of light reflected before and after repolishing and the amount calculated from the observed index of the glass are represented by the curves on Plate 8, fig. 4, where the abscissæ are the angles of incidence, and the ordinates the percentages. The curve for the values deduced from the index calculated from the amount of light reflected normally is not given, as it coincides so closely with the curve for the glass in its original state that, in order to render the differences visible, it would have been necessary to draw the diagram on a much larger scale.

More observations were made with the glass before repolishing than after, which accounts for the one curve being so much smoother than the other.

These results show (1) that repolishing increased the amount of the reflected light; (2) that before repolishing the amount of light reflected was less than the theoretical amount calculated from the observed index of refraction of the glass, but that in the case of the crown glass, by assuming a value for the index in accordance with the amount of light reflected at a perpendicular incidence, the amount reflected at other angles by the glass before repolishing was given correctly by the formula; (3) that after repolishing the observed amount of light reflected exceeded the calculated amount; (4) that in the case of the flint glass what may be described as the "polishing-effect" passed off in the course of a day or two, and then the theoretical and actual intensities of the reflected light agreed, but that with the crown glass this did not appear to be the case.

The effect of repolishing being to increase the amount of light reflected by the glass, it seemed desirable to ascertain whether repolishing would produce any change, and, if so, whether increase or diminution, in the amount transmitted. If the increase in the reflected light were due to a more perfect surface being obtained, and, therefore, to less light being irregularly reflected or diffused, the intensity of the transmitted beam would certainly not be weakened by repolishing; if, however, it were due to an increase in the refractive index of the surface-layer of the glass, then the intensity of the transmitted beam would be decreased.

Table XXI. contains the means of the values set forth in Tables XIII. and XIV., and in the second column the values for the transmitted light obtained with the same samples of glass two years previously (see Table VI.).

TABLE XXI.

Percentage amount of Light Transmitted by Crown Glass.

	Original determinations.	Not repolished.	Repolished with rouge.	Repolished with putty.
6·5 mm. plate . .	91·50	91·53	90·60	
11·5 " . .	90·07	90·28	89·77	89·75
15·0 " . .	89·13	89·31	88·61	88·72
18·5 " . .	88·51	88·55		
24·3 " . .	87·16	86·91	87·77	

Percentage amount of Light Transmitted by Flint Glass.

	Original determinations.	Not repolished.	Repolished with putty and examined	
			Immediately.	After an interval.
7 mm. thick . .	88·83	92·66		
49 ,, . .	85·40	. .	84·55	85·03
69·5 ,, . .	82·57	84·35		
91·3 ,, . .	80·74			

These numbers show that, except with the 24·3 mm. plate of crown glass, the effect of repolishing was to decrease the amount of light transmitted by both kinds of glass; they also show that, whilst the amount transmitted by the crown glass was the same as when it was first examined, the amount transmitted by the flint glass had increased considerably, although both kinds of glass had been kept during the interval wrapped in soft paper and in the same room.

The 24·3 mm. plate of crown glass behaved differently from the others. As is stated in Table XIII., it was first cleaned, and the amount of light it transmitted determined; each surface was then polished for 20 minutes with fine rouge on a silk polisher. This produced no change, and the plate was, therefore, sent to an optician, who returned it with the statement that it had been "polished with rouge on pitch, almost dry, to get a high polish." The moment the glass was placed in the photometer the polish was seen to be defective, the surface being apparently grained, and, as the table shows, its transmissive power was greatly decreased; it was returned to the optician to be again repolished, and then it let through more light than when first tested.

The values of ρ, calculated with the value of α previously obtained (p. 280), are given in Table XXII.

TABLE XXII.

Values of ρ for the Crown Glass.

	Original determinations.	Not repolished.	Repolished with rouge.	Repolished with putty.
6·5 mm. plate . .	·9648	·9650	·9601	
11·5 ,, . .	·9636	·9647	·9617	·9619
15·0 ,, . .	·9629	·9640	·9601	·9608
18·5 ,, . .	·9648	·9644		
24·3 ,, . .	·9642	·9642	[·9672]	
Mean	·9641	·9645	·9609	

Values of ρ for the Flint Glass.

	Original determinations.	Not repolished.	Repolished with putty and examined	
			Immediately.	After an interval.
7 mm. thick . .	·9463	·9665		
49 ,, . .	·9508	. .	·9460	·9482
69·5 ,, . .	·9461	·9562		
91·3 ,, . .	·9475			
Mean	·9477			

These results agree with those obtained by the direct measurement of the reflected light, and show that the effect of the repolishing is to increase the amount of light reflected and to decrease the amount transmitted, and this latter effect must be due to some cause other than a more perfect surface having been obtained.

The polish of the glass plates was examined by holding them close to the aperture in one of the screens of the photometer, and allowing the beam of light from the lamp to pass through the glass, the cross-section of the beam being smaller than the surface of the glass, and all other light being carefully excluded. If the surfaces had been perfectly polished, they would have been quite invisible, but such was not the case, the shadow cast by the edge of the aperture being just visible in all cases.

Examined in this way, there did not seem to be much, if any, difference between the various plates of crown glass, of which two were in their original state and four had been repolished, nor between them and the four faces of the flint block, of which two had been repolished.

The surfaces of the crown and flint glass wedges used for the reflection experiments did not appear to be quite so good; the difference, however, was very slight. The 7 mm. plate of flint glass was much inferior to all the other pieces, the boundaries of the beam of light which passed through it being distinctly visible. When examined in a strong light there appeared to be a slight film on the surface; on continued rubbing with a wash-leather this diminished, and then the surface of the plate, when placed in a beam of light in the dark room, was less visible than before. The inferiority of the surfaces of this plate appeared to be due to the films which had formed on them, and not to any roughness due to imperfect polishing.

The truth of BREWSTER's law, that the tangent of the polarising angle of a substance is numerically equal to its index of refraction, being generally admitted, it seemed desirable to ascertain the values of the polarising angles for the different plates before and after repolishing.

Table XXIII. gives the mean results collected from Table XV. The means show that the effect of repolishing was in all cases to increase the polarising angle, a result which is in accordance with those previously obtained.

Table XXIII.—Polarising Angles.

Crown Glass.

Not repolished.	Repolished with rouge.	Repolished with putty powder.
° ′ ″	° ′ ″	° ′ ″
56 14 30	56 22 30	56 49 0
56 21 30	56 28 30	56 49 30
56 25 30	56 42 30	56 52 0
	56 37 30	
Mean 56 20 30	56 32 45	56 50 10

Flint Glass.

Not repolished.	Repolished with putty powder.
° ′ ″	° ′ ″
58 14 0	58 48 0
	58 44 0
Mean 58 14 0	58 46 0

Table XXIV. gives the values of the refractive indices of the two kinds of glass as determined directly, and as deduced from the amounts of the reflected and transmitted light, and from the values of the polarising angles, both before and after repolishing. The values obtained in these ways do not agree well together, those deduced from the amount of the transmitted light being considerably the lowest; they show, however, that repolishing increased the theoretical value of the index as determined by three independent methods.

Table XXIV.

Values of the Refractive Indices.

		Crown glass.	Flint glass.
Observed		1·5145	1·6330
Calculated from amount of light reflected, determined directly.	Before repolishing	1·4830	1·6280
	After repolishing	1·5220	1·6590*
			1·6290†
Calculated from amount of light reflected, deduced from amount transmitted.	Before repolishing	1·4676	1·5930
	After repolishing	1·4928	1·6055
Calculated from polarising angle.	Before repolishing	1·5017	1·6149
	After repolishing with rouge	1·5136	
	,, ,, putty	1·5293	1·6490

* Immediately after repolishing.

† After an interval.

Sir DAVID BREWSTER stated, many years ago ('Phil. Trans.,' 1815, p. 126), that glass "acquires an incrustation or experiences a decomposition by exposure to the air which alters its polarising angle without altering its general refractive power," and added that by the action of heat alone he had produced a variation of 9° in the polarising angle of flint glass.

SEEBECK ('POGGENDORFF, Annalen,' vol. 20, 1830, p. 27) made a number of determinations of the polarising angles of different kinds of glass, and found that there was considerable difference between the observed values and those calculated from the refractive index, except in the case of surfaces which he himself had ground and polished (with emery and colcothar).

With one specimen of flint glass the difference was originally $-38'$; he then polished it himself, and found that the difference was only $+3'$; after being polished by an optician, the difference became $+28'$.

SEEBECK was of opinion that these differences were due to the treatment which the glass had received whilst being polished and cleaned, and that lapse of time made no change. The only experiments he appears to have made on this latter point were with crown glass, the surface of which, as the experiments here recorded show, does not alter, or at least only alters very slowly.

Lord RAYLEIGH found ('Roy. Soc. Proc.,' vol. 41, p. 275) that repolishing prisms of crown glass caused a considerable increase in the amount of light they reflected; but his experiments do not show that when the prisms were first examined by him they reflected less light than when they were originally polished.

CONCLUSION.

It seems probable that the amount of light reflected by freshly polished glass varies with the way in which it has been polished, and that, if a perfect surface could be obtained without altering the refractive index of the surface-layer, then the amount would be accurately given by FRESNEL'S formula, but that usually the amount differs from that given by the formula, being sometimes greater and sometimes less.

The formation of a film of lower refractive index on the glass would account for the defect in the reflected light; but, to account for the excess, it seems necessary to assume that the polishing has increased the optical density of the surface-layer, and the changes produced in the amount of light transmitted and in the angle of polarisation support this view.

After being polished, the surface of flint glass seems to alter somewhat readily, the amount of the reflected light decreasing, and the amount of the transmitted increasing, whilst with crown glass the change, if any, proceeds very slowly.

There is no evidence to show to what particular cause these changes are due.

The values of the transmission coefficients for light of mean refrangibility for the two particular kinds of glass are given, and show that for 1 centimetre the loss by obstruction amounts to 2·62 per cent. with the crown glass, and 1·15 per cent. with the flint glass.

Explanation of Plate 8.

Fig. 1. Double mirror photometer.
A. Photometer board.
B. Scale.
C. Lamp.
DD. Mirrors.
E. Block carrying photometer.
F. Photometer.
G. Wood stops.
H. Screens.
I. Windows in screens.

Fig. 1A. Photometer.
A. Base.
B. Wooden prisms.
CC. Pieces of white paper.
DDD. Windows in casing.

Fig. 2. Polarising photometer.
AA. Nicols.
B. Right-angled prism.
C. Lamp.
DD. Pieces of white paper.
E. Screen.

Fig. 3. Prism photometer.
A. Photometer board.
B. Scale.
C. Lamp.
DD. Pieces of white paper.
E. Block carrying photometric surface.
F. Screen.
G. Prisms.

Fig. 3A. Photometer.
A. Base.
BB. Right-angled prisms.
C. Screw.

Fig. 4. Curves representing the percentage amount of light reflected by crown glass at various angles of incidence.
1. Observed values with the glass in its original condition.
2. Calculated values from observed index of refraction.
3. Observed values with repolished glass.

IX. *On the Total Solar Eclipse of August* 29, 1886.

By Captain L. Darwin, *R.E.*, Arthur Schuster, *Ph.D.*, *F.R.S.*, *and* E. Walter Maunder.

Received January 28,—Read February 14, 1889.

[Plates 9, 10.]

Contents.

I. Origin of the Expedition and General Preparations. By Captain Darwin, Arthur Schuster, and E. W. Maunder.

An expedition for observing the Eclipse of the Sun of August, 1886, was organised and sent out by the Royal Society, the necessary funds being obtained partly by a special vote from the Treasury, partly from the annual grant to the Royal Society, and partly from the Society's private funds. A Committee appointed by the Council of the Royal Society discussed the principal questions to which observers were to direct their attention, and distributed the available instruments amongst them. It was also decided that, as far as the scientific part of the work was concerned, the observers should be independent of each other, and report separately to the Society; but that they should elect one of their members as chief, to represent them in all dealings with the authorities in the West Indies. Mr. NORMAN LOCKYER was accordingly chosen to be this representative.

The present report only deals with the photographic results obtained by its authors. Mr. NORMAN LOCKYER was the only other observer who took out photographic instruments; most unfortunately, the weather proved so bad at the station he selected that he was unable to see anything of the eclipse.

Captain ABNEY was unfortunately not able to take part in the expedition, but he gave his invaluable help to the observers in their preparations, and in this way contributed most materially to whatever success the photographic part of the expedition may have obtained. The photographic plates used by Dr. SCHUSTER and Mr. MAUNDER were prepared by him, and we wish to offer him our best thanks for the assistance he has rendered us.

The expedition left England on the 29th of July, 1886, and arrived at St. George, Grenada, on the 12th of August. A letter had kindly been sent by the Colonial Office to the colony, stating what the requirements of the expedition would be. The members consequently found on their arrival that the Governor, Mr. W. J. SENDALL, had made every possible inquiry as to the best sites for the observatories, taking into consideration the weather probabilities as well as their personal comfort; and they have to thank him for the greatest courtesy and consideration during the whole of their stay in the island. The protection of the instruments having been mentioned in the Colonial Office letter, Captain I. C. MALING, the Colonial Secretary, very kindly prepared models of huts which could be cheaply and readily constructed on the spot. When the expedition arrived at Barbadoes on their outward journey, they found these models awaiting them. A telegram was despatched to Grenada approving generally of the design, and thus work was actually commenced before the arrival of the expedition in the island.

Before the observers left England, the President of the Royal Society had written to the Admiralty requesting the co-operation of any men-of-war that might be on the station. As a result of this communication, three of Her Majesty's ships—the "Fantôme," Commander R. H. ARCHER, R.N.; the "Bullfrog," Lieutenant J. MASTERMAN, R.N.; and the "Sparrowhawk," Lieutenant C. F. OLDHAM, R.N.—were found ready and prepared to render every assistance. Every member of the expedition felt grateful for the willing way in which the valuable assistance of both the officers and men of these ships was given; and the President of the Royal Society, in a letter to the Admiralty on the return of the expedition, expressed the value to science of such ready co-operation.

After the arrival of the expedition at Grenada, two or three days were occupied in selecting stations and making preparations for conveying the observers to their destinations. St. George itself was not favourably situated, and it was moreover considered advisable to scatter the observatories as much as possible, so as to avoid the chance of a single mass of cloud proving fatal to the whole expedition. The observers were therefore divided into four groups, and, with the aid of the above-mentioned men-of-war, they soon found themselves distributed amongst their various stations. Mr. MAUNDER, who accompanied the Rev. S. J. PERRY, was conveyed in the "Bullfrog" to Carriacou Island, where a station was selected near the southern extremity of the island, close to a small house called "the Hermitage," belonging to Mr. PETER DRUMMOND, a gentleman of Jersey, who happened most fortunately to be visiting Carriacou at the time, and who, in the most generous manner, gave up the use of his premises to the observers. Mr. SCHUSTER and Captain DARWIN were dropped at Prickly Point by the "Fantôme," and there they found excellent quarters in a house which Mr. F. M. CHADWICK, the Colonial Treasurer, kindly placed at their disposal.

II. PREPARATIONS FOR THE ECLIPSE AT PRICKLY POINT. BY CAPTAIN DARWIN AND ARTHUR SCHUSTER.

The instruments used during totality at Prickly Point were mounted on two equatorial stands, which were placed in separate huts at a distance of about 20 yards from each other. On the evening of Thursday, August 19, the polar axes of both stands were adjusted in the usual way. Finders had been attached for this purpose to the photographic cameras, but more attention should be given in future to have these finders in convenient positions for observation, and of not too small an aperture.

One instrument, which was under Captain DARWIN's charge, was placed on a solid rock foundation, and the first adjustment was therefore considered sufficient, especially as extreme accuracy was not required for the purpose for which it was chiefly employed.

The foundation for the equatorial stand which carried Dr. SCHUSTER's instrument did not, unfortunately, prove sufficiently firm. As there was reason to fear that the heavy rains during the week preceding the eclipse might have altered the position of the polar axis sufficiently to interfere with the sharpness of the image, and as the camera could not be reversed for adjustment as long as the spectroscopes were attached to it, these were once more removed on Friday, August 27, two days before the eclipse, and the routine of adjustment once more gone through. From observations taken the day after the eclipse it appeared that the altitude of the polar axis was about 3′ too low; the error was therefore sufficiently small not to produce a detrimental effect during the time of exposure.

The clocks of the instruments were frequently adjusted, by comparing the rotation of the hour circle when the instrument was going with the time marked on a chronometer. In neither case were the instruments and their stands designed for each other, and it was found impossible to balance the instruments properly without a too great increase of weight. Thus the work which the clocks had to perform was very different in different positions of the instrument. To remedy this evil as much as possible, the clock adjustment was carried on in the position which the equatorials were to have during totality.

Owing to the unsettled state of the weather, the preparations for the eclipse were carried on under great difficulties, and the time at our disposal was found barely sufficient. We arrived at the observatory on a Tuesday, and the remainder of that week was taken up with the erection of the equatorials and the preliminary adjustments of the various instruments. During the week preceding the eclipse much time was lost owing to the frequent interruption of the work by heavy tropical showers. Tuesday and Wednesday were wet and stormy and no direct Sun light was available, although much required, to get the instruments into working order. Thursday was fine, and in the morning good progress was made; but Friday was again wet, and was followed by a rainy night. Saturday, the day before the eclipse, was cloudy in the morning, and the Sun only appeared at intervals. Our experience has thus taught us that a fortnight's time for preparation is hardly sufficient when two observers have to look after five different instruments, all requiring careful treatment. After the days had been spent in continuous work, the evenings were taken up with the preparation of photographic chemicals and occasional star observations.

We were without intelligent help except during the two days when Mr. LAWRANCE came to Prickly Point. We had taken him out, jointly with Professor THORPE, as private assistant, anticipating the great difficulties we should have to encounter. His time, however, was chiefly taken up at Hog Island, where Professor THORPE was observing, but our thanks are due to him for the assistance he gave us during these two days. As for unskilled help, we engaged for the whole time one negro servant, but now consider we should have done better to have had two.

The damp climate, with its steady temperature, varying day and night only between 81° and 85°, proved very exhausting, and the work could not be carried on as well and as quickly as it might have been under more favourable circumstances.

The richness of animal life proved a source of great annoyance. Mosquitos abounded in our residence. Wasps built their nests and spiders spun their webs with remarkable rapidity. The photographic room and even one of the equatorial stands had to be cleared of wasps' nests, and a dense spider's web was found an hour before totality stretched across the slit of one of the spectroscopes; the slit having been perfectly clear the night before.

It is difficult to realise at home the special difficulties of temporary observatories, but we venture to suggest that more skilled assistance should be provided at future eclipses. Although at Prickly Point we were enabled to carry out our programme with hardly any mishaps, we feel obliged to point out the difficulties under which we worked and which might easily have led to serious accidents. For example, five minutes before totality, Captain DARWIN's clock stopped altogether, although it is believed every possible pains had been taken in its regulation. It was only by a lucky alteration in the balance at the last moment that it was induced to go on at all.

Captain ARCHER had kindly sent us two sailors to render help during totality, viz., SAMUEL BROWETT, signalman, and HENRY STEELE, petty officer. One of these two was placed between the two huts to call out the time at intervals of half a minute while the other was to assist Captain DARWIN in the manipulation of his instrument. They both did what was required of them admirably.

During totality Captain DARWIN required no other assistance, as his programme was not heavy; but with Dr. SCHUSTER, whose whole observations were included in that time, the case was different. He gratefully accepted therefore the help offered to him by Dr. P. F. McLEOD, the officer of health in Grenada, and by Mr. MURRAY, who accompanied the expedition as naturalist.

Captain MALING had undertaken to make a drawing of the outer parts of the corona. According to a suggestion of Mr. LOCKYER'S, a disc had been prepared $6\frac{3}{4}$ inches in diameter. When placed at the proper distance the Moon and the inner parts of the corona could be screened off by means of this disc. It was placed on a wooden support at the top of an incline which ran down from the observatory to the sea shore. A post was driven into the ground some distance away from the disc. The observer was to look through a small hole in the post. This arrangement, however, wants very careful adjustment, and we had not much time to spare, as the principal objects of the expedition monopolised nearly all our attention. Whether from want of adjustment or from other causes, we cannot now decide, Captain MALING'S drawing includes the whole of the corona down to the prominences. Captain MALING'S statement is included in this report.

The following Table gives the position of the observatory and various data connected with the eclipse:—

Latitude	12°	0′ N.
Longitude	61°	45′ W.

Commencement of totality—

	h.	m.	s.
G.M.T.	23	17	12
L.M.T.	19	10	12
Duration of totality	—	3	50

Altitude of Sun	18°	45′
Parallactic angle	80°	25′
Angle between circle of declination and Sun's axis towards East	20°	37′

III. Totality at Prickly Point. By Captain Darwin and Arthur Schuster.

It has been mentioned that during the week preceding the eclipse, which took place on the Sunday, the weather had been very unfavourable; but the clouds cleared away on Saturday afternoon, the sunset was fine, and experience had taught us that a fine evening was generally followed by a fine morning. Our prospects, therefore, were very good on Saturday night, but early on Sunday morning the wind rose, which was a bad sign. At five o'clock, however, when we got up, the sky was still perfectly clear; the clouds came from the East at half-past five, at first in the form of detached cumuli, but before long the whole sky towards the East was overcast. The Sun rose behind the clouds and was still hidden when the time for first contact arrived. It was only twenty minutes before totality that the crescent of the Sun appeared, and then the wind soon made a clearance all round him.

Ten minutes before totality the sky was clear. The last instructions were given, the instruments put in position, and everybody took his appointed place. Five minutes before totality, as the darkness increased visibly, the weather seemed safe. Another minute passed and danger once more threatened; a small cloud rising from the South-East was driven by the wind right towards the Sun. The Moon from above descended over the Solar disc, and it seemed a race between the Moon and the cloud which should cover it first. Totality began, and the corona became distinctly visible, but was obscured again almost instantaneously by the cloud. For about three-quarters of a minute the corona was only seen through a film of cloud as a narrow hazy ring. The corona finally appeared, and remained clear as long as totality lasted.

Lieutenant R. J. Kidd, then Private Secretary to the Governor of Grenada, took temperature observations every five minutes for one quarter of an hour before and after totality. His numbers are as follows:—

h.	m.	° Fahr.
6	55	82
7	0	82
7	5	82
7	10	81·75
7	15	81·75
7	20	81·5
7	25	82
7	30	82·5

As the commencement of totality took place at 7^h 10^m it will be seen that during totality the thermometer only fell one quarter of a degree Fahrenheit, and reached its lowest point about ten minuntes after totality. The whole change in temperature was exceptionally small.

IV. On the Accuracy required in Adjusting an Equatorial for Photographic Purposes during Total Solar Eclipses. By Arthur Schuster.

Observers preparing for a total Solar eclipse have in general only a very moderate time at their disposal, and it is of great importance to them to settle beforehand to what degree of accuracy the adjustment of their instruments is to be carried. The time which is spent over adjustment in any one direction must necessarily be taken away from other more important matters, as there is never any lack of work on these occasions.

If, for instance, a photographic picture of the Solar corona is to be obtained, it would be clearly waste of time to refine on the adjustments of the equatorial or the rate of the clock beyond the point at which the Sun's change in declination would produce a visible effect. We shall see that this consideration limits the time of exposure for which the full advantage of the aperture of the lens can be realised, and this again will give us a limit beyond which it would be unnecessary to adjust the equatorial. We might, indeed, take the Sun's apparent motion into account, and point the instrumental axis, not to the pole, but to some point near it, which might easily be determined by calculation. We shall see, however, that the available time of exposure of a 4-inch lens is quite sufficient for our present requirements, and that it is therefore unnecessary to make allowance for the Sun's change of declination.

When the instrument is nearly adjusted, the relation between the true declination of a star δ and the apparent declination δ' is given by—

$$\delta = \delta' - \gamma \cos(\tau - \delta),$$

where γ is the angle between the true pole and the instrumental pole, τ is the hour angle of the star, and δ the hour angle of the instrumental pole. If δ is constant the change of apparent declination from bad adjustment in a short time t is found from the above equation to be $-\gamma t \sin(\tau - \delta)$, which will, numerically, always be smaller than γt. If the time t, measured in minutes of time, is p, we can write for this maximum change—

$$44 \times 10^{-4} p\gamma.$$

We find in this way that a change of apparent declination of one second of arc per minute could be produced if

$$\gamma = 3' \ 49''.$$

Now, the change of the Sun's declination may be, and during the last eclipse was, nearly one second of arc per minute.* It will be unnecessary, therefore, to spend any

* During the total phase of the late eclipse, owing to the low altitude of the Sun (18° 45′), the apparent change of altitude due to change of refraction was about $2\frac{1}{2}$ seconds of arc; but the change in declination due to refraction was small, and generally the effects of refraction may be neglected.

time in making the angle γ smaller than that given by the above value, as a closer adjustment may in some cases make matters worse instead of better.

Special considerations may of course induce observers to adjust their instruments more accurately. If, for instance, the equatorial adjustment is to be done once for all about a fortnight or a week before the eclipse, and if the conditions of the foundation on which the pillar rests give grounds for fear that the position may slightly change, it would be wise to aim at a greater accuracy in the first instance, so as to give some play for change.

So far the adjustments do not depend on the size of the object-glass, but, if we want to make the best use we can of the aperture, an apparent shifting of the Sun's image during the time of exposure must be confined within small limits, which we have now to fix.

The central disc of a star, or of a small object at a great distance, has an angular radius Θ in the focal plane of the telescope, given by

$$\Theta = 1{\cdot}22\,\lambda/\mathrm{R},$$

when R is the diameter of the objective. Experience shows that small objects can, under favourable circumstances and with strong illumination, be resolved when the centre of one diffraction disc coincides with the first dark ring; but for objects having no well-defined boundaries we must give wider limits, and I therefore take the point of fair resolution to be reached when the first two dark rings touch, so that the star discs stand perfectly clear of each other. In order to find out experimentally to what extent a small shifting of the images, such as might be produced by defective adjustment of the equatorial, might influence the possibility of resolution, I drew on a piece of paper two circles touching each other, corresponding to the two star discs, and another similar set above them slightly displaced in the line joining the centres. It was found that a displacement of one-tenth of the distance between the centres of the disc could only affect the resolution to a slight degree. We may, therefore, consider a displacement through one-fifth of the radius of each disc allowable without impairing definition. There is always something arbitrary in the fixing of these limits, but it must be done in every investigation in which we want to determine the accuracy to be aimed at. As object-glasses of different makers would probably differ 10 per cent. in their observed resolving powers, the limit chosen seems a fair and reasonable one.

We may allow then an angular shifting of

$$\phi = \Theta/5 = 1{\cdot}22\,\lambda/5\mathrm{R} = 1{\cdot}22 \times 10^{-5}/\mathrm{R}$$

if we substitute for λ its mean value of about 5×10^{-5} cm. This gives a value for ϕ which varies inversely with the aperture, and is equal to 2·5 seconds of arc for one centimeter aperture. If the exposure takes place during p minutes, and if the change of the Sun's declination in one minute is τ seconds of arc, the displacement will be $p\tau$, and we get for the longest time of exposure compatible with full definition—

$$\mathrm{R}p\tau = 2\cdot5.$$

In the West Indian Eclipse R was 10 and τ was ·9, which gives about 17 seconds as time of exposure. This is sufficient for obtaining a good image; and we conclude, therefore, that an adjustment of the polar axis to about $3'\cdot5$ is sufficient for full definition. Photographs taken with longer exposures would chiefly serve to fix the fainter parts of the outer corona.

By the ordinary means of adjusting an equatorial it is not difficult to obtain the necessary accuracy; nevertheless, the calculation shows that we can by no means neglect to go through the full routine of adjustment, and no equatorial should be sent out for eclipse purposes without complete appliances in the way of good finders, circles which are easily read, &c.

The adjustment of the rate of the clock to the necessary accuracy is, as will appear, much more difficult.

We have seen that the angular displacement may reach, but should not exceed, $1\cdot22 \times 10^{-5}/\mathrm{R}$ during the time of exposure. Reduced to seconds of time, this is equal to $\cdot17/\mathrm{R}$, or, if the exposure is to be q seconds of time, the error of the clock should not exceed $\cdot17/q\mathrm{R}$, which for an exposure of 17 seconds and an aperture of 10 cm. is one part in a thousand.

The instrument we had at our disposal did not allow an adjustment as accurate as this. At Prickly Point it was found difficult to go beyond an accuracy of one per cent. No record of the rate was taken at Carriacou, but it was probably no better than at Prickly Point. This would show that full definition would not be possible with an exposure of more than two seconds.*

We may finally calculate by how much the efficient aperture is reduced if the errors due to adjustment are greater than those we have hitherto allowed. If the centres of two star discs are separated by a distance $2\Theta + \mathrm{K}$, a displacement equal to K would just bring them into contact. A telescope which to the eye would resolve two stationary stars at a distance 2Θ will, if during the time of photographic exposure the displacement is K, have its effective aperture reduced in the ratio of $(2\Theta + \mathrm{K}) : 2\Theta$. Hence, if R′ be the effective aperture

$$\frac{\mathrm{R} - \mathrm{R}'}{\mathrm{R}'} = \frac{\mathrm{K}}{2\Theta}.$$

* Some of the statements made here do not agree with those made in the Preliminary Account ('Roy. Soc. Proc.' vol. 42, p. 180). The discrepancy is accounted for by the fact that I previously took wider limits for the allowable shifting due to the motion of the telescope, and that I have since then subjected the photographs to a closer investigation as regards fineness of detail actually shown.

Hitherto we have put $K = \frac{1}{5}\Theta$; but supposing the rate of the clock is such that during the exposure $K = 2\Theta$, which was very nearly the case during the late eclipse, the above equation shows that the effective aperture was reduced to half, or that the photographs could not show anything which an eye-observer with a telescope of 2-inch aperture might not have seen.

Let us now turn our attention to the resolving powers which have hitherto been actually obtained in photographs taken of the corona.

During the Eclipse of 1871 two prominences were separated in a photograph, which were at a distance of 15 seconds, and the corona itself gives no evidence of a finer structure. The aperture used was 4 inches.*

In the corona photograph of 1882 the diameter of one prominence subtends an apparent angle of about 15 seconds. As far as the photograph is concerned, we have no reason to suppose that an aperture resolving two stars at a distance of 15 seconds could not have shown everything that is seen on the photograph, for no detail of the corona gives evidence of smaller structure. This gives an aperture of 2 inches as sufficient to resolve all the detail shown in this photograph.

An examination of the drawing made by Mr. WESLEY of the present eclipse gives substantially the same result.

The 4-inch glasses used in the eclipses which I have mentioned all give, therefore, a resolution equal to that obtainable with a 2-inch aperture on a stationary object.

During the last eclipse the regularity of the clock motion was, as we have seen, not sufficient to give more perfect images; the same cause has very likely stood in the way of more perfect images during previous eclipses. We have at present no reason to suppose that the corona possesses any structural detail that could not be seen with an aperture of two inches, and it would be very desirable if eye observations could be taken at some future eclipse which would give us some idea of the detail of the structure visible with larger instruments.

But it is very remarkable that the definition obtained is just what we should have to expect, if we take the error of clock motion into account. If the motion of the clock is regular, we should by shorter exposures get in great part over the difficulty of adjustment, but in the instruments which have been at our disposal the mechanism of transmission from the clock to the telescope is not as perfect as it might be made, and the irregular vibrating motion produced by this cause would be sufficient to damage the sharpness of the image.

There does not seem to be any advantage at present in using larger apertures in future eclipses, at least if these larger apertures are accompanied by increased focal length. Most of the adjustments have to be more accurate for them, in order to get the advantage of the increased aperture, and the difficulties of mounting and adjusting are

* Owing to an oversight, the aperture was in the Preliminary Account stated to be 2 inches. The focal length of the lens was only half that used during the later eclipses, but the aperture was the same.

greater. The photographs would be larger and more convenient to copy, which would be an advantage, but it seems more rational to improve the definition obtained with a 4-inch lens, before larger apertures are used.

If we could increase the aperture without increasing the focal length, we could reduce the time of exposure and gain an advantage, provided that the clock motion is sufficiently uniform. All our investigations, then, seem to point to the desirability of an improvement not only in the average rate of the clock motion, for that could easily be effected, but in the steadiness and regularity of the motion.

I give, in conclusion, collected together, some formulæ which may be useful to future eclipse observers.

In the equations ϕ is the displacement in seconds of arc which is allowable, consistent with full definition as above defined.

R is the true aperture of the lens in centimeters.

R′ is the effective aperture of the lens.

q is the time of exposure in seconds.

Q is the longest time of exposure compatible with full definition.

γ is the greatest allowable angle between the true pole and the pole of the instrument in seconds of arc.

τ is the change of Solar declination measured in seconds of arc per second of time.

e is the error of the clock rate, measured in per cent.

In equations (2), (4), and (6) it is assumed that in adjusting the pole of the equatorial no account is taken of the change of the Solar declination during the eclipse.

$$\phi = 2{\cdot}5/\mathrm{R} \quad \ldots\ldots\ldots \quad (1),$$

$$\mathrm{Q} = 2{\cdot}5/\mathrm{R}\tau \quad \ldots\ldots\ldots \quad (2),$$

$$\gamma = 34600/\mathrm{R}q \quad \ldots\ldots\ldots \quad (3);$$

or, if the longest exposure Q is to be used,

$$\gamma = 13840\tau \quad \ldots\ldots\ldots \quad (4).$$

Finally, to determine R′, we easily find

$$\frac{1}{\mathrm{R}'} - \frac{1}{\mathrm{R}} = 60 \times 10^{-4} \times eq. \quad \ldots\ldots\ldots \quad (5).$$

This equation will allow us to determine the time of exposure allowable for a given effective aperture R′ if the clock rate is known. If the longest exposure Q is to be used, we get

$$\frac{\mathrm{R} - \mathrm{R}'}{\mathrm{R}'} = \frac{1{\cdot}5 \times 10^{-3} \times e}{\tau} \quad \ldots\ldots\ldots \quad (6).$$

V. The Photographic Camera. By Arthur Schuster.

1. *Adjustments.*

The lens used for obtaining photographs of the corona had a clear aperture of 4 inches (10·1 cm.), and a focal length of about 5 ft. 3 in. (160 cm.). It therefore gave images of the Sun of about ·3 in. in diameter.

The accurate adjustment of the camera is a matter of some difficulty, as distant objects are not sufficiently sharp to admit of delicate focusing. It was found better to focus on a sharp object at a moderate distance, and to find by calculation the correction for parallel rays. The correction to infinity δf for a distance u of the object focussed is given by

$$\delta f = f^2/u.$$

If, for instance, u were 1 kilometer, and f, as mentioned above, 160 cm., the correction would be 2·6 mm., and, as it proved impossible to adjust the focus more nearly than to about half a millimeter, an error of 20 per cent. in the estimate of the distance is allowable.

It is a matter of importance to be able to determine accurately the position of the corona in reference to the coordinates fixed in space. In Egypt and at Caroline Island this was done by means of a platinum wire stretched across the camera directly in front of the plate; the shadow of this wire appears on all photographs, and its position was determined by a succession of instantaneous photographs of the Sun's crescent taken directly before and after totality with a stationary telescope and at short intervals of time. This plan has some disadvantages. The shadow of the wire hides certain parts of the corona, and, for instance, in the photograph taken at Caroline Island the shadow is very awkwardly placed. Then, again, it is difficult to stretch the wire sufficiently tight. The camera has to be turned directly on the Sun while the crescent is being photographed, and the heat thus concentrated on the wire lengthens it and may change its position.

A different plan was therefore adopted. Two needles were placed one on each side of the camera, the line joining the points passing approximately through the centre of the plate; one of the needles was intentionally inserted somewhat obliquely, so that its shadow might always be recognised on the photographs. The image of the needles would thus completely determine the position of the plate in the camera. The direction of the line joining the needle-points can be very accurately determined with reference to the declination circle. We need only bring the image of some object, either terrestrial or celestial, just in coincidence with one needle-point, then with the other, and note the change of reading of the hour and declination circles. For safety, and as a check, photographs of the Sun's crescent might be taken after totality. The needles, in the present instance, did not reach sufficiently far into the centre of the camera. In previous eclipses the general illumination of the sky in the neighbourhood of the corona was sufficient to affect the whole plate; but it did not on this occasion, and the images of the needle-points only appear on one plate, owing to a cause to be mentioned presently. This one plate is, however, quite sufficient to fix satisfactorily the orientation of the corona. I believe that the method could be rendered perfectly safe in future eclipses, and it is certainly more accurate than that hitherto used.

2. *Arrangements during Totality.*

Dr. P. F. MACLEOD, the resident officer of health in Grenada, and Mr. MURRAY, who accompanied the expedition as a naturalist, had offered their assistance during totality. This was gratefully accepted, and my best thanks are due to them. Mr. MURRAY undertook to screen the camera while the slides were being drawn or pushed in, so as to prevent a confusion of image due to shaking when the slides are touched. Dr. MACLEOD took charge of the slides before and after exposure, handing them as they were wanted, and placing them in a bag after they had been taken out of the camera. I had originally intended to take eight photographs, but had to change all arrangements suddenly when I saw the corona obscured by a cloud at the beginning of totality. As the brighter parts of the corona were faintly visible, and as I did not know whether the cloud would clear away in time, two photographs were taken, referred to in Mr. WESLEY's report as Plates 1 and 2. The exposure of these must have been about 15 seconds each.

It is only in a very indirect way that I could, after the eclipse, obtain an idea of the various exposures given, and not much value is to be placed, therefore, on the numbers. I knew the time I intended to expose, but I generally closed the camera sooner, as I was always afraid of a fresh cloud coming on.

The corona came out clearly while the third exposure took place. Instead of a plate, a piece of sensitized paper had then been put into the slide, but the result was not good.

Three further plates were taken with the corona clear, the last about 15 seconds before the end of totality. After the spectroscopic cameras had also been shut, I intended to take out all slides. Unfortunately I loosened the wrong hook, and drew the shutter instead of the slide, but, noticing my mistake at once, pushed it back. That moment the Sun came out, and the last plate was partly spoilt, but parts of the corona show well, and it has been used in making out the details of the corona. The accident was fortunate, in so far as the image of the needles necessary for orientation came out very clearly owing to the illumination by the reappearing Sun.

3. *Results.*

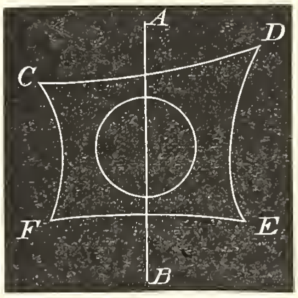
Mr. WESLEY, who has made a careful drawing of most of the recent eclipses, has described the corona as it is shown on the photographs on the present occasion, and I need not add anything to his remarks. But I should like to draw attention to one or two points which have a bearing on the speculations respecting the origin of the corona.

It is generally accepted now, I believe, that the Sun cannot have an atmosphere of anything like the extent of this luminous appendage. The reasoning generally given, and which seems conclusive, is that the pressure on the Sun's surface must, to judge from spectroscopic evidence, not exceed that on the surface of the Earth, and that an atmosphere comparable in size with the volume of the Sun would necessarily produce an enormous pressure. I do not know, however, whether attention has been drawn to the fact that, even if such a gaseous atmosphere existed, it could not be luminous to any great height unless it contained a great number of solid or liquid particles. The same laws of convective thermal equilibrium which regulate the decrease of temperature in the atmosphere act still more strongly on the surface of the Sun, and even taking the highest estimate which can reasonably be made of the Sun's temperature, the gases rising in the Solar atmosphere would quickly fall in temperature below the point at which they can be luminous. The only way in which they can be kept luminous is by containing sufficient matter to absorb the radiation from the body of the Sun, or by some independent cause, such as friction between solid and gaseous matter, or electric discharges.

If electric discharges are the cause of the luminosity, the matter through which the discharge takes place must either be supplied by the Sun or by something outside, such as streams of meteoric matter. The latter hypothesis has the great advantage that it may possibly account for the periodicity of Sun-spot phenomena, and it deserves, therefore, the attention of scientific men. Now, it occurred to me several years ago that if meteoric streams fixed in space were in any way connected with the corona we should find some evidence of them in the general shape as seen from the Earth. During eclipses which take place about the same periods of the year the Earth and the orbits of these hypothetical meteor streams would occupy the same relative positions. We might then expect some periodicity in the shape of the corona depending on the time of the year. To my surprise, I found that, as far as the evidence went, it seemed indeed to point in that direction. In the report of the Eclipse of 1875 a paragraph was inserted, which, without laying any stress on the point, called attention to the similarity between that eclipse and the one which had taken place the previous year during the same month. The eclipses which have taken place since have added to the evidence, and it seems worth while, therefore, to draw attention to it, without however in the least wishing to imply that I consider the fact as proved or even as probable.

The Solar corona often shows a rough symmetry about its axis of rotation, but deviates from complete symmetry owing to one of the halves being broader than the other. Hence, it often appears in the form of a trapezium. Fig. 1 may serve to illustrate this. AB is the axis; C, D, E, F the points of the longest rays of the corona, the distance DE being longer than the distance CF. Now, during the eclipses which have taken place early in April the eastern half of the corona is the one which is broadest, while in the eclipses observed in July and August the opposite held good and the western half was the broader. The eclipses of December and May have hitherto shown no difference between the two halves. It will no doubt be considered that the number of well ascertained coincidences is too slight to prove anything, and with this opinion I quite agree, especially as the changes in the corona which seem to depend on the Sun-spot cycle have to be taken into account. Nevertheless, it seems worth while to give the evidence here.

Fig. 1.



To begin with the eclipses of which we possess photographs, the only ones, perhaps, we can safely take into account, the following Table will illustrate my meaning :—

Date.	Year.	Corona.	Sun-spot numbers.
April 6	1875	Eastern half broader	20·5
May 17	1882	Absence of symmetry	60·0
May 6	1883	" "	63·7
July 29	1878	Western half broader	3·3
August 29	1886	" "	25·7
December 22 . . .	1870	No symmetry	135·4
December 11 . . .	1871	Symmetry, but both sides equally wide	98 0

The Sun-spot numbers represent frequency of spots as determined by Mr. R. Wolf. As far as 1878 these numbers are given in Mr. Ranyard's Eclipse volume ('Roy. Astron. Soc. Memoirs,' vol. 41) for the actual date of the eclipse. After that date the numbers are those given from time to time for the average of the whole year by Mr. R. Wolf in the 'Comptes Rendus.'

As regards the drawings, we have the following :—

Date.	Year.		Sun-spot numbers.
April 16 . .	1874	The drawings of Mr. Bright and Mr. Degermann show a distinctly greater width on the east side. In Miss Alice Hall's drawing the eastern half is less broken up than the western, but not broader. In Mr. Wright's drawing the points of the streamers on the eastern side are further apart than on the west	119·1
August 7 . .	1869	Observers speak of the trapezian form of the corona, which is important, as the Sun-spots, though not at their maximum, appear in greater number at that time than in either 1882 or 1883. The drawing of Mr. Meek, given by Mr. Schott, seems the only one that is oriented relative to the Sun, and here the western side of the Sun is the broad one	77·6
August 18 . .	1868	The drawings are too uncertain to come to any conclusion, but if lines are drawn giving the ends of the extreme rays, the rays on the west are in nearly all cases further apart	42·9

We have, therefore, not one case in which the eastern side was the broadest in the autumn or the western in the spring.

If we take the time at which the greater eastern width changes to the greater western width to be about the middle of June, and middle of December, we should expect that in the eclipse which has just taken place the two sides would be about equally developed, but if there is a slight asymmetry the eastern side should be the broadest.*

VI.—The Coronagraph. By Captain Darwin.

1. *Description of the Instrument and its Adjustments.*

The coronagraph was designed by Dr. Huggins as the instrument which would give the best chance of rendering it possible to obtain photographs of the corona in Sun light. For this purpose a reflector is to be preferred to a refractor, and special precautions are taken to avoid internally reflected light. The telescope is of the Newtonian form.

The instrument which I took out is by Grubb, of Dublin, and was mounted on an ordinary equatorial stand. The light enters a tube 4 feet long, fitted with numerous diaphragms. It then passes into a tube 5 feet 6 inches long (a little less than the focal length of the mirror), of about double the diameter of the first tube, the mirror

* [*Note added July* 14, 1889.—This prediction has not been verified. In the Eclipse of January 1, 1889, although there is not much difference between the two sides, the western half is the broader. Nevertheless, the considerations in the text are of some use, as they show the importance of taking account of the relative position of Sun and Earth in discussing the shape of the corona in different eclipses.]

being at the further end. The beam enters this tube at one side of the centre, and is reflected back at a small angle from the mirror at the further end to the photographic plate, which is alongside of the place where the light enters. A partition in this tube separates the incoming and reflected rays for as long a distance as possible. Below this partition there are oval diaphragms, through which both beams pass. I may here remark that this last precaution is, I rather think, a mistake ; for the backs of most of these diaphragms are necessarily visible from the photographic plate, and, to whatever extent they are useful in preventing stray light from getting to the bottom of the tube, they are proportionately harmful in reflecting this light directly back on to the photographic plate, which is very near to them. I was so much afraid of the direct Sun light striking the back of these oval diaphragms that I placed a temporary diaphragm of only 2 inches in diameter at the bottom of the outer tube, thus ensuring that the beam of Sun light should pass clear through the diaphragms in the inner tube. I should gladly have dispensed with this if I had dared.

The two tubes are held in position in a long wooden frame. There is a half-open space between the tubes, which has to be covered up by a cloth or other temporary contrivance. This opening should be provided with a permanent cover.

I arranged an instantaneous shutter close in front of the plate, that is to say, in the position which Dr. HUGGINS considered most advantageous. It consisted of a long wooden slide, with a rectangular opening 6 inches wide, which was drawn across an opening of $2\frac{3}{8}$ inches diameter by two pieces of elastic. It was released by cutting a thread. Both the elastics and the thread were fastened to the body of the telescope, and not to the camera or slide which takes the plate. This latter part is not too firmly fixed to the body, and I was anxious to avoid vibrations.

As Dr. SCHUSTER has fully described the adjustment of his instrument for equatorial movement, focus, &c., I will only remark that the same methods were used in the case of the coronagraph in so far as they were applicable.

2. *Observations in Sun Light and during the Partial Eclipse.*

The most important observations to be made with the coronagraph were with the view of testing the practicability of obtaining photographs of the corona during Sun light by this instrument. This could be done in two ways :—

(1.) By obtaining photographs shortly before or after the eclipse, and comparing any irregularity that might appear in the halo round the Sun with photographs of the corona taken during totality—a similarity of form indicating that the corona had been photographed.

(2.) By taking photographs during the partial eclipse. Then, if the light of the corona produced any effect on the plate, the dark limb of the Moon would be seen against it.

It was advisable in trying the first of the above experiments to take a series of photographs in each of the reversed positions of the instrument. Then any irregularity in the illumination round the Sun which was reversed on turning the instrument over would clearly be proved to be due to instrumental causes; whereas any irregularities which were visible in photographs taken in both positions, and which were not thus reversed, must be due to some outside causes, either atmospheric or coronal. In my instrument reversal was only possible near noon; and on this account, as well as on account of the Sun being at its greatest elevation, the best results would be obtained by taking one series of photographs shortly before noon in one position, and another series in the other position soon afterwards.

With regard to the length of exposure, I thought that there was no advantage to be gained for either test by giving a longer exposure than was necessary for the air glare to produce a result on the plate. I estimate the exposure that I gave with the automatic shutter at between one-fifth and one-tenth of a second.

The plates which I used for these tests consisted of gelatino-chloride dry plates on paper and on glass. These were prepared for me by two makers: by Mr. A. Cowan (for Messrs. Marion and Co.), to whom I am indebted for making for me a specially prepared batch of plates, and by Mr. Warnerke.

Chloride plates were chosen in preference to bromide plates, because it was considered that their relatively greater sensitiveness to ultra-violet rays would be advantageous for distinguishing between the corona and the air glare. The plate to be chosen is no doubt the one which is most sensitive in that part of the spectrum where there is the maximum difference of intensity of light in the spectra of the corona and of the sky. I may here remark that Dr. Schuster informs me that, according to his photograph, the maximum difference would appear to be in the less refrangible part of the spectrum, rather than in the violet; and, if that is the case, bromo-iodide plates would, I think, have been preferable.* Paper was considered better than glass, as tending to reduce the halation to a minimum; as a fact, I relied chiefly on the paper negatives.

I used various developers for these chloride plates, without observing any marked difference in the results:—

(1.) Hydrokinone, 1 grain to the ounce. To 2 oz. of the above add $\frac{1}{2}$ dr. of a saturated solution of potassic carbonate, with common salt as a restrainer.

(2.) Potassic citrate, 136 grs.; potassic oxalate, 44 grs.; water, 1 oz. To 3 parts of the above add 1 part of ferrous sulphate, 140 grs.; sulphuric acid, 1 drop; water, 1 oz. Potassic bromide was used as a restrainer.

(3.) A weak ferrous sulphate developer.

Some of the plates were developed in the West Indies, and some on my return home. The results appeared to be the same, but they were, I think, much easier to develop in England. Ice was used freely in Grenada, but I do not think the gelatine became quite hardened during the time of development, and the plates appeared to be more liable to fog.

* See p. 341.

3. *Results of First Test, or Photographs taken before the Eclipse.*

For the first test, that is, the one by means of photographs taken before or after the eclipse, I had intended to have taken several series, but fortune did not favour me. We reached Prickly Point on a Tuesday. By the following Sunday, although a great deal remained to be done in the way of final preparations and adjustments, I was able to take my first photograph. This left me six clear days before the eclipse in which to make the final arrangements, to obtain that experience so necessary when working in a new climate, and, in my case, with an instrument with which I was not very familiar, and also to obtain, if possible, one or more series of photographs for the test in question. I may here remark that, before starting, Dr. HUGGINS had given me every possible assistance and advice, without which I could hardly have undertaken the work; and that I had also spent several mornings in London in practising with the coronagraph, but from want of time I had to entrust a great deal of the troublesome preparation at home to Mr. LAWRANCE, to whose careful attention at this period I owe a great deal. As already remarked, the weather was very unfavourable, and on several days photographic work was an impossibility; on others I could only get casual photographs in the intervals between the frequent tropical showers, or in gaps in the cloudy sky. For two days after the eclipse the weather was also unfavourable. In fact it was only on the day before the eclipse that I succeeded in getting a series of photographs about noon in both positions; but, the rapidity of change in form of the corona being an unknown quantity, the shorter the interval the more valuable would the photographs be. The sky on that day was variable, but generally clear—as clear as it ever was during our stay in the West Indies, but not, I think, as clear as it often is in England.

Soon after coming home, Mr. WESLEY, who has had great experience with regard to photographs of a similar nature, very kindly undertook to examine this series. Several of the negatives exhibited corona-like markings, but Mr. WESLEY could not find any detail on any one of the photographs which was confirmed by the others. This was done before he had had an opportunity of seeing the photographs of the corona taken during totality. Had he succeeded in making a drawing of the supposed corona, its comparison with the true corona would have therefore afforded a valuable test as to its genuineness; under the circumstances this was, of course, impossible. Mr. WESLEY also informs me that he thinks that "the photographs taken on the same side are as little comparable with each other as with those taken on the opposite sides," which, as far as it goes, indicates that the irregularities are due to atmospheric rather than to instrumental causes. My own opinion on the above points coincides with that of Mr. WESLEY; but I was especially glad to obtain his assistance, because of his large experience, and also because he had not at that time seen any photograph of the corona.

4. *Results of Second Test, or Photographs taken during the Partial Eclipse.*

The second of the two tests could be applied by taking photographs either during the eclipse, before or after totality, or sufficiently near first or last contact for the Moon to be still eclipsing the corona. I succeeded in taking over twenty photographs during these periods. Many of these, when developed, showed what I may describe as a false corona, that is, an increase of density near the Sun, including the part between the cusps, and therefore in front of the Moon. In none of these can the limb of the Moon be seen against the corona as a background. Besides subsequent examination, I watched these photographs very carefully during development, without result. I mention this circumstance because they appear to have gone back considerably, so that, in several instances in which the air glare was clearly visible before fixing, it is now barely discernible.

I have also searched for any trace of a remarkably large prominence in the place where I knew it should be found, but without result. This prominence was hardly, if at all, covered by the Moon after totality, that is, during the period in which nearly all my photographs were obtained. Prominences are certainly more actinic than the corona, and we should therefore expect them to appear on the plate if the corona is obtainable by this method. However, if the air glare increases much more rapidly in intensity than the corona does as the Sun is approached, this argument is not sound, as the prominences might then be more overpowered by sky light than the outer parts of the corona would be.

5. *Photographs taken during Totality.*

It will be observed that the two experiments or tests just described were made by taking photographs before and after the eclipse, and during the partial phases. But during totality the instrument was not idle. The following was the programme which I had laid down for myself during the 3 minutes 50 seconds available :—

During the first minute a photograph was to be taken with the prismatic camera. After that, six plates were to be exposed with the coronagraph—four chloride plates with the same length of exposure as that given during Sun light, and two bromide plates with exposures of 5 and 10 seconds respectively.

This programme could not be followed exactly. Immediately after I had commenced exposing the prismatic camera, I looked up and found that a light cloud was drifting across the corona. The sky became clear again in about 50 seconds. I was anxious not to take any photographs with the coronagraph during the exposure of the prismatic camera, for fear of vibration; but, as nearly a minute had been lost, something had to be sacrificed, and I decided to take some of the photographs with the coronagraph before putting the cap on the prismatic camera. I do not think that the work has suffered in consequence, and at all events I obtained all the plates I had allowed for in my programme.

The instantaneous photographs of the corona were complete blanks, proving, I think, that the exposure had been far too short. I developed them with the same solutions, and for at least the same length of time, as when developing, immediately beforehand, some of the plates exposed during the partial eclipse; the instrument was in the same condition as before and after totality, when successful photographs were taken. These circumstances are worth mentioning, to show that I did not fall into some of the commoner traps with which the photographer is surrounded. Three of the photographs showed signs of fog, which was probably only due to the length of time which I allowed them to stay in the developer.

The long exposure photographs were not taken with any special object beyond that of obtaining a record of the corona. The plates used were bromo-gelatine dry plates prepared by Captain ABNEY, and I used the ordinary alkaline development. The extension of the corona shown on these plates is not very great, and they show signs of vibration; they have, however, I hope, been of use to Mr. WESLEY in his drawing of the inner parts of the corona. As my main object was to get instantaneous photographs, these plates had to be taken without removing the automatic shutter; the shutter had, therefore, to be worked by hand, and this probably caused the vibration. It may, however, have been caused by a puff of wind; and on another occasion I should take far greater precautions against this danger by surrounding myself with canvas screens in all exposed directions, and as high as possible.

6. *Comments and Conclusions.*

Returning again to the consideration of my observations with regard to the special uses of the coronagraph, it will be seen that my results are adverse to the possibility of obtaining photographs of the corona during Sun light with this instrument. It is, however, I consider, by no means proved that the method is impossible, for there are several reasons why this trial should not be considered conclusive.

(1.) The atmospheric conditions were very unfavourable. The air was fully charged with moisture, and on the morning of the eclipse the sky was certainly not of that dark blue which, no doubt, indicated atmospheric purity. It was slightly hazy, and not, I think, as clear as an average English blue sky. About a minute after totality I noticed a halo with prismatic colours round the Sun—an indication, I presume, of suspended matter.

(2.) The Sun was at a low elevation during the eclipse, and the station was only about a couple of hundred feet above the sea. Both these circumstances, no doubt, increased the air glare.

(3.) Professor THORPE'S observations at this eclipse show that the light from the corona was not so bright as on other occasions. This also appears to be the general impression amongst other observers who had seen previous eclipses. If this was due to the corona not being so luminous, the opportunity was, no doubt, an unfavourable one independently of the state of the atmosphere. But this effect is not to be distinguished from the light being diminished by an impure or dense atmosphere. Experiments comparing the light of the corona with Sun light immediately before and after the eclipse would be necessary to settle this point.

(4.) The exposure was too short. I feel confident that at Grenada an alteration in this respect would not have materially altered the result; but, even with the atmosphere in its unfavourable condition, I think a larger exposure should have been adopted. Before leaving England I had come to the conclusion that the right length of exposure would be that sufficient, and not much more than sufficient, for the air glare to produce an effect on the plate. I thought that the corona would be more readily visible if the background of the sky was faintly developed on the negative; and in this opinion I think Dr. HUGGINS would have agreed, as his photographs seem to have been treated in that way. I found that my instantaneous shutter, when set at its slowest rate, gave the required result, and I was at the time quite contented. I have, however, very carefully reconsidered this subject, and now come to the conclusion that this was a mistake. The question to be settled is—At what photographic density do we get the greatest ratio of small changes of shade in the negative, as seen by the eye, compared with the changes in luminosity of the objects photographed? Captain ABNEY gives some results in the form of curves in his Text-book of Science on Photography, which, if modified to suit this problem, would appear to indicate that the ratio is greatest when the photographic density is about one-third or half way between white and the deepest shade due to the full development of the image of a bright object.*

If this is the right interpretation—and I believe it to be so—then it is evident, in order to have the best chance of photographing the small difference of shade between the sky and the corona, I should have given an exposure which would have allowed me to bring up the sky to what I may call a third or a half full density. In order to have done this I should have lengthened my exposure.

Acting in accordance with the advice of Dr. HUGGINS, I adopted a slow development with weak solutions. I could not have followed a higher authority, but I must confess that I have some doubts whether a longer exposure with a quicker but well-restrained development might not have produced better results. The best time for watching for faint outlines is during development, and with a well-exposed plate we have an opportunity of observing the image in every stage.

All these considerations appear to me to point to longer and more varied exposures than I gave, together with a well-varied development.

My conclusions are, therefore:—

(1.) That my results do not prove the impossibility of photographing the corona in Sun light.

(2.) But they prove that under certain circumstances the light of the corona is not

* See p. 256. The ordinates of his curves represent the shade as measured by the proportion of black to white; the scale of shade as measured by the eye is somewhat different from this. The abscissæ represent the length of exposure, which I imagine to be equivalent to differences of luminosity.

sufficient to produce any effect on chloride dry plates with an exposure which is sufficient for the air glare or false corona.

Captain ABNEY pointed out to me—and I think correctly—that the sky or background to the Sun, as shown on the photographs, is due to two causes: first, the light of the sky, and, secondly, the reflected light from the interior of the instrument. Dispersion of light from the mirror or its surroundings would shed a uniform light over the whole surface of the exposed plate; whereas the sky is brightest near the Sun, and possibly not always quite uniform or regular in its appearance. Now, on several of the photographs that I took there is a perfectly uniform light over the whole sky, with no apparent increase of brightness near the limb of the Sun. This constitutes the only hopeful sign I have seen; because, if the above views are correct, it indicates that I have not given a sufficiently long exposure to photograph the sky round the Sun, and gives hopes that the false sky that I did obtain might be diminished by instrumental alterations.

7. *Discussion on the possibility of obtaining Photographs of the Corona during Sun Light.*

But there are certain considerations which appear to me to indicate that a practical method of photographing the corona during Sun light is not likely to be obtained.

If I am right in considering that the increased density round the Sun in these photographs is a true picture of the sky, and not due to irradiation or internal reflection, then it will be seen that this sky illumination is not uniform round the Sun, and that the form of the false corona thus formed would always be difficult to distinguish from the form of the true corona, and would be liable to be mistaken for it. It is quite possible that changes in the appearance of the sky may be more visible when photographed than when seen direct, and that these changes may be as sudden as that from blue sky to cloud. At high altitudes this difficulty would no doubt be lessened, but it would, I think, always be a source of trouble. If, however, these irregularities in the false corona are not due to irregularities in the sky, I can only say that the instrumental defect which causes them is a very difficult one to cure.

In considering the utility of the photographs of the corona taken in Sun light, if obtainable, the effect of the image being superimposed on the much denser picture of the air glare should be carefully considered. In the first place, it is to be observed that the air glare or light of the sky increases rapidly as the Sun is approached, and that the light of the corona also increases in a similar manner. This will, I think, cause the corona to be distinguished with difficulty in the photographs taken in Sun light. This may not be so much the case when considering abrupt changes of shade in the corona, but, in so far as the form of the corona is distinguished by a gradual change of light, it certainly will be. In the following figures, which have no numerical significance whatever, and which are merely intended to illustrate my argument, I have assumed the corona to be only from 10 to 15 times less bright than the air glare. In fig. 2, let the lines round the Sun represent lines of equal intensity of sky light, the corona being supposed to be non-existent. In fig. 3, let the Sun be eclipsed, and the lines represent lines of equal intensity of corona light. Place one image on the other, and the lines of equal intensity of the combined light of corona and air glare will be as shown in fig. 4, in which it will be seen how materially the form of the corona appears to be altered. It is to be noted also that an alteration in the state of the atmosphere by altering the ratio of air glare to corona light would appear to alter the form of the corona as shown on the Sun light photographs.

For the purposes of the following discussion of the effect of combining the sky and corona lights on one picture, it is assumed that this particular difficulty does not exist; that is to say, that the air glare is uniform, or, what comes to nearly the same thing, that we are discussing a radial inequality of the corona. In dealing numerically with this problem, very little trust must be placed in figures used; but, however faulty they may be, they may, at all events, help to make the consideration of the subject more easy.

Fig 2. Fig. 3. Fig. 4.

Captain ABNEY informs me his experiments with regard to sky light when compared with the results obtained by Professor THORPE at this eclipse show that at 30′ from the Sun's limb the corona, as seen at Grenada, was about 60 times less brilliant than the air glare at the same distance from the Sun's limb, as seen at South Kensington under the most favourable circumstances, that is, with the darkest sky measured. As the sky on this occasion at South Kensington was probably darker than the sky at Grenada, the ratio at this latter place was probably greater than 1 to 60; but, on the other hand, with a clearer sky the corona would have been brighter and the ratio therefore less. Hence the ratio of 1 to 60 is about half way between the ratio under favourable circumstances at South Kensington and the ratio at Grenada near the time of the eclipse. For the purposes of argument, let it be assumed that at Grenada the ratio of the light of the corona to the light of the sky at 30′ from the Sun's limb was as 1 to 50, a ratio, if the above figures are correct, no doubt considerably less than the true ratio.

Captain ABNEY also informs me that experiments he has made show that an abrupt change of $\frac{1}{2}$ per cent. in the density of a photograph is about the minimum change of shade that can be seen by the eye; or, in other words, that a photograph may be regarded as a drawing in which only 200 different shades can be used.

It has already been shown that to obtain the best results a photograph at a half or a third full density should be obtained. Let us assume the most favourable circumstances, and that the development has brought the part of the picture under consideration up to half full density. Then, if there are only 200 possible shades in any photograph, there are, therefore, only 100 possible shades below this shade, which for convenience may be represented by 100. Let the light which caused this shade be also represented by 100. Now, if the shade on the photograph varied proportionately with the light, each of the 100 parts of light would be represented by one shade on the photograph, and a change of light less than 1 per cent. would not produce a change of shade on the photograph which would be visible to the eye. But there is no doubt that under certain conditions the shade on these photographs varies proportionately more rapidly than the light which it represents, and, judging from Captain ABNEY'S curves, before mentioned, it is not quite a fanciful supposition to assume that under the most favourable circumstances it varies twice as rapidly in parts of the photograph; that is to say, that a change of $\frac{1}{2}$ per cent. in the luminosity of the object is the minimum change which would be visible on the photograph. Now, in a photograph taken in Sun light of the sky at 30′ from the Sun's limb, the 100 parts of light will consist of 98 of sky light and 2 of corona light. The sky light being assumed to be constant, the minimum visible change of $\frac{1}{2}$ per cent. in the total light must be due to a change of 25 per cent. in the total light of the corona; whereas, with a photograph taken during a total eclipse, on similar suppositions, a change of $\frac{1}{2}$ per cent. in the light of the corona will produce a visible change of shade on the photographs, or, in other words, the totality photographs will show 50 times as much detail as the Sun light photographs. It will be observed that, for the purposes of comparison of the photographs taken in Sun light and during totality, neither an error in the assumption of the shade to which the photographs were developed, nor in the assumption of the ratio of change of light to change of shade, nor in the number of possible shades in a photograph, would vitiate the result, as they would apply equally to both cases.

It therefore appears to me most probable that, under all circumstances, by whatever ratio the air glare is brighter than the corona, in very nearly the same ratio will the detail of the corona be obliterated in a photograph taken during Sun light, as compared with one taken during a total eclipse; that is to say, that, unless a change of shade in the corona were considerably more than 50 times as abrupt as the least change visible in the totality photographs at Grenada, it would be invisible in the photographs taken there in Sun light.

This naturally leads to the question—Are there abrupt changes of shade in the corona, or does the light diminish gradually as the distance from the Sun increases? As far as I have seen, an increase of exposure of the photographs taken during totality regularly brings with it an increased extension of the corona, as photographed; that is to say, that the light of the corona gradually diminishes in intensity from the Sun outwards. The detail of the inner parts of the corona soon gets obliterated as the exposure increases, which shows that this detail is due to delicate rather than to abrupt changes in intensity.

I conclude, therefore, that the effect of the light of the sky on photographs taken during Sun light, when the atmospheric conditions are at all comparable with those at Grenada, would be to entirely alter the form of the corona, in so far as that form is made visible by gradual changes of luminosity; and that, with regard to abrupt changes of luminosity, they would have to be exceedingly well marked before they would be visible in the Sun light photographs, whereas, as a rule, the form of the corona is indicated by somewhat delicate shading.

Thus, the possibility of photographing the corona in Sun light appears to depend on the extent to which at high altitudes, or at other localities, more favourable atmospheric conditions may be found. With clearer air, both the Sun and the corona will be brighter and the air glare less, but to what extent this change may be hoped for I do not know. It is to be observed, however, that besides air glare we have to deal with internal reflection, which increases with the increased brilliancy of the Sun. If my exposure, short as it was, was in reality sufficient for the internal reflection to produce an effect on the plate, with longer exposures and a brighter Sun, but under otherwise more favourable conditions, this might become a serious source of trouble.

The limb of the Moon has undoubtedly been seen for some time after totality (not at Grenada, where no doubt the atmospheric conditions were too unfavourable), and, if visible, it should, I think, be possible to photograph it. But we must be careful in using this as an argument in favour of the possibility of obtaining photographs of the corona in Sun light. In the first place, this phenomenon has generally been observed within a few minutes of totality. For example, Mr. S. P. LANGLEY, in 1878, at an elevation of 14,000 feet, was able to observe the limb of the Moon for 4 minutes 12 seconds after the reappearance of the Sun without taking any precautions to shield his eyes.* Mr. H. H. TURNER has kindly calculated for me that at this time only 0·068 of the Sun's surface was exposed; and, as the outer rim of the Sun has only about half the brilliancy of the central parts, it may be safely assumed that the total light emitted was not more than one-twentieth of full Sun light. If the air glare varies as the total light emitted, and if, as has been assumed for the purposes of argument, the ratio of air glare to corona were 50 to 1 in full Sun light, it would only be $2\frac{1}{2}$ to 1 under these circumstances. This would, of course, make the limb of the Moon much more readily visible against the corona. This argument does not apply to the cases where the limb of the Moon has been seen long before or after totality, but as long as the cusps are fairly sharp another circumstance must be considered. In all the photographs which I took during the partial eclipse in which there is a false corona, it will be seen that it is distributed round the mass of the Sun as a centre, and does not uniformly fringe its outline. The cusps often protrude out of it, and are quite free from it. Hence, the corona will be less overpowered by the air glare

* 'Nature,' November 18, 1886.

near the cusps than at other parts of the limb of the Sun. In the second place, in the case of the Moon being seen against the corona, we are dealing with a sudden change of shade, and not a gradual one, as in the case of the corona. This, as I have, I think, proved, would make it more easily photographed. It would have been encouraging to have obtained photographs of the limb of the Moon during the partial eclipse; but, unless they were obtained when a large part of the Sun was exposed, it would have done little towards proving the possibility of getting photographs of the corona in full Sun light.

It is to be observed that many of these remarks indicating the improbability of obtaining results by this method do not hold good in the case of exceptionally well marked features of the corona, where the changes of shade are really abrupt, and especially if they are radial in their direction. Thus, the first successes that we should expect to obtain would be in such cases as that of the "remarkably formed rift" in the corona of 1883, to which such a distinct likeness is reported to have been seen in photographs taken in Sun light by Dr. HUGGINS in England.

As for hoping to obtain any photographic records in Sun light of the outer limits of the corona as visible to the eye during totality, it is, I think, out of the question, as the following considerations will show, although the actual numbers quoted may be considerably in error. Judging from my own results, 10 seconds with a bromide plate would appear to be too small an exposure for the extreme limits. Assuming that the exposure I gave during Sun light was one-fifth of a second, and that the chloride plates are 30 times less sensitive than the bromide plates, then it would take 1500 times the exposure I gave during Sun light to photograph the outer parts of the corona during totality. On this point it is instructive to compare Mr. WESLEY's drawing from the photographs with Captain ARCHER's direct drawing, probably one of the most accurate ever taken.*

8. *Should the Experiment be Tried Again?*

In conclusion, the questions naturally arise: Is the experiment worth trying again, and, if so, under what conditions? As to the latter point, experience seems to show that:—

(1.) It should be tried at a station at great elevation above the sea.

(2.) It should be tried when the Sun is nearly vertical, during an eclipse of the corona by the Moon, Mercury, or Venus.

(3.) The exposure must be more varied and longer. At present I see no reason why increase of exposure should not simply increase the density of the air glare in the same proportion as that of the corona, thus gaining no advantage by the change.

Unless the air glare and internal reflection can be so diminished as to make an exposure of one second with an instrument like that used by me a possibility, I fear

* See Appendix to Captain ABNEY and Professor THORPE's paper on the Photometric Intensity of the Coronal Light during this eclipse (*infra*, p. 382).

no good results are to be expected. As to whether the experiment should be repeated, it is to be observed that the coronagraph is admirably adapted for taking photographs of the corona during totality, for ordinary records; for reflectors have several advantages over refractors for this purpose, and the special contrivances of this instrument would all be more or less useful under the circumstances. Hence, another trial can easily be combined with what may be described as the ordinary routine work of an eclipse. Atmospheric or other conditions are now certainly unfavourable, and time only can settle the question whether this is a permanent or a temporary difficulty.* I should therefore recommend the experiment being made again in connection with other work, but not before the next occasion when the path of a total eclipse passes over high ground (say, over 7000 feet) which can be conveniently reached, and where there appears to be a reasonable chance of having a clear atmosphere. But on all occasions it would, I think, be worth while arranging that some one, not necessarily a trained astronomer, should be detailed to observe for how long before and after totality the outer limb of the Moon can be observed. This was not done at Grenada. During partial eclipses a look-out should also be kept for this phenomenon.

Most of what I have said has been unfavourable to Dr. HUGGINS' method. But there is very material evidence in its favour, and, for the benefit of any one reading up this subject, I have given the necessary references in a footnote.†

VII. THE PRISMATIC CAMERA. BY CAPTAIN DARWIN.

1. *Description and Adjustments.*

The instrument was the same as that used by Dr. SCHUSTER, in Egypt, in 1882 (see 'Phil. Trans.' 1884, Part I.), and by Mr. LAWRANCE, at Caroline Island ('Phil. Trans.,' A, 1889).

The camera has a lens of 3 inches aperture, and of 20 inches focal length. There is no slit, and the prism, which is placed directly in front of the lens, has a refracting angle of 60°. A photograph taken with this instrument of any small object which has a bright line spectrum will show several images, each one corresponding to some

* It is a curious fact, that the limb of the Moon beyond the Sun has seldom been reported to have been observed long before totality during the last 15 years, although before that date it is said to have been seen on several occasions. See 'Astron. Soc. Mem.,' vol. 41, 1879, pp. 25–39.

† (1.) "On a Method of Photographing the Solar Corona without an Eclipse." W. HUGGINS, 'Roy. Soc. Proc.,' No. 223, 1882.

(2.) "On some Results of Photographing the Solar Corona without an Eclipse." W. HUGGINS, 'Brit. Assoc. Rep.,' 1883.

(3.) "On the Corona of the Sun."—Bakerian Lecture for 1885. W. HUGGINS, 'Roy. Soc. Proc.,' No. 239, 1885.

(4.) "Photographing the Corona without an Eclipse." E. L. TRONVELDT, 'Observatory,' No. 117 1886 line in the spectrum. Thus, in a photograph taken during totality, there will be several distinct images of each prominence, and, in measuring the distances between them, we are, in fact, measuring the distances between the corresponding lines in the spectrum of the prominence. A number of small prominences close together gives the appearance of a segment of a circle, while the continuous spectrum of the corona shows as a confused band across the plate. This will explain why it is difficult to obtain a clear interpretation of the various appearances shown in these photographs.

The accurate adjustment of the prismatic camera in its present form is difficult. In order that the photographic plate may be in focus for a large range of the spectrum, the camera is provided with a back which can be inclined to the optic axis of the instrument. In order to find the proper angle and position of the plate, Sun light which had passed through a collimator accurately adjusted for parallel rays was thrown into the instrument. The spectrum thus formed was focussed on the plate, and the instrument, therefore, adjusted for parallel rays entering in the direction of the optic axis of the collimator. The position of the blue and red end of the spectrum was then marked on the ground glass of the camera, and the collimator removed. If the instrument were then fixed on the equatorial, it would have been in focus, provided the camera were pointed relative to the Sun as it was before relative to the collimator. This was the awkward part of the adjustment; the camera was turned until the spectrum formed by the Sun occupied the position previously marked. The spectrum, of course, was blurred, owing to the finite size of the Sun, but the adjustment could be made approximately, and the result shows that fair definition was obtained. The slant of the plate essential for securing fair focus for rays of different refrangibility must be detrimental to the simultaneous focus of different parts of the Sun's circumference. The instrument can, therefore, never be a perfect one, but it affords valuable information in every eclipse in which a number of sharply defined prominences appear.

2. *Observations and Results.*

Only one photograph was taken during totality. The exposure was estimated at nearly two minutes, but for about fifty seconds the corona was nearly obscured by clouds. It has been explained how other photographs with another instrument, but on the same mounting, had to be taken at the same time as this one, and how it, therefore, came about that the plate shows signs of a shake.

The images of five prominences can be distinguished, but of these only two were measured, which will be called prominences Nos. I. and II. No. I. is evidently the large prominence on the north-west of the Sun which forms such a marked feature in some of the photographs. There are also two segments of rings in a different part of the plate.

The instrument used for measuring the distances apart of the images of the prominences was very kindly lent to me by the Royal Observatory, Greenwich. It was designed for measuring the Transit of Venus photographs, and consists of two parallel micrometers, rigidly fixed together and capable of sliding in one direction. With one micrometer the image is observed, and with the other the distance is read off on a glass scale.

Two of the images of all the prominences are especially dense, and from their position, and by reference to previous results, these can be recognised without much doubt as being due to the Solar H and K lines. Taking these as a basis, three other lines, h, H (γ), and F, can be recognised in the images of prominences I. and II. with some degree of certainty. The accuracy of this assumption can best be tested by seeing how nearly the wave-lengths of three of these five lines agree with the wave-lengths of three known lines, on the assumption that the two others are known. For this purpose F and h were taken as known with wave-lengths 4860·5 and 4101·2 respectively. Then the wave-lengths of the other lines were calculated on the assumption that the distances measured on the plate varied inversely as the square of the wave-length. This last assumption is, however, known to be inaccurate, and a correction had to be found and applied. This was done in the following manner:— A photograph of the Solar spectrum was obtained with the prismatic camera, but placing a slit and collimator in front of the prism. The distances apart of known lines of wave-length as near as possible the same as those believed to have been found in the spectrum of the prominences were measured; the wave-lengths of these lines were then calculated as before on the assumption that the same two lines were known. We, therefore, get in this Solar spectrum the known and the calculated wave-lengths of certain lines; the differences between the two give corrections which, if applied to the calculated wave-lengths of the lines of the prominences for the same part of the spectrum, should give their true wave-lengths. The results thus obtained are recorded in the following Table:—

Wave-length obtained as described above.		Order of intensity, both prominences.	Names and wave-lengths, known lines for comparison.
Prominence I.	Prominence I		
4860·5	**4860·5**	3	**4860·5** F
4471·0	. .	5	4471·0 f
4339·3	4340·3	3	4340·3 H (γ)
4101·2	**4101·2**	4	**4101·2** h
3966·3	3965·0	2	3968·5 H
3932·8	3930·5	1	3932·8 K
3890·0	3890·0	3	? 3887·0 H (ζ)

The three other prominences showed only in the H and K lines, and were not, therefore, measured.

It has already been explained how it came about that the instrument was shaken during exposure. The result of the shake is that there are two distinct sets of images, corresponding to two positions of the instrument. The second set of images is far more dense than the other, and was alone used in the measurements. The images in this case are lengthened out in one direction, either by vibration or through imperfect movement of the clock, and prominence No. I. presents a curious appearance in consequence. The image is wide, and is also separated into two parts by a band of white, giving the appearance of another shake, but a comparison with other photographs shows that it is in reality due to the peculiar formation of this prominence. In this instance this white band was used in measuring the distances of the images. The first set of images is much more faint than the other, and, as far as the prominences are concerned, is only visible as the H and K lines of No. I. prominence. The existence of two segments of circles has been mentioned; these were extremely puzzling at first, until it was noticed that they coincided in position with this at first unobserved faint set of images; that is to say, that if the circles had been completed they would have run through the bases of these prominences. No other rings corresponding to other lines can be seen. The H and K lines are in all cases by far the strongest, and it is, therefore, not surprising that these should be visible when the exposure is not sufficient to produce any others. But it is remarkable that, in the second or stronger set of images, the H and K lines of the same prominence should not have rings corresponding to them, for they are far more dense; that is to say, it would appear at first sight that the same object produced an image during a short exposure, and yet failed to produce one during a considerably longer exposure. It is probable from its position that this bright inner circle was covered by the Moon during the period when the one set of images was being formed, but was visible during the other exposure, which is thus presumed to be the first. It is also to be observed that the part of the limb where these rings occur was that which was least obliterated by clouds at the commencement of totality.

VIII. The Spectroscopic Cameras. By Arthur Schuster.

1. *Resolving Power of the Instruments.*

In order to form a clear idea of the amount of detail we may expect in a photograph of a spectrum, it is necessary to enter further than is generally done into the details of the construction and adjustment of the instruments used.

Lord Rayleigh has given the theory of the spectroscope for the case of a sufficiently narrow slit. It is easy to extend his results so as to apply also to the case of slits of which the width cannot be neglected. If R is the horizontal width of the beam entering the prisms (the refracting edge being supposed vertical), r that of the beam leaving the prisms, the equation $\delta i' = \mathrm{R}\,\delta i/r$ defines the angle through which the emergent beam is turned, owing to a small rotation δi of the incidental beam. If α is the width of the slit, f the focal length of the collimator, the geometrical image of the slit will subtend an angle Ra/fr at the centre of the lens of the telescope or camera, and to this we must add the width (λ/r) of the diffraction band of an indefinitely small slit in order to obtain the actual angular width of the image. The angle between two rays having a refractive index differing by $\delta\mu$, is on leaving the prisms, as shown by Lord RAYLEIGH, $t\delta\mu/r$, where t is the aggregate effective thickness of the prisms. This angle must be equal to the angular width of the slit in order that a double line may just be resolved. We then obtain $t\delta\mu = a\psi + \lambda$, when ψ stands for R/f and means the angle subtended by the useful horizontal aperture of the collimator at the slit. If $\delta\lambda$ is the difference in wave-length of the two lines, we find

$$\delta\lambda/\lambda = \frac{\partial\lambda}{\partial\mu}(\lambda + a\psi)/\lambda t.$$

In order to define the actual and possible power of a spectroscope, it is convenient to distinguish between the resolving power of a spectroscope and the purity of a spectrum, and to introduce the following definitions (see 'Encycl. Britann.,' "Spectroscopy") :—

The unit of resolving power of a spectroscope in any part of the spectrum is that resolving power which allows the separation of two lines differing by the thousandth part of their own wave-length.

The unit of purity of a spectrum is that purity which allows the separation of two lines differing by the thousandth part of their own wave-length.

We speak, therefore, of the resolving power of a spectroscope, which is a constant for each instrument, and of the purity of a spectrum, which differs according to the width of the slit. Both resolving power and purity vary inversely as $\delta\lambda/\lambda$, and are equal to unity if that fraction is equal to 10^{-3}. Hence, as from Lord RAYLEIGH'S investigation for narrow slits,

$$\delta\mu = \lambda/t,$$

or

$$\frac{\lambda}{\delta\lambda} = t\frac{\partial\mu}{\partial\lambda},$$

$$1000\,R = t\frac{\partial\mu}{\partial\lambda}.$$

Also, from the above equations,

$$1000\,P = t\lambda\frac{\partial\mu}{\partial\lambda}\Big/(\lambda + a\psi),$$

or

$$P = R\lambda/(\lambda + a\psi).$$

Here P stands for the purity, and R for the resolving power. They are both numerically equal if the slit a is indefinitely reduced. If the light is weak, narrowing the slit will not diminish the intensity until the width of the slit, as calculated from geometrical optics, is equal to the width of a diffraction band. In that case $\alpha\psi = \lambda$ and the purity is half the resolving power. Hence we shall, when light is of consideration, only be able to make use of half the resolving power of a spectroscope. Whether slits which are wider than the limit here given show the fainter lines on a photograph more distinctly owing to their increased width is a matter for investigation; but it would, perhaps, be well in a future eclipse to adopt that width of slit which makes $\alpha\psi = \lambda$.

Two spectroscopes were to be used on the present occasion, and the following numerical data will allow us to calculate their resolving power and the purity of the spectrum which they gave:—

Spectroscope I.

	cm.
Focal lens of condenser to form image of the corona on the slit	34·0
Focal lens of collimator	106·0
Aperture of ,,	6·0
Width of slit (α)	·003
Greatest thickness of prism	13·2
Focal length of camera lens	83·1

The beam was limited by the aperture of the collimator lens. As the full aperture of the collimator was used, we must put $\psi = 6/106$ and $\alpha\psi = 17 \times 10^{-5}$; hence, for a wave-length of 4×10^{-5}

$$\lambda/(\lambda + \alpha\psi) = 4/21.$$

With the width of slit used, we have, therefore, made use of the fifth part of the resolving power. To calculate the latter, we have, in the first place, to consider that the effective thickness of the prism used was only 11·4 cm., as the aperture of the collimator lens was not sufficient to use the full width of the prism. The dispersive power of the glass was sufficiently near to that for which Lord Rayleigh has calculated the value 1·02 cm. as the necessary thickness to resolve the sodium lines. As, according to our definition, a resolving power of ·98 is required to separate the sodium lines, we may take the prism to give a resolving power of 1 for each centimeter thickness in the neighbourhood of the sodium lines; it will vary inversely as the third power of the wave-length, and will, therefore, be 3·2 for each centimeter in the violet. We have also to reduce the resolving power in the ratio of 10 : 12, as the beam is limited by a circular aperture. We have, therefore, finally in the violet

$$R = 11{\cdot}4 \times 3{\cdot}24/1{\cdot}2 = 31,$$

$$P = 4 \times 31/21 = 6.$$

The purity was such, therefore, that near the wave-length, 4×10^{-5} two lines differing by the six-thousandth part of their own wave-length should be resolved.

Spectroscope II.

	cm.
Focal lens of condenser to form image of the corona on the slit	26·5
Focal lens of collimator for G	32·5
,, ,, telescope ,,	20·0
Aggregate thickness of two prisms	10·0
Width of slit (α)	·01

The beam was limited by the prisms, the aperture of the collimator and camera being more than sufficient to fill the prisms and transmit the beam.

The calculation is carried on as before, except that we must now take the useful instead of the full aperture of the collimator in order to find ψ. As the prisms were placed in minimum deviation for G, this can be calculated, and we find it to be 2·4 cm.; this determines

$$\alpha\psi = \frac{\cdot 01 \times 2\cdot 4}{32\cdot 5} = 74 \times 10^{-5},$$

$$\mathrm{P} = \mathrm{R}\lambda/(\lambda + \alpha\psi) = 2\mathrm{R}/39\,;$$

or, roughly speaking, the purity in this case was only the twentieth part of what it might have been if the slit had been narrower. The spectroscope was intended originally for the outer parts of the corona, and the slit was, therefore, intentionally kept rather wide. There was at the time no means of measuring the width of the slit, and I did not realise how much of the resolving power was lost by widening the slit. The full resolving power in the violet was 32, and therefore the purity realised 1·6.

2. *Adjustments of Instruments.*

In order to be able to differentiate between the spectra of different parts of the corona, its image must be thrown on the slit of the spectroscope; and for that purpose the plane of the slit must be placed in the focal plane of a condensing lens. This was done as follows: an auxiliary telescope was focussed for parallel rays, and the slit of the spectroscope looked at from behind through the condensing lens, which was moved backwards and forwards until a sharp image of the slit was seen in the telescope. The method is the same as that usually employed for adjusting the collimator of a spectroscope; only that, instead of observing the slit through the collimating lens, we observe it through the condensing lens.

The backs of the cameras employed could be tilted so as to have a larger portion of the spectrum in focus at once. It would be worth while to determine by calculation the best shape of the camera lenses in order that the foci of the different rays shall be as much as possible in a straight line. An achromatic lens is not as good for the purpose as an ordinary lens, but a combination might be found which would give better results than the ordinary lenses.

3. *Results.*

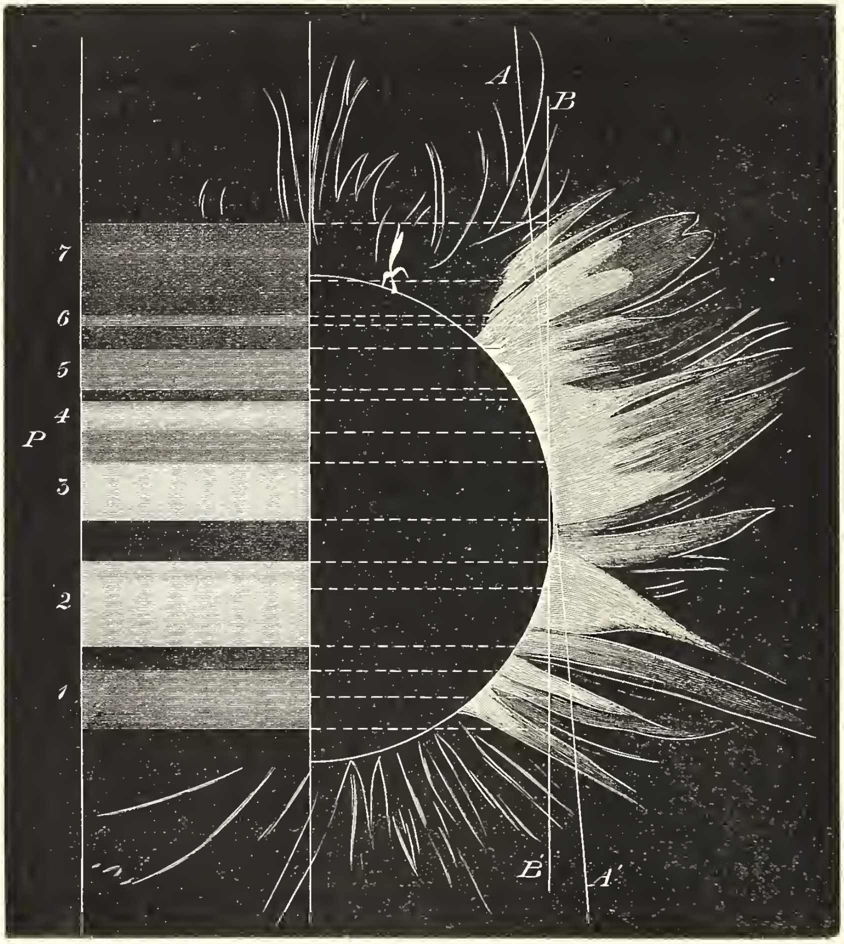
The general appearance of the spectrum of the corona, as shown on the plate exposed during the whole of totality in the spectroscope described above as No. II., is given in Plate 9, fig. 1. It has a streaky appearance, consisting of a series of bands stretching from the ultra-violet into the green, and is crossed by the bright lines of corona and prominences. The horizontal bands are parallel to the dust lines, but they must be chiefly due to the different brightness of the portions of the corona cut by the slit. This is shown by the fact that they are wider and more diffuse than the dust lines shown on the reference spectrum, and that we are able to recognise in the photograph of the corona itself the brighter portions cut by the slit. A few dust lines do actually appear, but can be easily distinguished. Fig. 5, p. 326, shows the section of the corona cut by the slit. The left-hand part of the drawing is a tracing of the streamers in the corona. The line AA′ is the line along which I had intended to place the slit. It is parallel to the declination circle, and tangential to the western limb of the Sun. It was, however, impossible to place the slit accurately in the position aimed at, and I think that, for reasons presently to be mentioned, the slit really ran along the line BB′. It must have crossed one of the prominences, for the prominence lines are very strongly marked, and this prominence will allow us to fix exactly the position of the image on the slit.

The right-hand part of the drawing gives the distribution of light and shade in the spectrum of the corona near the Solar line G reduced to the same scale as the outline of the corona. The images of the prominence appear along the line marked P. If we go from this line towards the southern side we find, in the first place, a strongly marked band, due no doubt to those parts of the corona which are closely adjacent to the Sun's limb. This band and the next are separated by an interval in which the photographic intensity is very small along the spectrum, and I believe that here the slit must to some extent have overlapped the image of the Moon.

Next comes a band, marked (2) in the drawings, in which all corona lines appear very strongly. Here the slit must have crossed the most intense streamer of the Solar corona, as will be seen by comparison with Mr. Wesley's drawing. The most southerly band (1) coincides with the section of another streamer. It was the light interval between (2) and (3) and the portion of this band (1) which induced me to fix on the line BB′ as the most probable line of the slit.

The part immediately above the prominence towards the north, which must be close to the body of the Sun, is comparatively light; this is partly due to a speck of dust on the slit, but the continuous spectrum of the corona here must have been weaker. Generally speaking, the spectrum all over the northern side is fainter than that towards the south. It will be noticed that the bands are nearer together in the violet than in the blue. This is due to the fact that the back of the camera was tilted in order to have the whole range of the spectrum as nearly in focus as possible. The magnifying power was, therefore, smaller in the violet than in the blue, and hence the tapering of the spectrum towards the more refrangible side.

Fig. 5.



The spectrum can be traced from a wave-length 4950 in the bluish-green to about 3700 in the ultra-violet. The maximum of actinic intensity of the coronal light was decidedly more towards the red end of the spectrum than that of Sun light. This shows that the continuous spectrum is principally due to incandescent matter of a lower temperature than that of the Sun. Although the polarisation of the corona and the appearance of Fraunhofer lines show that part of the light is due to scattering of minute particles, only the smaller part can be owing to this cause, as otherwise the maximum of actinic intensity in the spectrum would be displaced towards the violet and not towards the red end of the spectrum. The faintness of the Fraunhofer lines is further evidence in the same direction, and also makes it probable that there is not much matter round the Sun sufficiently large to scatter Sun light without polarising it.

If we now turn to the lines shown on the photographs, our attention is at once arrested by the H and K lines. They form in this, as in previous eclipses, the most intense feature of the corona. In the Eclipse of 1882, these lines were so bright that the atmosphere scattered enough of their light to make them appear over the disc of the Moon and at a considerable distance outside the corona. In the Eclipse of 1883, the same lines appeared reversed over the lunar disc; on that occasion, therefore, the light scattered by the atmosphere in front of the Moon must have been derived from some source showing absorption lines. In our photographs the same lines end sharply with the corona, and we must conclude, therefore, that, in spite of the unfavourable atmospheric conditions, there was but little light scattered by our own atmosphere in the neighbourhood of the Sun. This is confirmed by the photograph of the corona itself; for, while on previous occasions the sky light formed a bright background, on which the shadow of the wire stretched across the camera showed distinctly throughout the whole plate, the two needles do not show at all on our plates, even on those which had the longest exposure. The analysis of the light seen in front of the Moon, or in the neighbourhood of the corona, seems to give us important information on the general light emitted by it, for in three successive eclipses bright lines, dark lines, and no lines at all appeared on the photograph.

The prominence lines on the plate present a curious winged appearance towards the violet. At first sight it might seem that this is due to some defect in the spectroscope, such as a reflection on the inside of the slit; but, after careful consideration, I do not think that this is possible. One edge of the wing is perfectly sharp, and almost as black as the prominence line itself. The H and K lines in the corona seem perfectly sharp along their whole length, and do not show anywhere a trace of fuzziness such as would be produced by a defective slit. The reference spectrum shows the finest lines with perfect definition. The wing appears in all prominence lines, and, if it is not due to an instrumental defect, must be owing to a rapid motion of the matter forming the prominence. I find that an approaching motion of about 247 miles per second in part of the prominence would account for the displacement. According to YOUNG, such motions have been observed, though seldom. The limb of the Sun on which the prominence appears was receding.

Another fact of some importance is this, that, while the continuous spectrum on the northern hemisphere of the Sun is weaker than on the southern, the H and K lines are stronger. The hydrogen lines $H\gamma$ and h do not appear at all on the southern side, while I can, under favourable conditions, trace $H\gamma$ to some distance on the northern. It will be noticed from Mr. WESLEY's drawing that the side on which these lines appear is the side which was rich in prominences. A similar phenomenon was shown in the photograph taken in 1882, when the hydrogen and calcium lines appeared strong in the corona on that side on which they were strong on the edge of the Sun, presumably in a prominence.

The explanation which first suggests itself cannot be the true one. We might think that, if many prominences exist over one part of the Solar surface, the corona above, which undoubtedly does to some extent scatter the light falling on it, will show the prominence lines more strongly than other parts which have no protuberances near them. No bright line could, however, be produced in this way, for the light scattered by the solid and liquid particles of the corona must have the same composition as the direct Sun light which reaches the Earth. The particles are illuminated not only by the prominence light, but by the light from the whole Solar surface beneath them. If we imagine ourselves placed near the Sun, the spectrum there seen cannot differ on the average from that which we observe here, and therefore the scattered light must also be of the same composition, and must show dark lines on a bright background, and not bright lines.

Only one other explanation occurs to me. We must imagine that sufficient matter is actually thrown into the corona from the prominences to show the lines. This does not seem surprising if we look, for instance, at the large prominence on Mr. WESLEY's drawing. The life of such a prominence is short; had the eclipse taken place a few hours later, it might not have appeared as a prominence; but would it not have left behind it a sufficient quantity of calcium and hydrogen to show their characteristic lines? We need only consider the remarkable effects produced in our atmosphere by the Krakatoa eruption, and reflect that matter is projected into the Solar atmosphere on a vastly larger scale, to see that the coronal spectrum, as we observe it, may contain injected calcium, which only very gradually sinks back again into the Sun.

According to this view, we must separate the true coronal spectrum, which will be seen evenly all round the Sun, from the spectrum due to prominence matter, which will differ much in intensity during different eclipses.

The strongest of the true corona lines has, according to my measurements, a wave-length 4232·8. It is slightly less refrangible than the calcium line 4226·4 which forms a conspicuous feature in the Solar spectrum. That it is not identical with it is proved not only by the measurements, but by the fact that on the corona photograph the Fraunhofer line appears faintly by the side of the corona line. As the well known green corona line is always present in the spectrum of the chromosphere, it is very likely that this new line, which appears so strongly on our photographs, should frequently make its appearance there. YOUNG, in his excellent book on "The Sun," gives, indeed, a line 4233·0 as one of about 30 lines which appear there on "very slight provocation." It therefore seems highly probable that the two lines are identical, and that the comparatively weak Fraunhofer line 4233·0 is a corona line.

A faint Fraunhofer line has just been mentioned as appearing in the corona spectrum. The appearance of dark lines has been a source of considerable trouble in the reduction of the photographs. After all the measurements had been made, it struck me that some lines which I had put down as corona lines were really only the intervals between Fraunhofer lines, and I had to subject the photograph to a further careful examination. The effects produced by the overlapping of a spectrum of dark lines over one of bright lines is very complicated; especially, apparently, some of the weaker Fraunhofer lines can be traced, while some of the stronger ones do not make their appearance. I believe that this is due partly to the overlapping of bright and dark lines, but principally to an optical effect of contrast.

A bright line shows black on the negative and is bounded on both sides by an apparently lighter background. This is a well-known contrast effect. The H and K lines, for instance, seem to be surrounded by a lighter band, which follows the contour not only of the lines, but also of the wing by the side of the prominence. If, now, a Fraunhofer line happens to be by the side of a bright line, the contrast is strengthened, and both the bright and the dark lines appear more distinctly than they otherwise would. This is the only simple way in which I can explain some of the appearances of the photographs.

The triplet with a wave-length 4026·0, 4029·7, and 4036·8, which is represented in fig. 1, Plate 9, is a case in point; the group of Fraunhofer lines at 4031, about, is weaker than the strong Fraunhofer line 4045, and yet is much more easily visible on the eclipse photographs. This I believe to be due simply to the fact that the two lines 4029·7 and 4036·8 set off by contrast the intervening group of Fraunhofer lines.

Such contrast effects may, in some cases, have materially affected my measurements. I have made these as carefully as I could, and I believe that the great majority of the lines I have put down as corona lines are really such, but some of the weaker lines may be due to the optical effects I have just described.

Possibly the true spectrum of the corona may be still further complicated owing to the following cause:—The base of the corona gives us evidently a strong continuous spectrum, and it is possible that the lines of the outer corona may therefore appear as dark lines in a bright background. Captain ABNEY found some Fraunhofer lines reversed over the face of the Moon, while the G band was absent. The cause I have suggested may account for this. All these considerations show how very careful we must be in the interpretation of photographs of the coronal spectrum.

I had intended to have made a careful drawing of all I can see on the photographs. Figs. 2 and 3, Plate 9, are specimens of certain portions of the spectrum on a scale 40 times the original.* I had, however, to give up the work, as I found it too trying to the eyes. The length of spectrum represented in fig. 3 is in the original about 2 mm.

* The distances between the horizontal bands are magnified on a slightly different scale in the two drawings, so as to make them equal, while as before explained, the real distances decrease towards the violet.

By holding the photograph against a bright sky, and examining it with a lens of about half an inch focal length, I can see what I have tried to represent in the drawing. But I could only carry on the work about an hour at the time, and it was always followed by strong pain and neuralgia in the eye, lasting sometimes for several days. I am sorry, therefore, not to be able to accompany this report with a complete set of drawings. The two specimens which I can give will, however, give a fair idea of the appearance of the lines on the photograph. The band reaching from a wave-length 4318·4 to a wave-length 4323·7 has a curious shape. It is broad near the centre of the field, where it is widest, and there nearly covers the bright space on the least refrangible side of the G band, which comes out so strongly in photographs taken on a small scale. If it was of equal width throughout, I should have taken it for that band, coming out by contrast between the Fraunhofer line G and the group of lines at 4324. But the manner in which the line becomes thinner towards the outer parts of the corona shows that it must be a real band.

As we go from this band towards the less refrangible side, we come to very complicated markings until we reach the Hγ prominence. These markings I believe to be due to an overlapping of Fraunhofer lines and corona lines; whether the difference in the appearance on the northern or southern side is real or not I cannot be certain. The line at 4378·1 is the most conspicuous one in this part of the spectrum. By its side we can trace the Fraunhofer line 4383. The series of lines here seem to widen at the base of each bright band of the corona, and they are weak on the northern side.

I have not been able to trace with certainty a difference in the lines of the spectrum of different parts of the corona, except that already mentioned of the calcium and hydrogen lines. The group of lines at 4076 (fig. 3, Plate 9) at first sight looks as if there was such a difference; but we have here possibly only one broad band, and the lighter appearance in the centre may be a defect in the photograph, or it may be due to a Fraunhofer absorption line, which ought to be here, or, finally, to a reversal of a corona line against a hotter background.

The results obtained with Spectroscope I. are much less satisfactory. The plate had a very narrow escape during development. Owing to the hot damp weather the films were often found to detach themselves from the glass in the developing bath; but we had not found any difficulty when the bath was kept cool by ice, and when the film was soaked with alum. Captain DARWIN had kindly offered to do the whole of the photographic part of the work for me, and for this, as well as for continuous assistance in other ways, I have to offer my sincere thanks. The plate in question was developed during the afternoon of the eclipse. For some time nothing appeared, and when at last the image showed itself the plate at once began to frill at the edges. It was only by repeated treatment with ice and alum that Captain DARWIN saved the plate, but the image is very faint. What this is due to, I cannot say with certainty. The plate was one of Captain ABNEY's red end plates, and probably was less sensitive in the blue and violet than those used in the other spectroscope, which also were supplied by Captain Abney. The spectroscope was in good adjustment, and the reference spectrum taken with it on the day of the eclipse is perfectly sharp. If the development could have been carried further without danger, a better image might have been obtained. The plate shows in a very striking way the displacement of actinic intensity towards the red in the coronal spectrum. We can recognise on the plate the absorption of the G band and the strongest corona line, and thus fix the extension of the spectrum. The reference spectrum reaches from F into the ultra-violet, and is very strong between *h* and H. It shows, in addition, a band in the yellow or red, which, however, is very faint. The corona spectrum begins about F, is strongest between F and G, and falls off very rapidly in intensity about *h*. It can be barely traced between *h* and H. The lines shown on this photograph will be given in the annexed Table.

I must now describe the method adopted to determine the position of the lines which appear on the photographs. In 1882 a reference spectrum had been taken on the same plate previous to totality, but that reference spectrum was found to be of very little use in measuring the lines. It had to be near the edge of the plate in order not to interfere with the spectrum of the corona; and, owing to the curvature of the lines, we cannot directly compare the position of a line coming from the centre of the slit with one from near one of the edges. In order to compare accurately two spectra on the same plate, they must be in contact with each other, which would be impossible unless part of the coronal spectrum were sacrificed. A reference spectrum taken on a different plate is as useful as one taken on the same plate, if precautions are taken to fix well the position of the plate in the holder.

All good photographs of the corona which have been taken hitherto show the calcium lines coincident with H and K, and we can take these always as the starting point for our measurements. The distance of these lines from any unknown line will be the same in the centre of the reference spectrum as in the centre of the coronal spectrum, provided no shrinkage of the film has taken place during development. But, if a shrinkage has taken place, there is no reason to suppose that it is uniform all over the plate, and it may, therefore, be just as different in the reference spectrum and the coronal spectrum, if these are on the same plate, as if they are on different plates. Besides the H and K lines we can generally recognise some other feature in the coronal spectrum, consisting either of another prominence line or some well-marked Fraunhofer line; and in that case interpolation becomes easy with the help of the lines on the reference spectrum.

If we add to these considerations the danger of exposing to Sun light, or even day light, a plate which is to be used during the eclipse, merely for the sake of the reference spectrum, I think it will be conceded that I was justified in taking the reference spectrum on a separate plate. That this danger, which I had pointed out before the ,eclipse, is not imaginary, is shown by the failure of the Carriacou photographs.

In the present instance the hydrogen lines Hγ, *h*, and those in the ultra-violet, appear in the prominence spectrum as well as H and K, and they give us sufficient data to determine the wave-lengths of the lines which appear in the spectrum of the corona. The measurements were taken by means of a dividing machine reading to ·002 mm. The following Table gives in centimeters the distance of three of the hydrogen lines from H, as measured on the reference spectrum and on the spectrum taken during the eclipse.

	Reference spectrum.	Eclipse spectrum.
h	·3187	·3184
Hγ	·7943	·7930
F	1·5362	1·5382

h and Hγ appear as prominence lines, and their distances from H are practically the same on the two plates. About F there is more doubt. There is a faint mark on the eclipse spectrum which I had originally taken to be the image of the prominence which I expected to find there, but which may easily be an accidental spot on the photograph. The distance of this spot from H is 1·5450 or the tenth part of a millimetre further than F. The discrepancy seemed to me to be too great, and on examining the photograph again I found on the more refrangible side of the spot a distinct absorption line, which, when measured, gave 1·5382 as distance from H. Considering the faintness of the image near F, the measurement seemed now to agree, as well as could be expected, with the reference spectrum, on the supposition that F shows as an absorption line in the corona. I conclude, therefore, that the distance of a given line from H on the eclipse plate is the same as that of a corresponding line on the reference plate. In order to find the wave-lengths of the lines in the corona I proceeded as follows.

The distances of the following lines were carefully measured on the reference spectrum from H :—

	λ
Fe	4004·9
Fe	4045·1
h	4101·2
Ca	4226·4
Hγ	4340·1
Fe	4666·5
F	4860·7

Assuming the wave-lengths of F and of H to be given, we can, from the observed positions, calculate by some interpolation formula the wave-lengths of the intervening reference lines. The formula I used is the usual one

$$D\left(\frac{1}{\lambda_1^2} - \frac{1}{\lambda_x^2}\right) = \alpha\left(\frac{1}{\lambda_1^2} - \frac{1}{\lambda_2^2}\right),$$

where λ_1 and λ_2 are the two known wave-lengths, λ_x that to be determined. D is the distance measured on the plate between λ_1 and λ_2, α that between λ_1 and λ_x. The wave-lengths thus found are approximate only; but, by comparing the calculated values with those found in Ångström's map and given above, we can construct a table of corrections for the reference lines. A curve was drawn having the calulated wave-lengths as abscissæ and the corrections as ordinates. This curve was found to be quite regular in shape. The same interpolation formula was now used to calculate approximate wave-lengths for the corona lines, and the corrections were read off from the curve. I have found this combination of interpolation by calculation and by a graphical method to be very convenient. In order to show the accuracy which may be obtained in this way I give the following example :—

I left out originally the reference line 4666·5 altogether. There was, therefore, a very large gap between the lines F and Hγ. The curve had to be drawn, more or less, as a straight line between the points corresponding to the two lines. Treating the reference line, then, first like an ordinary corona line, that is, finding its approximate wave-length by calculation and the correction from the curve, I found 4667·0 as its wave-length, being very near the correct value. The curve was improved by making it still further agree with this line, and, as there is no gap of equal magnitude in other parts of the spectrum, I concluded that now the errors due to interpolation are negligible. It is more difficult to give an idea of the possible error due to faulty measurement. Most of the important lines were measured several times, and, from the way in which the wave-lengths agree, I should say that an error of 1·5 near F and 1 near H in X^{th} metres will not often occur in the stronger lines. The position of the weaker lines is, of course, more uncertain.

As the strong corona line 4232·8 had to be used as the starting point for the measurements of the second plate, considerable trouble was taken to find its position. Perhaps the best idea of the accuracy which can be reached in such measurements can be got from a comparison of the individual measurements. The following then are in centimetres, the distances from H of the corona line and of the calcium line 4226, as measured on the corona plate and on the reference spectrum respectively. The measurements were taken at different times during the last two years. Those marked* were taken by my assistant, Mr. Arthur Stanton.

Distance from H.

Corona line.	Calcium line.
·5920	·5810
·5920	·5810
·5950	·5752
·5930	·5780
·5930	·5790
·5920	*·5804
·5930	*·5816
·5930	*·5804
5902	·5810
·5910	·5824
·5930	·5822
*·5904	
Mean . ·5923	Mean . ·5802

Reduced to wave-lengths, the difference is 6·4, and, taking the wave-length of the calcium line as 4226·4, I arrive at the adopted wave-length 4232·8.

The plate taken with Spectroscope I. was more difficult to reduce. When the G absorption and the strong corona line had been recognised, the distances of the other lines were measured, and the corresponding wave-lengths found by help of the reference spectrum. All measurements of this plate were taken by Mr. STANTON, as my eyes could no longer stand the strain. The measurements are very difficult, as the band which has left a mark on the plate is exceedingly narrow, and it is sometimes impossible to distinguish accidental spots from real lines. Mr. STANTON has found a number of apparent absorption lines in the spectrum. Most of these agree with strong Fraunhofer lines, and this justifies the belief that his measurements are substantially correct as giving the positions of certain markings on the plates; but it is quite possible that specks of dust or other accidental marks have occasionally been taken for corona lines.

The following Table gives a list of the lines which have been measured on the two plates. For reference, the corona lines observed in 1882 and 1883 have been added. A query is attached to those wave-lengths taken in 1883 which have not been included in Captain ABNEY'S final list. The lines which are printed in thicker type are those I have measured repeatedly, and about which I can speak therefore with greater confidence; but all lines given have been measured at least twice. In the column headed intensity—(6) represents the highest intensity, and (1) the lowest. The intensities refer to the appearance of the lines in the centre of the spectrum, that is, on the prominence line. The numbers given are, of course, very approximate only, as the judgment about intensities much depends on the fatigue of the eye and other circumstances. Down to intensity (3) the lines are easily visible; (2) means more difficult to see, but not doubtful; the lines marked (1) are doubtful.

	Spectroscope II. Tangential slit.	Intensity.	Spectroscope I. Radial slit.	Eclipse 1882.	Eclipse 1883.	Remarks.
1	3923·9	2				
2	**3933·0**	6	..	3933	3934	K.
3	3942·3	2				
4	3950·1	2	..	3948		
5	3958·8	2				
6	**3968·1**	6	..	3968	3969	H.
7	3975·4	3				
8	3979·7	1				
9	3985·0	1	3936	..	3986 ?	
10	3990·0	2	..	3992		
11	3998·4	2	4003	..	..	An absorption line shown here in Spectroscope I., 3997, reversed across Moon's disc in 1883.
12	4004·0	1	...	..	..	
13	4009·7	3	4008			
				4015	4016	Strong in 1882.
14	**4026·0**	3	..	..	..	There is a group of Fraunhofer lines in the Solar spectrum about 4031, which is apparently visible between these lines; hence the actual positions of the lines are a little uncertain.
15	**4029·7**	4	..	..	4031 ?	
16	**4036·8**	4	..	..	4037 ?	
17	..	..	4047	4044	4045 ?	Apparently double in Spectroscope I.
18	4054·8	2	4050			
19	**4056·7**	3	4054	4057	4056	
20	**4063·5**	3	..	..	4064	The centre line shows only in the outer parts; possibly covered by Fraunhofer line 4071 in the centre. This line reversed across Moon's disc in 1863; possibly a broad line showing reversal in centre.
21	**4067·7**	1	4071	4067	..	
22	**4075·7**	3	..	..	4075	
23	**4084·2**	4	4087	4085	4085	4084 extends outwards in Spectroscope I. The two lines 4084 and 4089 are the most striking objects in this part, and look like band fading towards red (see fig. 3, Plate 9).
24	**4089·3**	4	4090			
25	**4099·7**	2				
h	4101·2	..	..	4101	..	*h* prominence line, presence in corona towards North uncertain; not visible in corona towards the South.
26	4105·2	2				
27	4112·5	2				
28	4122·7	2				
29	4128·0	1				
30	4138·0	2	..	..	..	There was a group of lines here in 1883, but the measurements do not seem to agree very well. Spectroscope I. appears to show an absorption line at 4143.
31	4144·2	2	..	..	..	
32	4148·9	4	..	..	..	
33	4156·2	3	..	..	..	
34	4165·0	3	4163			
35	4169·7	3	4166	4168	4169	
36	4173·6	2	..	4173 (short)	..	Absorption line shown in Spectroscope I. at 4180.
37	4177·3	3	..	4179		
38	**4183·5**	3	..	..	4185	

	Spectroscope II. Tangential slit.	Intensity.	Spectroscope I. Radial slit.	Eclipse 1882.	Eclipse 1883.	Remarks.
39	4189·2	3				
40	4195·0	3	4198	4195	4192	Extends outwards in Spectroscope I.
41	4203·5	3	..	..	..	Shows as absorption line in Spectroscope I.
42	4211·8	3	4212	4212	4213	
43	4216·5	3				
44	4222·6	3				
45	4232·8	5	4233	4224	4227	
46	4237·9 ?	1	..	..	..	Doubtful.
47	4241·0	4	..	4241	..	Shows as absorption line in Spectroscope I.
48	4247·2	4	..	..	4248 ?	There was evidently a group of lines here in 1883, but the measurements given in the Report of that eclipse do not agree very well at this point.
49	4253·6	3	4251	4252	4255	
50	4258·5	3	..	..	..	
51	4268·5	3	..	4267	..	
52	4275·5	2	..	..	..	
53	4280·6	4	..	..	4279	
54	4286·3	3	..	4289	4291	
55	4293·9	2	4293	..	..	These lines are probably only the intervals between the groups of lines which make up the G band.
56	4301·0	2	..	..	..	
57	4305·5	2	..	..	..	
58	4310·2	1	..	..	..	
59	4318·4 4323·7	4 4	.. ..	.. ..	.. ..	A band broad in the centre and narrowing in the outer parts of the corona.
60	4332·1 4339·7	.. ..	.. ..	.. ..	.. ..	The photograph here shows complicated markings (see fig. 2, Plate 9). The radial spectroscope shows an apparent absorption at 4336.
61	4346·9	2	..	..	..	This line is strongly marked in the most southerly band, but this is possibly accidental.
62	4354·7	1	..	..	4353	
63	4365·4	3	..	..	..	Triplet very clearly marked in bands (2) and (3). The line 4378·1 is very conspicuous. 4370 is marked short and winged in 1882.
64	4372·2	3	..	4370	4370	
65	4378·1	4	..	..	4377 ?	
			4389	..	..	Extending outwards as an absorption. There is a strong Fraunhofer line at 4384.
66	4387·6	2				
67	4395·8	2	..	4395	..	This line shows strong in band (1).
68	4402·2	3	..	4401	4400 ?	These bands present a curious appearance in Spectroscope II., being very broad near the middle and weak. 4422 and 4427 appear strong in Spectroscope I. 4401 was short in 1882.
69	4411·3	2	..	4414	..	
70	4418·3	1	4422	..	..	
71	4427·5	3	4427	..	4427 ?	4427·5 seems rather stronger in the northern part of the corona.
72	4434·8	2				
73	4445·8	1	..	4442	4449 ?	
74	4452·9	3				
75	4460·7	2				
76	4468·5	3	..	..	4465 ?	

	Spectroscope II. Tangential slit.	Intensity.	Spectroscope I. Radial slit.	Eclipse 1882.	Eclipse 1883.	Remarks.
77	4474·4	2	..	4473	4473	
78	4482·7	2	4481	..	..	Extending outwards in Spectroscope I.
79	**4485·6**	3				
80	4493·4	1	4491	..	..	Extends outwards in Spectroscope I.
81	4498·5	3				
82	4505·4	2	..	4501	4501	Marked double in 1882.
83	4508·9	2				
84	4515·6	2	..	..	4518 ?	
85	4520·7	1	4520	4526		
86	4530·0	1	4527	..	..	Shows as an absorption line in Spectroscope I.
87	4536·1	2				
88	4541·3	1				
89	4547·8	2	4550	..	4546 ?	This and the succeeding two lines are very broad at the base of each horizontal band.
90	4557·2	2	4563	..	4555 ± 3 ?	Marked in Spectroscope I. as the beginning of a band.
91	4570·2	2	..	..	4571 ?	The continuous spectrum here is very strong, and the line difficult to measure.
92	4579·7	1	..	..	4577 ?	
93	4588·8	1	4586			
94	4596·2	2	4593	..	..	Extends outwards in Spectroscope I.
95	4605·9	2	..	..	4604	
96	4616·9	2				
97	4621·7	1	4620	..	4620	
98	4627·9	4	4631	..	4636	
99	4644·0	3	..	..	4642	
100	4652·1	3	4652			
101	4657·9	3				
102	4667·6	1				
103	4678·9	2	..	..	4673	
104	4686·0	1	..	..	..	Broad.
105	4696·0	..	..	..	4695	
106	4708·0	..	..	..	4706	
107	4725·0	..	..	4721	4717	
108	4733·0	..	..	..	4730	
109	4746·0	..	..	..	4738	
110	4796·0	..	..	..	..	Very uncertain.
111	4881·0	..	..	..	..	
112	4834·0	..	..	..	..	
113	4877·0	..	..	..	..	
114	4891·0	..	..	..	..	
						End of visible spectrum at 4948.

If we compare together the measurements taken during different eclipses, we find that the agreements are good, especially those taken with Spectroscope II. in 1886 compared with those taken in 1883. The resolving powers in the two cases were about equal, and double that used in 1882. Nearly all lines found in 1883 have a representation of intensity (3), or above in 1886. Our data as yet are insufficient to judge whether there is any great difference in the spectra during different eclipses. The slit in 1883 was radial; that in 1886 was placed tangential to the Sun's limb, and this would account for some difference in the relative intensities of lines, for some of the lines in the radial spectroscope fade undoubtedly more rapidly as we go outwards in the corona than others. A number of lines are wide in those parts of the corona which show the continuous spectrum, strongly thinning out where that spectrum is weak. Nevertheless, I cannot help thinking that, if the line 4232·8 had been as strongly marked in previous eclipses as it has been in this, it would have attracted special notice. On the other hand, a line 4015, which was marked strong in 1882, and which was present in 1883, does not appear in our photographs. There is, therefore, some ground to believe that the spectrum of the corona differs much on different occasions, although we want further evidence to settle the point definitely.

I have collected in the following Table the most persistent lines in the spectrum of the corona; that is to say, those which appear in all three eclipses.

1886.	1883.	1882.
4056·7	4057	4056
4084·2	4085	4085
4169·7	4169	4168
4195·0	4195	4192
4211·8	4213	4212
4232·8	4227	4224
4253·6	4255	4252
4372·2	4370	4370
4402·2	4401	4400
4474·4	4473	4473
4498·5 } 4505·4 }	4501	4501 (double)
4725	4718	4721

It seems curious that the discrepancy is greatest for our strongest corona line. The line 4227 in 1883 may have been the calcium line. That our corona line is less refrangible is proved not only by the measurements which are given above in detail, but can be shown by simply placing the corona spectrum film to film against the reference spectrum. If the H lines are made to coincide, the corona line is shown distinctly to be by the side of and not above the Fraunhofer line belonging to calcium.

For the greater part of these lines the agreement is as good as can be expected.

Amongst the stronger lines the following have been observed in 1886 and 1883, but not in 1882 :—

1886.	1883.
4029·7	4031
4036·8	4037
4063·5	4064
4075·7	4075
4183·5	4185
4247·2	4248
4280·6	4279
4378·1	4377
4427·5	4427
4468·5	4465
4627·9	4636
4547·8	4546
4557·2	4555 $\pm$ 3
4570·2	4571

Here again the agreement is good for the greater number of the lines.

If we endeavour to trace coincidences between the corona lines and the lines of known elements, we meet with serious difficulties. Owing to the multitude of lines, accidental agreements will be frequent, and no certain conclusions can be drawn unless we can trace a number of coincidences, or, at any rate, discover some group repeated with all its characteristic features. Nor must we forget that the green corona line, which, before the Egyptian Eclipse, was the only known corona line, has no representative in the spectrum of terrestrial substances, and the same holds for a large number of the lines occurring most frequently in the Solar chromosphere.

It should, therefore, not surprise us if we cannot recognise any of our elements in the corona.

I have been much puzzled, however, by a series of coincidences, which, although I have finally come to the conclusion that they are accidental, yet seem to deserve being mentioned in this report. If we look at photographs of the line spectrum of nitrogen, the most striking features in the violet are as follows :—

Nitrogen.		Corona.	Manganese.
1. A very strong line at	3994·5		
2. A triplet, the least refrangible being the strongest	4025·3	4026·0	4029·5
	4034·4	4029·7	4031·8
	4041·7	4036·8	4040·6
3. A triplet, in which the two least refrangible are the strongest	4228·9	4232·8	4227·0
	4236·4	4237·9 ?	4234·6
	4240·6	4241·0	
4. A group of lines, having its two strongest lines at	4628·9	4627·9	
	4641·2	4644·0	

Now almost the three strongest corona groups occur exactly at the same places as the groups 2, 3, and 4. When we compare the individual lines within each group, however, the agreement is not so satisfactory, and the defective coincidence in group 3 argues strongly against its reality. The strongest corona line is at 4232·8, falling in the middle between the two nitrogen lines; but the line 4228 is really a weak nitrogen line, and while the other two are about equally strong and so broad, at any rate at atmospheric pressure, that they would not be separated on our photographs, but show as a broad band with 4239 as centre, I conclude that the strongest corona line can have nothing to do with nitrogen; and this makes the other coincidences very doubtful, especially as the strongest of all violet nitrogen lines at 3994·5 has no representative in the corona spectrum. Nevertheless, I think it would be worth while to look for the strong green nitrogen line in the spectrum of the corona on the next occasion. I have also examined the spectrum of oxygen, finding a number of curious coincidences which are also very likely accidental only.

Finally, there are some of the corona lines which seem to lie very near lines of manganese. As Mr. LOCKYER has shown that the spectrum of manganese plays an important part in cosmical spectroscopy, the coincidences deserve careful consideration. I have, therefore, added a column for the manganese lines in the above Table. The strongest manganese lines in the violet are 4235·0 and 4230·7; the mean of these two numbers is 4232·85, or exactly at the place at which the strongest corona line is placed. I think that our opinion as to the presence of manganese in the spectrum of the corona must depend on the question whether the two manganese lines could possibly, with the instruments used, look as a single line. My own opinion is against such a view. Our photograph resolves lines which are quite as close or even closer together than the two lines in question, which are quite sharp. The corona line on our plate has a certain width filling a space between 4231 and 4235·0, that is to say, it fills exactly the two spaces between the two manganese lines, but does not overlap them. It is strongest near its centre. From the width of the slit used, I calculate that an indefinitely thin line would, in this part of the spectrum, cover a space of about 2·6 units, so that the manganese lines should be separate, and reach respectively from 4228·4 to 4232·0, and from 4233·7 to 4236·3. The corona line should, therefore, show very decided signs of duplicity, and I cannot reconcile its actual appearance with the supposition that it is the representative of the two manganese lines. For the present, then, our attempt to identify corona lines has only led to negative results.

4. *Summary of Results and Suggestions.*

In conclusion, I give a summary of the principal results of the spectroscopic camera, together with a few suggestions which may prove useful to future eclipse observers.

Summary of Results.

1. The continuous spectrum of the corona has the maximum of actinic intensity displaced considerably towards the red, when compared with the spectrum of Sun light. This proves that it can only in small part be due to light scattered by small particles.

2. While on the two previous occasions on which photographs of the spectrum were obtained lines showed themselves outside the limits of the corona, this was not the case on this occasion. Hence there must have been less light, due to the scattering in our atmosphere.

3. Calcium and hydrogen do *not* form part of the normal spectrum of the corona. The hydrogen lines are visible only in the parts overlying strong prominences; the H and K lines of calcium, though visible everywhere, are stronger on that side of the corona which has many prominences at its base.

4. The strongest corona line on the present occasion was at $\lambda = 4232 \cdot 8$; this is probably the same line as 4233·0 often observed by YOUNG in the chromosphere.

5. Of the other strong lines, the positions of the following seem pretty well established:—

4056·7,	**4084·2**,	4089·3,	**4169·7**,	**4195·0**,	**4211·8**,
4280·6,	4365·4,	**4372·2**,	4378·1,	4485·6,	4627·9.

The lines printed in thicker type have been observed also at the Caroline Island and Egyptian Eclipses.

6. A comparison between the lines of the corona and the lines of terrestrial elements has led to negative results.

Suggestions concerning the Spectroscopic Arrangements in future Eclipses.

1. In order to distinguish better any difference in the spectra between different parts of the corona, a larger image should be thrown on the slit. A lens of 4 or even 5 feet focal length might be employed with advantage. The aperture of the lens need not be larger than that required to fill the collimator with light.

2. The width of slit should be equal to $f\lambda/R$, where R is the useful aperture of the collimator lens, λ the wave-length, and f the focal length of the collimator. In order to prevent difficulties, due to dark lines, &c., f should be about 4 or 5 feet.

3. A resolving power of about 12 in the yellow, if full use is made of it, seems sufficient. This can be obtained either by one large prism or two small ones.

IX. Photographic Results obtained at Carriacou Island.
By E. W. Maunder.

The work allotted me in the observation of the eclipse was purely photographic, and was intended to be, in its general character, a duplication of that undertaken by Dr. Schuster. The photographs which I was to take were to be both of the corona itself and of its spectrum. For the former I was provided with a lens of about $4\frac{1}{4}$ inches aperture, corrected for the photographic rays, and having a focal length of about 5 feet. The diameter of the image of the Moon on the photographs, which were taken in the primary focus, was therefore about six-tenths of an inch. For the photographs of the spectrum I had two spectroscopes, the second of which was only provided immediately before the instruments were packed up for shipment. The first spectroscope had two prisms, each 1·75 inch in height and 2·5 inches in base, and with refracting angle of 62°; and it was used in conjunction with a condensing lens of 3·5 inches aperture, and focal length of 17·5 inches. The second spectroscope had one prism, 2·6 inches both in height and base, and with refracting angle of 60°; the condensing lens used to throw an image of the Sun on the slit of this spectroscope was 3 inches in aperture, and had a focal length of 14·5 inches. These three instruments were all attached to the same frame, which was mounted equatorially and supplied with clock-work. The polar and declination axes and the R. A. and declination circles were those of the Corbett Equatorial of the Royal Observatory, Greenwich; the driving-clock also belonged to the same instrument, but the stand to which these were attached was made specially for the expedition. It was a tripod stand, composed of pieces of angle iron bolted together, and was found to be light and portable, and at the same time strong and steady. In addition to the camera and spectroscopes, a telescope of 3·6 inches aperture and 5 feet focal length, together with its finder, was mounted on the same stand; and a lens of 1 inch aperture and 4 feet focal length, with a little screen in its primary focus, was attached to the side of the coronal camera as a finder.

Owing to a delay in the selection of the equatorial mounting to be assigned to my use, and to the fact that the second spectroscope was added to the equipment as an afterthought, the preparation of the entire instrument was thrown so late that it was completed only just in time to be packed for shipment, and I had no opportunity, even for a moment, to test its performance as a whole, or of the different parts separately, except the coronal camera, until my arrival at Carriacou. On setting up the instrument there, it was at once seen that the driving-clock drove in the wrong direction, the Corbett equatorial having been last used in the Southern hemisphere. By the help of one of the artificers of the "Bullfrog" this was altered, and the driving of the clock rendered fairly good. Its actual rate was not determined, as the necessary alterations were not completed until the day before the eclipse. But the gearing of the hour circle into the driving-screw of the clock remained loose and unsatisfactory, and it was not found possible to remedy it. The spectroscopes, too, when put together, proved to require many alterations in small details, which cost the most unceasing labour, but which were all effected very satisfactorily before the day of the eclipse.

The programme which I was to carry out with the above instruments comprised the taking of one photograph of the spectrum of the corona during totality with each of the two spectroscopes, and of seven photographs of the corona itself with the five-foot coronal camera. The slit of the two-prism spectroscope was to be adjusted so as to point east and west, and to lie across the centre of the Sun; the slit of the single-prism spectroscope was to lie north and south, and to form a tangent to the east limb of the Sun.

The expedition reached Grenada on the afternoon of Thursday, August 12, and on the afternoon of the following day the huts which had been made for the Rev. S. J. PERRY and myself were taken on board H.M.S. "Bullfrog," which left St. George's at daybreak on August 14, in order to convey us to Carriacou, an island some twenty miles north of Grenada, and the largest member of the chain of islets known as the Grenadines. We anchored in Grand Ance Bay, off Hillsborough, the principal village of the island, and made the necessary examination of the country near, and inquiries respecting other portions of the island, in order to be able to select the most suitable site for our observing station. As there were no safe anchorages for the gunboat on the east of the island, and as Grand Ance Bay was on the west of the island, in its broadest part, and was, moreover, surrounded by steep and lofty hills, we were compelled to fix upon Tyrrell Bay, a little further to the south, where the island was much narrower, and where the hills were only about 200 feet in height. A position was finally selected on the top of the ridge, from whence a good eastern horizon was obtained. The site chosen was about 300 yards to the north of a small house known as "The Hermitage," the residence of Mr. DRUMMOND, and its approximate position as given by the Admiralty chart was W. long. 61° 29′, and N. lat. 12° 27′.

A heavy rain storm on August 16—the fringe of the tornado which wrecked the neighbouring Island of St. Vincent—rendered the steep slopes of the hill so muddy and sodden that the work of conveying the huts and instruments to the summit proved a lengthy and laborious task, and the huts were not finally completed and the instruments erected until Friday, August 20. The eight days still remaining before the eclipse were then devoted to the necessary adjustments of the various instruments and the alteration of the driving clock. The work was continually interrupted by short showers and passing clouds, but all the cameras were brought into good focus, the several telescopes and condensing lenses all rendered truly parallel, and the elevation and azimuth of the mounting ascertained to be correct within a minute of arc—the circles with which the instrument was provided being divided to half degrees, but reading to minutes by means of a vernier—before the day of the eclipse.

The entire programme of the eclipse had also been rehearsed on two occasions, viz., the mornings of Thursday and Saturday, August 26 and 28. Friday, August 27, was wet.

On the day of the eclipse the Sun rose behind cloud, and the sky was generally overcast, though with breaks here and there. A smart shower fell shortly before the eclipse, which necessitated a hasty closing of the observing huts, but it passed off before totality began, and the total phase was observed in what appeared to be an entirely clear space of sky. An alarm clock, ringing at every tenth second, was set up in the observing hut, and the timekeeper started the clock at the moment totality commenced, and called out at every tenth second the number of seconds yet remaining before the first re-appearance of Sun light. The clock face had been numbered for 205 seconds, and the total phase was over about one second and a half after the last had been counted, so that, estimating half a second for the delay in starting the clock, the duration of totality must have been 3 minutes and 27 seconds.

As totality approached I watched the Sun in the finder of my telescope, and gave the word "Start the clock" to the timekeeper as the last ray of Sun light disappeared. He started the striking-clock very promptly, and called the seconds very sharply and clearly throughout the eclipse.

I drew back the slide of the camera of the two-prism spectroscope first, then that of the single-prism spectroscope, and then proceeded to expose seven plates upon the corona itself with the five-foot camera, the several plates being exposed for the following times :—

First plate	0·2	second.
Second plate	2·0	,,
Third plate	10·0	,,
Fourth plate	40·0	,,
Fifth plate	7·0	,,
Sixth plate	4·0	,,
Seventh plate	0·2	,,

The fourth plate was exposed at the word "120 seconds," *i.e.*, 85 seconds after the commencement of totality, and closed at the word "80 seconds." The seventh plate was exposed before the word "20 seconds." It would have been quite possible to have obtained at least two more short exposure plates had I had them ready, but I had not judged it wise to attempt more than the seven of the original programme, as I had not been able to manage more than that number during the rehearsals, but I found that I was able to work more rapidly and collectedly during the eclipse itself than during the preliminary drills.

At "10 seconds" the timekeeper gave the word "close cameras," and I closed the single-prism spectroscope first, and the two-prism spectroscope afterwards. Both were closed some seconds before the end of totality. I was able to look up at the corona during the exposure of my plates, and I watched it through the finder during the 40 seconds exposure of the fourth plate. I saw no trace of a red or rosy tint in either chromosphere or prominences, but I remarked two exceedingly bright and beautiful prominences of the intensest silver whiteness. The taller of these is very well shown on some of my photographs.

The light during totality was feeble, but was just barely bright enough to enable me to read the programme which I had written out in a bold round hand, and had pasted on the top of the coronal camera.

After totality the Sun was covered by light cloud almost immediately, but a photograph was secured to give the direction of the two needles which were fixed in the east and west sides of the coronal camera, close to the sensitive plate; and later on the Sun was brought on the bottom of the slit of each of the spectroscopes and the plates were re-exposed for a second in order to secure a reference spectrum.

The photographs were not developed until after the return of the expedition to England, when Captain ABNEY kindly consented to undertake the operation. No ice could be obtained at Carriacou, and many of the best trial plates, taken for the purpose of ascertaining the focus of the different cameras, had been spoiled or completely lost by the heat. It was, therefore, thought unwise to run the risk of developing the eclipse photographs at Carriacou, and the plates were accordingly securely sealed up, and brought home undeveloped.

Of the seven photographs of the corona taken with the five-foot coronal camera, five proved to be good, one showed some deformation of the image, and the seventh was spoiled. The spoiled plate was the fourth in order of exposure, and was exposed for 40 seconds; the accident which rendered it useless was brought about in the following manner:—Mr. DRUMMOND, the owner of the estate on which we had fixed our observing station, and who had been our most self-sacrificing host, had looked through the little telescope attached to the coronal camera during the first 80 seconds of totality, but immediately on the exposure of the plate in question he stepped down and I took his place. Unfortunately, in making the transfer in the semi-darkness, the instrument received a severe jar, a jar rendered the more serious by the unsatisfactory character of the gearing of the R. A. circle alluded to above. The clock, however, drove the telescope very satisfactorily, both before and after this occurrence. The other plates were placed, with the photographs of the other observers, in the hands of Mr. W. H. WESLEY, Assistant-Secretary of the Royal Astronomical Society, who has prepared a drawing from the collation of the entire series. The plates exposed upon the corona were supplied by Captain ABNEY, and were $3\frac{1}{4}$ inches by $4\frac{1}{4}$ inches.

The two spectrum plates were also supplied by Captain ABNEY, and were $1\frac{5}{8}$ inch by $4\frac{1}{4}$ inches in size. Both these unfortunately proved to be useless, for, on development, the coronal spectrum was found to be masked by an ordinary Solar spectrum.

It appears most probable that, whilst taking the reference spectrum, I inadvertently exposed the plates to full Sun shine for a moment or so; for it does not appear possible that the exposure at the actual time of the eclipse can have been prolonged beyond the duration of the total phase. It is to be much regretted that the attempt was made to secure anything beyond the coronal spectrum upon the same plate with it, and that any instrument not absolutely necessary should have been mounted on the same stand as the cameras and their accessories. But for this mistake I should probably have had to report the success of all the nine photographs instead of that of only six of them.

X. Description of the Eclipse and Drawing of the Corona. By Captain Irwin C. Maling, Colonial Secretary.

The Total Eclipse of the Sun on the 29th August, 1886, was observed by me from Prickly Point, Grenada, West Indies, the station selected by Captain L. Darwin, R.E., and Mr. A. Schuster, F.R.S., of the Eclipse Expedition; they kindly requested me to take charge of the disc, and the following are the results of my observations:—

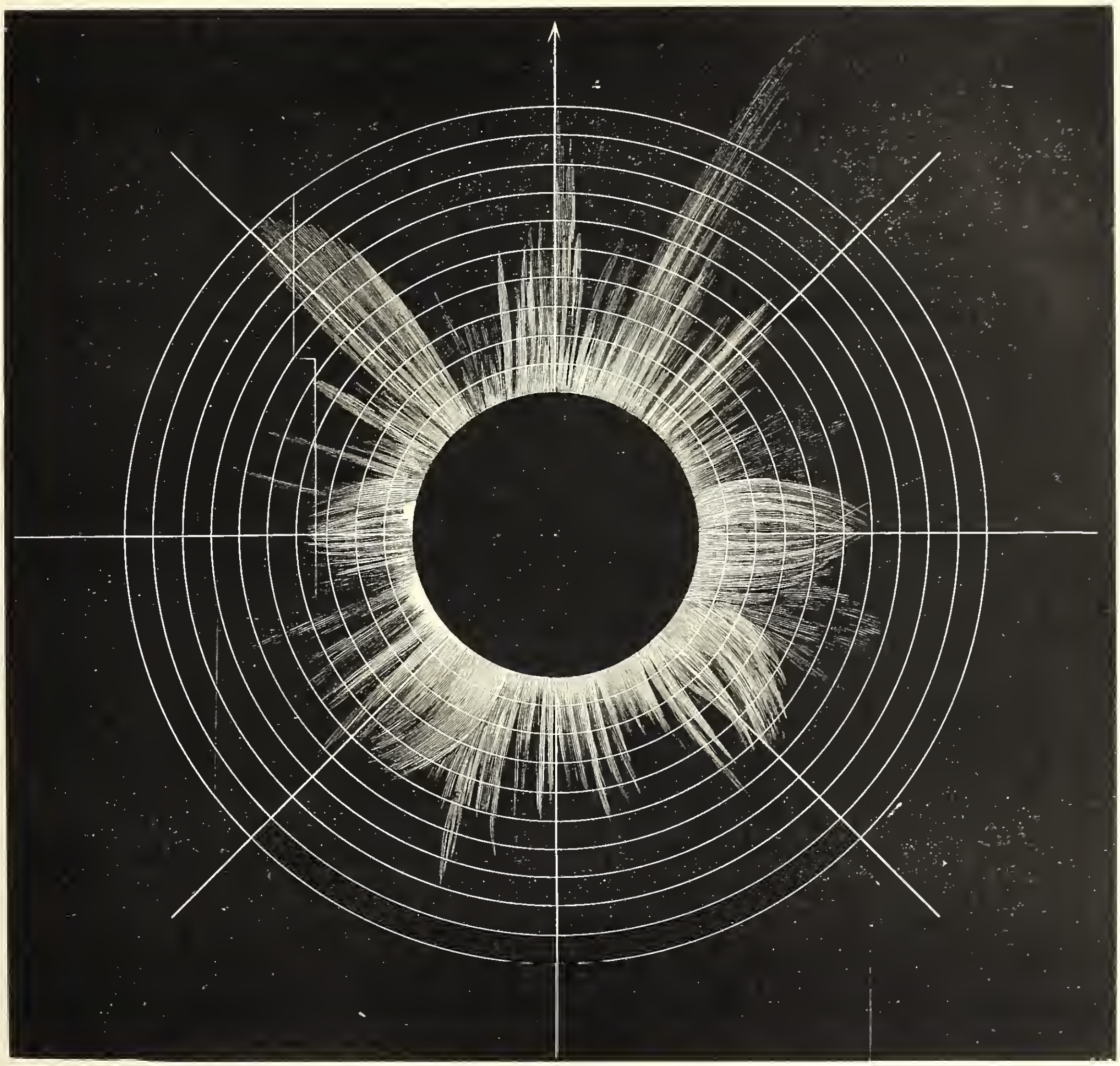
Previous to the commencement of totality my eyes were covered for 10 minutes to enable the sight to be as strong as possible; I had, however, scarcely begun my drawing when a small drift of cloud passed over the eclipse, hiding it for about 40 seconds, after which time it was perfectly clear, and I was enabled to continue my observations. The Moon was surrounded by a bright halo resembling that painted round the heads of saints in old pictures, from which long streams of light extended, varying much in length, form, and apparent density. The longest ray was on the upper right side. It was of a bright pale yellow, fading into white at the extreme point; it appeared to be about two and a half times the diameter of my disc.

The next longest, and I admit most beautiful, streamer was on the left side, in about the 315°, counting from zenith to the right, and immediately above a small red prominence. This ray differed from its companion on the right, inasmuch that it was of a conical shape, dense along the edges and upper curve, and gradually thinning towards the corona. Its colour appeared to be of a whitish-yellow, and the centre seemed to be hollow, as if one could see through it. This description also applies to the smaller conical shaped rays on the right and the lower part of the corona.

I further observed two small prominences of a red salmon colour on the left of the Moon. The upper one was round, the lower irregular and angular, apparently in contact with the Moon, or immediately contiguous to it.

It will be observed that a small space between the long conical streamer and zenith is left bare. I was unable to complete my drawing, owing to totality being over.

I attach the original drawing in chalk done on the spot. It has not been touched in any way since.



XI. On the Photographs of the Corona obtained at Prickly Point and Carriacou Island. By W. H. Wesley.

The drawing from which the plate (Plate 10), has been engraved was made from a series of 7 negatives, taken by Mr. Maunder, and 5 by Professor Schuster. In the original negatives the diameter of the Moon's disc is $\frac{5}{8}$ inch, and the drawing has been made to a scale of $2\frac{1}{2}$ inches for the Moon's diameter. The following is a brief description of the individual plates.

Mr. Maunder's *Negatives.*

Plate 1. Exposure 0·2 sec. Corona well defined, but not extending further than 7′ from limb. Three prominences on N.E. limb, and lower part of great prominence on N.W.

Plate 2. Exposure 2 secs. Corona well defined, but not extending further than 11′ from limb. Same prominences visible as on Plate 1.

Plate 3. Exposure 10 secs. Corona can be traced on N.W. to nearly a Lunar diameter from limb, but is extremely ill defined, showing scarcely any detail. Negative so dense that prominences can hardly be made out.

Plate 4. Exposure 40 secs. Corona of great extent, but ill defined. There has been much shake, and two separate images, about 13′ apart, are superposed upon the plate.

Plate 5. Exposure 7 secs. Corona can be traced on N.W. to about 27′, but detail very imperfect and indefinite. Not quite so dense a negative as Plate 3.

Plate 6. Exposure 4 secs. Greatest height of corona 22′. Dense negative, but detail very ill defined.

Plate 7. Exposure 0·2 sec. Scarcely a trace of corona. Plate fogged, but prominences on N.W. and W. limb perfectly defined and better seen than on any of the plates.

Professor Schuster's *Negatives.*

Plate 1. * Exposure 15 secs. (?) Lower portions of corona only are just visible. Two prominences on N.E. limb, and lower part of great prominence on N.W.

Plate 2. * Exposure 15 secs. (?) Corona slightly more shown than in Plate 1. One prominence visible on N.E. limb, and lower part of great prominence on N.W.

Plate 4. Exposure 20 secs. (?) Corona of greater extent than in any of the plates of this series, reaching on the N.W. to a height of nearly 26′. Details of coronal structure very well shown. Negative dense near limb; prominences not very distinct.

Plate 5. Exposure 15 secs. (?) Coronal detail well shown, but extension somewhat abruptly cut off at a height of about $\frac{1}{3}$ of a Lunar diameter. Dense near limb; prominences not well seen.

Plate 6. Exposure 5 secs. (?) Somewhat thin negative; details of corona in N. and S. polar regions very clearly defined, but a superposed image (a few minutes of arc from the principal image), with a trace of the re-appearing crescent, has blotted out the corona on the W. side.

* The corona was behind a film of clouds while these photographs were taken.

The Prominences.

These have been drawn from Plates 1, 2, and 7 of Mr. MAUNDER'S, and Plates 1 and 2 of Professor SCHUSTER'S. Twelve prominences are visible on the plates. Of these, four inconspicuous ones are near together in the N.E. quadrant. On the W. limb is a well-marked group of seven prominences extending from 20° N. to 10° S. of the equator, and at about 45° N. is a very remarkable branched one, rising to a height of 5′. The accompanying woodcut gives an enlarged view of this prominence, which is perfectly shown on Mr. MAUNDER'S Plate 7.

The Corona.

This has been mainly drawn from Plates 4 and 6 of Professor SCHUSTER'S. A somewhat greater extension is given in Mr. MAUNDER'S Plate 3, but its definition is so imperfect that but little use has been made of this plate. To ensure greater accuracy, two perfectly independent outline drawings were made to scale; these were then superposed, and further measurements made in every case of difference.

The rifts at the N. and S. poles, which have generally characterised the corona, were well shown in 1886. They were very wide, the northern rift extending along the limb for a distance of more than 40°, and the southern for 50°. They are almost symmetrically placed about the Sun's axis. Both rifts are filled with rays, somewhat similar to the polar rays of 1878, but not nearly so fine, numerous, or regular. The southern rift is less obvious than the northern, the rays filling it being broader, longer, and less definite. These rifts are bounded on each side by groups of more or less synclinal structure, which are clearly separated from the masses of equatorial rays. The synclinal groups bounding the great rifts are very unsymmetrical, those on the east being comparatively low and depressed towards the equator, while the corresponding rifts to the west rise to a much greater height, and are nearly radial in direction. The general mass of coronal structure on the western side is therefore much greater than that on the east, although the rifts are symmetrically placed. This want of symmetry extends also to the masses of equatorial rays, that on the west extending along the limb nearly twice as far as the corresponding mass on the east; it also rises to a much greater height, and has a far more complicated structure than the comparatively low and structureless eastern equatorial group.

The specially *synclinal* structure is best seen in the mass bounding the northern rift to the west. In the centre of this mass is the tall prominence before referred to, and over this prominence the coronal rays bend towards each other on either side. The base of this group is encroached upon by the broad equatorial mass, which appears to overlap it. The group bounding the northern rift on the east shows but little structure. The synclinal group to the west of the great southern rift is nearly radial, narrow, and conical, and extends to a height of quite 24′ from the limb. The corresponding group to the east extends not much more than half this height, and is broad and depressed towards the equator.

On comparing the corona of 1886 with those of other years, it appears decidedly different from any previously photographed. The great northern and southern rifts extending for some distance along the limb, and the character of the rays filling the rifts, recall to some extent the more extreme form shown in 1878. The depression of the main coronal mass towards the equator, however, even on the eastern side, is not nearly so great as in 1878, and the western side is totally different in character. In fact, the western half of the corona of 1886 shows a striking resemblance to the corresponding side in 1871, but the compressed eastern half in 1886 and the wide polar rifts have nothing in common with the corona of 1871.

In 1875 there was the same decided want of symmetry between the two sides of the corona, but in this case it was the western half which was more depressed towards the equator. In 1875, also, there was a greater tendency towards the extreme polar depression of 1878.

The corona of 1886 has no resemblance to that of 1882, which had no conspicuous polar rifts. Almost equally dissimilar was the corona of 1883, with a single great rift at the north pole only, opened at an angle of about 90°, but hardly extending to the limb, and very unsymmetrically placed with regard to the Sun's axis. Of the Eclipse of 1885, I believe the only successful negatives were those taken by Mr. Radford, at Wellington, N.Z., now in the possession of the Royal Society, but which have not yet been published. I am not certain about the orientation of these, but, as far as I can judge, they present no resemblance to the corona of 1886.

In conclusion, it may be said that this corona occupies a middle place between the extreme forms of 1871 and 1878.

X. *Report of the Observations of the Total Solar Eclipse of August* 29, 1886, *made at the Island of Carriacou.*

By the Rev. S. J. Perry, *S.J., F.R.S.*

Received April 5,—Read May 5, 1887.

[Plate 11.]

The astronomers appointed by the Committee of the Royal Society to proceed to the West Indies to observe the total eclipse of the Sun on the morning of August 29, sailed together from Southampton in the R.M.S. "Nile," Captain Gillies, on July 29, and, after a fair passage, anchored at Barbados at daybreak on August 11. A committee meeting on board had partly fixed our plans with regard to the stations of observation, so that, when we found two of H.M.'s gunboats awaiting our arrival in the roadstead, the instruments of Mr. Maunder and of the Rev. S. J. Perry were, after consultation with the commanders of H.M.'s vessels, at once transferred to the "Bullfrog," whilst the remainder of the instruments found a ready berth on the deck of H.M.S. "Fantôme," which, being the larger of the two gunboats, was reserved for the observers destined for Grenada and its immediate vicinity. Both the war-vessels started the same morning for Grenada, Mr. Lockyer and Dr. Thorpe sailing on board the "Fantôme," in order to secure the earliest possible interview with the Governor of the Windward Islands. The rest of the astronomers left the same evening in the R.M.S. "Eden," Captain Mackenzie, and, after touching at St. Vincent, arrived at Grenada early on the afternoon of the 12th. The private luggage of Mr. Maunder and of the Rev. S. J. Perry was immediately placed on board H.M.S. "Bullfrog," where they received the heartiest welcome from Captain Masterman, R.N., who devoted the best part of his own cabin to Father Perry, and found a comfortable private cabin for Mr. Maunder. Captain Archer, R.N., had also arrived at Grenada in command of H.M.S. "Fantôme"; and the "Sparrowhawk," a surveying vessel, commanded by Captain Oldham, R.N., was anchored in the harbour of St. George, her officers having been placed by the Hydrographer of the Admiralty at the disposal of the expedition. Previous to our arrival Governor Sendall, most ably assisted by Captain Melling, had personally inspected most of the best sites for the astronomical observations, collected all existing records of the weather, and designed huts for the protection of the instruments. Carriacou and Green Island were told off for the northern station, to be occupied by Father Perry and Mr. Maunder, assisted by the officers and men of H.M.S. "Bullfrog" and by Sub-Lieutenant HELBY, of H.M.S. "Sparrowhawk." It was thought, however, more advisable not to separate the members of this party by a distance of some twenty miles, and, therefore, the more northerly island of Carriacou was fixed upon as the site best suited for both observers.

The 13th of August was spent in packing the huts and getting them on board, and on the 14th H.M.S. "Bullfrog" left early for Carriacou, and cast anchor in Hillsborough Bay the same afternoon. Immediately on our arrival we received a visit from the resident magistrate, Mr. ROCHE, and from the harbour master, Mr. ISAACS, who both offered us every assistance in their power. On landing we paid our respect to the venerable Canon PETRETTO, whom Governor SENDALL had specially named, along with Mr. ROCHE, as most anxious to render us every possible aid. From information received from these gentlemen and from the resident physician, Dr. ARCHER, we concluded that the southern shore of the island might provide an excellent site for the observations, as well as good anchorage, and a fair prospect of landing safely our heavy instruments. A note to Mr. DRUMMOND, the owner of an estate in the south of the island, met at once with a cordial response, and we were invited to make "The Hermitage" our home during our stay at Carriacou.

On August 15, it being Sunday, we lay at anchor off Hillsborough, but the next morning, in spite of a heavy sea, which formed part of the cyclone that destroyed five churches and many houses at St. Vincent, only forty miles to the north of Carriacou, we steamed round to Tyrrel Bay, and took up our final position close by the estate of Mr. P. DRUMMOND. With some difficulty we found our way through the coral reefs in the captain's cutter, and were met on the shore by the land agent of Mr. DRUMMOND, who pointed out the best spot for landing and the most accessible road to "The Hermitage." This building stands on the summit of a ridge 200 feet above the level of the sea, the land stretching in a long promontory towards the South-West between Tyrrel and Manchioneal Bays. On examining carefully the bearing of the neighbouring hills we found that none would at all interfere with our view of the Sun on the morning of the eclipse, Chapeau Carré being sufficiently remote from the East point, and the others still more so; and, as everything else was as favourable as we could expect, we fixed upon this ridge as our station of observation. The following bearings of some of the chief objects in view serve to fix our position very accurately. The observations were made by Mr. MAUNDER, the angles being reckoned from the true North, through East:—West end of Sandy Island 2°·5, Chapeau Carré 43°·2, Eclipse Peak 86°·0, centre of Frigate Island 168°·3, peak at entrance to Tyrrel Bay, N. side, 329°·5.

The site chosen for our huts was at the summit of the ridge, about 300 yards from "The Hermitage," and towards the ENE. The ground in the immediate vicinity was fairly level, and the foundations for the equatorials excellent. A number of labourers and a bullock-cart were hired for the following day, and soon after daybreak the men of H.M.S. "Bullfrog" began to land our heavy packages on the sandy beach.

The native workmen carried some of the lighter pieces to the top of the ridge as soon as they were landed, but most of the instruments had to be placed on the bullock-cart and dragged up the steep, rough road at great cost of time and labour, the men aiding the bullocks when necessary. The ship's carpenter, with a shore party of seamen and marines, was soon at work clearing the ground and erecting the observing huts, and these had to be fixed rather firmly in the ground, in order to resist the storms of wind so violent in these islands. The dwelling of Mr. DRUMMOND, thanks to his self-sacrifice, afforded excellent accommodation for the two astronomers and for Lieutenant HELBY; but the incessant attacks of the gallinippers, which brought on an incipient fever, rendered it necessary for Mr. MAUNDER to sleep on board H.M.S. "Bullfrog" during our stay at the island. A small cottage, consisting of two rooms, and situated close to the main dwelling, was also placed at our disposal. One of the rooms served to keep our packages dry, and the other, at the expense of the sailroom of the gunboat, was excellently fitted up as a developing room for photography. A bed of concrete was laid in one of the observing huts for the photoheliograph of Mr. MAUNDER, and concrete was also used to fix firmly in position the legs of the strongly-built tripods on which stood the equatorials of JONES and ALVAN CLARK. The landing of the instruments commenced on the 17th, thus leaving twelve days before the eclipse for the erection of huts and instruments and for all necessary preparations. The exact bearing of the polar axes of the equatorials was determined by observations of Polaris and of δ Ursæ Minoris, and we were ready on Monday, the 23rd, to commence the testing of our instruments. The Simms transit-theodolite from Stonyhurst Observatory was of great use for observing altitudes of the Sun by which to rate our chronometers, and also for determining the positions of the disks erected to obscure the inner corona for those who had undertaken to make sketches of the outlying streamers during totality. These disks were fixed firmly on the top of each hut, with sight-holes on uprights placed at a convenient distance on the side opposite the rising Sun. Captain MASTERMAN and Paymaster OSBURN kindly volunteered to observe and sketch these faint, delicate objects.

In the course of the morning of the 23rd Father PERRY adjusted the grating of his spectroscope, and obtained a very perfect spectrum. H.M.S. "Fantôme" arrived the same day from Grenada, and we learnt from Captain ARCHER that Mr. LOCKYER had just established himself at Green Island, the station appointed at first for Mr. MAUNDER.

On the 24th the weather in the early morning was all that could be desired, and the Sun could have been observed under the most favourable circumstances had the eclipse occurred on that day, although later in the morning there was a succession of heavy showers.

The detailed plan for the morning of the eclipse was definitively settled on the 25th, the assistants were chosen, and everything made ready for a complete rehearsal on the morrow. The plan finally adopted was the following:—

1. The Jones 4-inch equatorial, provided with a Hilger solar prism and power of 110, was destined for observations of first and last contacts; but, as it was raining heavily both at the beginning and at the end of the eclipse, this notice of the preparation for contact observations will amply suffice.

2. After contact, the solar prism was to be dismounted, and a large direct-vision spectroscope by BROWNING substituted in its place, to be used during totality as an analysing instrument by Lieutenant PASCOE, R.N., assisted by Dr. ARCHER.

3. Dr. WRIGHT, R.N., had charge of the Hilger direct-vision spectroscope, mounted on its own stand, and to be used as an integrating instrument. The observations with this spectroscope, and also those undertaken by Lieutenant PASCOE, were intended to supplement any results obtained with the grating attached to the $5\frac{1}{2}$-inch Alvan Clark equatorial.

4. Mr. MAUNDER'S work with the photoheliograph and spectroscopic cameras, which will form the subject of a separate report.

5. Drawings of the streamers of the outer corona, to be made with the aid of disks obscuring the inner corona. The sketches of Captain MASTERMAN, R.N., and of Paymaster OSBURN, R.N., will be appended to this report, with their own remarks explanatory of the nature of the results obtained.

6. Spectroscopic observations with a Rowland grating attached to the $5\frac{1}{2}$-inch Alvan Clark equatorial. The telescope to be pointed by Lieutenant HELBY, and the readings taken by Father PERRY.

This equatorial of ALVAN CLARK was the instrument used by the Rev. T. WEBB in the preparation of his well-known work on "Celestial Objects." The glass was one of those guaranteed by Mr. DAWES, and it would be difficult to surpass it in excellence of defining power. The mounting was not comparable with the quality of the glass, and no driving clock was attached. Mr. WEBB used only a slow motion in R.A.; but, as it was necessary to vary the position of the slit of the spectroscope during totality, in order to place it successively on different parts of the corona, it became imperative to provide a slow motion for N.P.D. before taking the instrument to the West Indies. This addition was made by COOKE, of York, and he also arranged the clamps so that the telescope might be fixed firmly in every direction. The absence of clockwork to drive the instrument made it necessary to have an assistant to point the telescope, and therefore Lieutenant HELBY, of H.M.S. "Sparrowhawk," was chosen for this work. The grating used with this equatorial was kindly lent by the authorities of South Kensington; but a direct-vision spectroscope, constructed by HILGER, was also provided, by which an equal dispersion could be obtained, and which might replace the grating in case of accident. The plate of the grating was polished and figured by J. A. BRASHEAR, and the parallel lines, 14,438 to the inch, were ruled on Professor ROWLAND'S engine at Baltimore in 1884. The plate was mounted by HILGER on a student's spectrometer belonging to Stonyhurst. The combination answered very well, and gave little trouble in the adjustments.

The work expected from this instrument by the Committee of the Royal Society was an examination of the spectrum of the inner corona immediately before and after totality, and a search for the bands of carbon during totality. The observations before and after totality were to serve as a test of the accuracy of Mr. LOCKYER's theory concerning the concentric layers of the solar atmosphere, in which selective absorption is supposed to take place. It is important to know whether nearly the whole of the Fraunhofer lines are produced in the layer observed by Professor YOUNG close to the photosphere in 1870, or whether they are due to the combined absorptive action of successive layers, each producing its own characteristic lines. If the latter hypothesis be the true one, then the layers nearer the Sun's centre, being hotter than those outside them, should produce brighter lines. These, therefore, would be the first to come into view as the eclipse approaches totality, and they would also be the most enduring after totality. Belonging, as they are supposed to do, to the inner layer, they should be short and bright, and not increase in length, but only in relative intensity, as the darkness became greater. The other lines, belonging to layers farther removed from the Sun's centre, would be invisible at first, owing to their want of intensity, but they would gradually come into view as the darkness increased, and always appear less brilliant and longer than those which preceded them. Immediately after totality the previous order would be, of course, reversed, the longest lines, which are also the faintest, disappearing first, and then the others, according to their length, leaving the shortest and brightest in view, until even these are at last overpowered by the returning light of the photosphere. In 1882 the Egyptian observations had favoured this theory, and Mr. TURNER was asked to repeat in 1886 the observations previously made in the F. region, whilst Father PERRY watched the same phenomena in the region on the less refrangible side of b. The plan adopted at Carriacou was that Lieutenant HELBY should keep the slit of the spectroscope radial on the centre of the solar crescent for eight minutes both before and after totality, whilst Father PERRY watched the changes in the bright line spectrum of the inner corona. To enable the assistant to point the telescope with very great accuracy, a cap with a white enamelled surface had been closely fitted to the slit, and on this cap two sets of parallel lines at right angles to each other had been most carefully ruled, the distances between the lines being one-tenth of the projected diameter of the solar disk. The cap could be fixed only in one position, so there was no possible danger of one set of lines not being in exactly the same direction as the slit, and still less of the slit being partially covered. A clear image of any visible corona was thus secured, and the assistant could see perfectly whether the required portion of the image fell upon the slit.

The remaining work expected from this instrument was a search during totality for the two principal bands of the carbon spectrum. In 1883 Professor TACCHINI had thought he glimpsed the carbon bands, and some few previous observations rendered their existence in the coronal spectrum not improbable. It was evidently of very great importance to test thoroughly so interesting a fact, and Professor TACCHINI joined the British Expedition at Southampton with the intention of placing, if possible, this question beyond the region of doubt. The instructions to Lieutenant HELBY were to place the slit of the spectroscope exactly on the inner edge of the corona at the commencement of totality, and then to move it successively to distance 0·1, 0·2, 0·3, 0·4, and 0·5 of a diameter from the dark surface of the Moon, repeating afterwards at the Sun's pole what first was done near his equator. To avoid rotating the spectroscope, the slit was placed radial at the solar equator, and tangential at the poles. The same portion of the spectrum remained always in the field of view during the whole of the observations, and embraced rather more than the distance from W.L. 5600 to *b*, comprising, therefore, the positions of the two principal bands of the carbon. To fix accurately the place of any lines visible, photographs of the solar lines in the portion of the spectrum required had been taken at Stonyhurst on plates stained with eocine, and on others kindly sent by Captain ABNEY, R.E. As these did not come out distinct enough to use safely with a feeble illumination, a number of the principal lines in the field of view were measured with a micrometer, and then mapped on a large scale and reduced photographically to the scale required. Transparent scales graduated to tenths of millimetres were also prepared, so as to be ready for any change that might be required.

On August the 26th, between 6 and 7.15 A.M., we had the first all-round practice with every instrument in position, with all hands on shore who were to take part in the observations on the 29th, and each thing done just as if the eclipse had been taking place. The time was called every ten seconds by ROBERT SMITH, A.B., coxswain of the captain's cutter, in a loud and distinct tone, that could be heard easily by all present. JOHN COLLUM, signalman of H.M.S. "Bullfrog," and other reliable seamen, noted down the observations as these were called out, and affixed the corresponding times. All was found to work well, and not a few useful lessons were learnt for the morning of the 29th. Later on in the day, whilst observing with the grating, I found the heat so intense that I was forced to leave the instrument for a time and retire to the house. On my return I perceived at once that some inquisitive looker-on had been gently feeling the grating with his greasy finger, probably to ascertain its degree of smoothness. My dismay at first was great, as I was afraid I might have been obliged to abandon the grating in favour of the Hilger direct-vision prisms, with which I had supplied myself in case of accident. I removed part of the roof of my observing hut, in order to test the grating thoroughly, and I was satisfied at last that the spectrum showed no signs of being in the least affected by the stain left by the finger on the surface of the ruled metallic plate. I resolved, therefore, to retain the grating; but, finding that the second order of spectrum with a power of 4 gave a more brilliant picture than the first order with the power of $6\frac{1}{2}$, I made up my mind to adopt the second order for the day of the eclipse, although this necessitated the sacrifice of the photographs of the solar lines, which had to be replaced by a scale divided in tenths of millimetres.

The early morning of the 27th was cloudy and showery, and all practice impossible at the eclipse hour, but the Sun was observed later on in the day, and the position of the various points of the limb accurately determined for all circumstances that might arise in the use of direct or inverted images, of solar prisms, or of projections.

On August the 28th the sky was quite clear an hour before sunrise, and, observing the Sun at the time when first contact was to take place on the morrow, I found the definition very good, but the low altitude made the limb somewhat unsteady. I was then using the power of 110 on the Jones 4-inch equatorial. Changing my instrument, I then took a number of readings of the solar lines, using the first and second orders of the spectrum with the powers of $6\frac{1}{2}$ and 4 respectively. As I again found the second order with power 4 to be much more distinct than the other combination, I resolved to adhere to my intention of adopting the second order for any observations on the morrow.

At 2 A.M. on the 29th, the morning of the eclipse, not a cloud was to be seen, and at 4 A.M. the stars were still shining brightly in every direction, although a slight breeze had sprung up from the South. The wind then shifted gradually towards the East, and at 5 A.M. clouds were fast beginning to appear. Soon it became but too evident that rain was falling at no great distance to the North-East of our station, and heavy clouds began to show themselves in the direction of the rising Sun. External contact took place in the midst of rain, and the first glimpse of the Moon was obtained through the Jones equatorial when one-third of the Sun's surface was already obscured. The clouds then cleared off rapidly, and we could safely uncover our larger instruments. The sky, however, remained only fairly good until the near approach of totality. This was particularly unfortunate, as the interval between first contact and totality would have been most valuable for testing the more delicate adjustments of our instruments, and for preliminary observations.

As soon as the Sun's image could be seen upon the cap of the slit, Lieutenant HELBY placed the centre of the radial slit on the middle of the outer arc of the solar crescent, and kept it there as steadily as possible; but no bright lines came into view between W.L. 5600 and *b* until one minute before totality, when the first line seen was 1474 K., which stood out very brightly, and then followed almost immediately a number of bright lines close by *b*, on the less refrangible side. I estimated their number at about fifteen. They seemed to be of different lengths, but I did not see them long enough to judge of their relative intensities. The height to which 1474 K. extended from the photosphere might be about 8′ of arc. The exact position of this line and the general position of the group were fixed by the lines of the solar spectrum, which had been under my eye for some time previous. I never moved the grating, or the viewing telescope, during the observations, so that everything was in excellent adjustment the whole time, and the field of view was well known. The captain's coxswain was counting the time aloud during the whole of totality, the seconds being taken from Mr. MAUNDER's clock.

When totality commenced, the slit of the spectroscope was radial on the inner edge of the corona, near the centre of the line where the thin crescent had just been visible; no carbon bands could be perceived. The slit was then moved successively to distances 0·1, 0·2, 0·3, 0·4 and 0·5 of a solar diameter from the Moon's dark limb as 190^s, 180^s, 170^s, 160^s, and 150^s were called by the coxswain. All this time I kept my eye steadily at the viewing telescope, but could see nothing of the carbon bands. The slit was then moved to the vicinity of one of the Sun's poles, and placed tangentially on the inner edge of the corona at 130^s before the end of totality. Afterwards it was gradually shifted away from the Moon's limb, the distances being 0·1, 0·2, 0·4, and 0·5 of a diameter, at 120^s, 110^s, 100^s, and 90^s respectively, and in none of these positions could I catch the slightest trace of the bands of carbon.

Thinking it hopeless to continue any longer the search for carbon, and wishing to be prepared in good time for the observation of the bright lines at the end of totality, I asked Lieutenant HELBY to place the slit at once radial at the point of re-appearance of the photosphere. Whilst this was being done, I took up a powerful binocular, which I had placed for this purpose close at hand, and viewed for a moment the eclipsed Sun. The upper rays on the western limb, to the right and left of the vertical line, were by far the longest streamers, and were situated almost at right angles to each other, a third, but shorter, ray appearing between them to the left of the vertical diameter. These rays were all well defined, and the one most to the North of West was curved on both sides like a leaf. On the Eastern, or lower, limb the rays were irregular and less extended than in the West. I did not notice any rays near the poles, but my view was scarcely more than an instantaneous glance.

At this moment Lieutenant HELBY lost the solar image from an irregular movement of the telescope, but I was able to recover it almost immediately. Whilst thus replacing the Sun upon the slit, I obtained a hasty view of the corona upon the white enamelled cap, and this picture far surpassed in beauty anything I had seen before, although my binocular is an excellent instrument. The details of the streamers, and the short red prominences, were exceedingly well defined, showing the splendid quality of the Alvan Clark objective and the purity of the sky at the moment.

When the coxswain had called out 20^s, the slit being radial near the point of re-appearance, I saw a large number of lines flash out in my limited field of view: there might have been fifty altogether between W.L. 5600 and *b*. This lasted only a very short time, and after totality no lines were seen, as the rising wind interfered considerably with the steadiness of the telescope, and in a few minutes we were again deluged with rain. The darkness was never much less than that of a fair moonlight night, but during totality the light was not equal to that of a full Moon in a clear sky. Heavy rain prevented the observation of last contact.

Since the observations were taken I have frequently wished that my equatorial had been supplied with clockwork, which would have enabled me to dispense with the aid of an assistant for fixing, in each case, the position of the slit. I have not any reason to doubt but that the gentleman who so kindly aided me on this occasion did his duty as perfectly as could be expected; but it is not such an easy matter for one who has had only a few days' acquaintance with the slow-motion rods of an equatorial to keep the slit of a spectroscope on any precise point of a celestial object. The strength of the wind and the imperfections of the slow-motion rods added in the present instance to the difficulty of following the Sun exactly with the R.A. rod, and of changing the N.P.D. as required. I am forced to the conclusion that the time lost by the unaided observer in placing the slit of his spectroscope would be more than compensated by the security he would feel, that he was viewing exactly the desired point of the object. Without clockwork this is not practicable; but I should never think of again attempting eye observations with a spectroscope during a total eclipse without a clock to drive my equatorial, and then, if a grating was used, I should certainly dispense with any assistant at the telescope.

The two main questions to be answered by the spectroscopic observations at Carriacou were: (1.) Does the absorption, which produces the Fraunhofer lines, take place mainly in a single layer of the solar atmosphere, or in concentric layers? (2.) Does carbon exist in the corona? As far as the above results may afford any satisfactory evidence on these two points, I should be inclined to say that the difference in the length of the lines observed before totality on the less refrangible side of *b* seems somewhat to strengthen the view that the absorption takes place in concentric layers. And the search for carbon tells us that, if present, its spectrum was not strong enough in 1886 to make any appreciable effect upon the retina, when the eclipsed Sun was viewed through so powerful a diffraction spectroscope as that used at the island of Carriacou. It may, perhaps, be established, by later observations, that the intensity of the carbon spectrum varies in each eclipse, and may have some direct connection with the amount of solar activity.

I should mention in conclusion that the diameter of the solar image on the slit plate of my spectroscope was 20 millimetres, the width of slit used $\frac{9}{100}$, and its effective length $6\frac{82}{100}$. The dispersion was sufficient to enable me to see b_3 and b_4 very distinctly separated.

Subjoined are the Sketches of the Coronal Streamers with explanatory notes by Lieutenant Commander J. MASTERMAN, R.N. (Plate 11), and Mr. F. W. OSBURN, R.N. (p. 362).

Notes on the Solar Eclipse of 29th August, 1886, observed at Carriacou.

H.M.S. "Bullfrog," under my command, was ordered to convey the northern division of the expedition for observing the eclipse to Carriacou, an island about twenty miles N.W. of Grenada, and render what assistance she could; I and all the other officers of the ship offered our services for the observations, which were accepted.

On arriving at Carriacou, on the 14th August, we were cordially welcomed by Mr. Roche (the magistrate), and hospitably entertained by Mr. Drummond, the owner of the property on which we selected a spot to erect the huts. This was a small plateau, on the summit of a steep ridge 175 feet above the level of the sea. The sea was on each side of the ridge, and Mr. Drummond's house was 200 or 300 yards off. The beach in the bay where the ship anchored was very suitable for landing the instruments.

I undertook the observation of one of the disks; it was mounted on the hut used by Mr. Maunder. The height of disk above eyepiece was 11 feet $10\frac{1}{2}$ inches, the angle subtended by the disk being $72'$; the diameter of the disk was 9 inches; the cross-bar was $1\frac{1}{4}$ inch thick; the uprights were 1 inch thick; the length of uprights was 2 feet $10\frac{1}{2}$ inches; the horizontal distance of the eyepiece from the disk was 33 feet 7 inches; the apparent diameter of the disk from the point of observation was equal to $2\frac{1}{4}$ solar diameters.

On the 26th and on the 28th I practised the observations under as nearly as possible the same conditions that we should be under on the day of the eclipse. I was prevented from doing so on the 27th by the weather.

The hole in the eyepiece I had increased to $\frac{1}{4}$ inch diameter.

At 10 minutes before totality my eyes were bandaged with a thick black handkerchief, without any pressure on the eyeballs, but totally excluding the light. The bandage being taken off at the commencement of totality, I looked through the eyepiece, but found the adjustment not correct, and lost some time in correcting it before I could commence my observations.

The sketch I took of the phenomenon, as I saw it, together with a copy (which differs only in being a little more finished and shaded a little darker), accompanies this.

The first things that caught my attention were the two rays of light marked A and B; they seemed to be of exactly the same length, and each to make an angle of about 45° with the vertical, in length $1\frac{1}{2}$ diameter of the disk from the disk, or 4 solar diameters from the Sun's periphery. The next thing that I observed was the ray that I have marked C, very bright, but small and partially hidden by the cross piece; the observed extension was under 2 solar diameters from the Sun's limb. The two remaining rays, D and E, on the Sun's eastern limb, were very faint, and they seemed to fade into the general surrounding light instead of tapering away to a point, as the others did.

At the end of the observation I am certain that B was considerably longer than A, though, as I said before, they appeared to be exactly similar at first. I should be inclined to attribute this to the fact that the Sun was altering a little in azimuth, and towards the end of the observation I had given all the correction that the apparatus admitted of, and therefore the Sun got nearer to the side of the disk.

I found that the Sun altered in altitude so rapidly that it was difficult to keep pace with it with the rough arrangement for altering the elevation of the eyepiece; a small rack and pinion would have been a great boon.

A better arrangement for making correction in azimuth and a longer range would be an advantage.

The cross-piece, $1\frac{1}{4}$ inch wide, interfered with the observation; a thin metal rod or stout wire would have been better.

I observed with the disk until the last moments of totality, and only just looked off in time to see the burst of bright light at the second internal contact. I at once made the (original) sketch which I send, and saw nothing of any other phenomena, as, two minutes after, everything was obscured in clouds and we were in a drenching rain.

I had the tides both before and after the eclipse measured, and found the rise and fall was normal, 18 inches.

J. MASTERMAN,
Lieutenant Commander, H.M.S. "Bullfrog."

Grenada, 1st September, 1886.

NOTES DESCRIPTIVE OF DRAWING (PLATE 11) BY LIEUTENANT J. MASTERMAN.

A and B.—Very distinct, especially B. They each formed an angle of 45° with the vertical. B appeared to increase in length, not in brilliance; A did not.

C.—Distinct, but small. I could not detect any sign of C above the batten, though, if the upper edge had been inclined to the horizontal at the same angle as the lower edge was, I think I must have done so.

D and E.—Very faint indeed, merging into the luminosity which was faintly apparent between all the shoots of light; but, faint as this luminosity was, it had a distinct definition, which had the appearance of going about the points of the shoots.

Copy of sketch of the phenomenon as observed by me with one of the disks mounted on the roof of a hut.

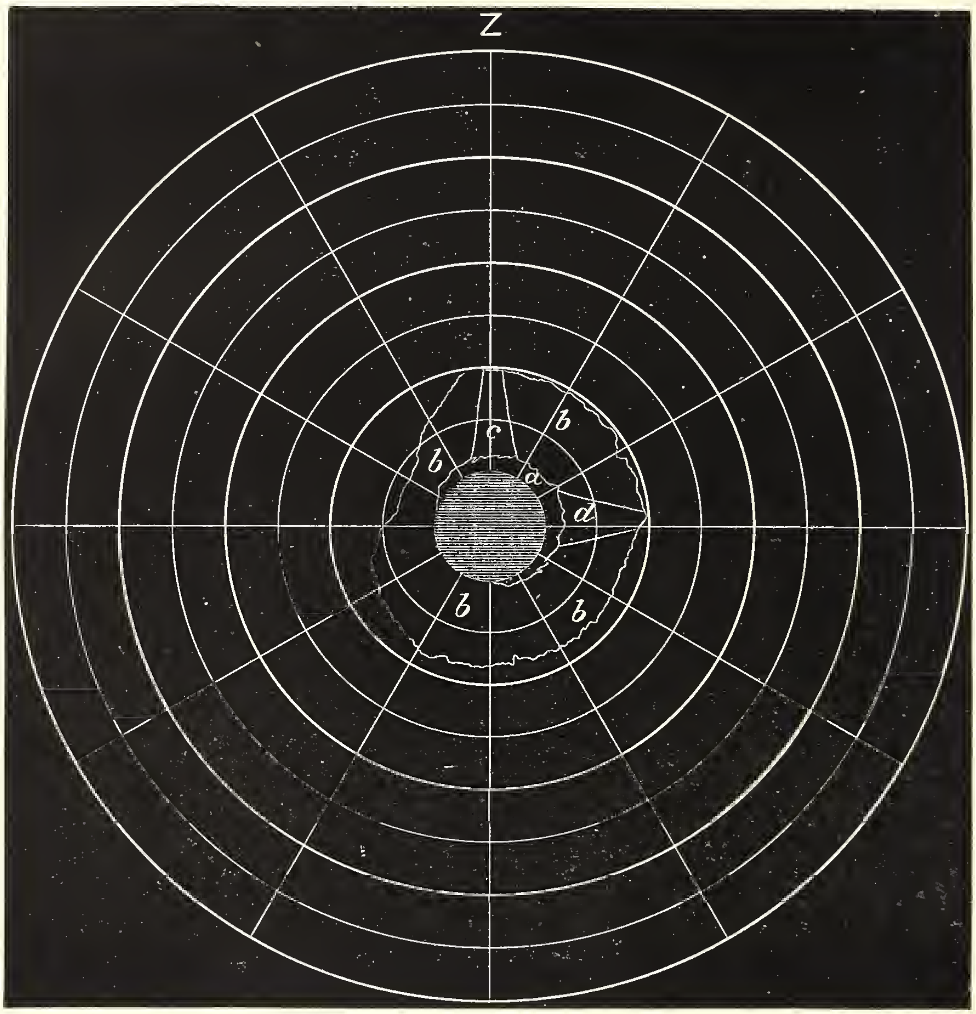
Dimensions:—Disk, 9 inches diameter; cross batten, $1\frac{1}{4}$ inch; upright battens 1 inch, length 2 feet $10\frac{1}{2}$ inches.

J. MASTERMAN.

Drawing by Mr. F. W. Osburn, R.N., of H.M.S. "Bullfrog."

Horizontal distance from eyepiece to disk, 26 feet 10 inches; height above level of eyehole, 9 feet 4 inches; diameter of disk, 9 inches; angular diameter, 1° 31′; breadth of upright batten, 1 inch; breadth of cross batten, $1\frac{1}{4}$ inch; height of disk above ridgeway of roof, 2 feet $10\frac{1}{2}$ inches.

Note.—At the commencement of totality, when my eyes were first unbandaged, I found, on looking through the eyepiece, that the eclipsed Sun was about 4 diameters above the disk, and some time was wasted in shifting the eyepiece down as far as it would go, when the Sun and corona were just covered by the disk; but before totality was over, the corona was again visible over the top of the disk.



Marked *a*.—Around three parts of the disk there appeared a band of bright light, sharply defined and irregular in shape, its broadest part on the right, and gradually diminishing until, at the left lower corner, it ceased altogether.

Marked *b*.—Entirely surrounding the disk I observed an irregularly shaped field of very faint light, standing out at its widest part nearly 1 diameter beyond the disk.

Into the field above mentioned I observed two very faint rays extending, one towards the zenith (but so faint as to be hardly discernible), and the other to the right of the disk (which I saw more clearly), on each side of the horizontal support; these rays are marked respectively *c* and *d*. They appeared about equal in length, *i.e.*, 1 diameter of the disk.

Francis W. Osburn.

XI. *On the Determination of the Photometric Intensity of the Coronal Light during the Solar Eclipse of August* 28–29, 1886.

By Captain W. de W. Abney, *C.B., R.E., F.R.S., and* T. E. Thorpe, *Ph.D., F.R.S.*

Received February 7,—Read February 14, 1889.

Introduction.

Although it has long been suspected that the amount of light emitted by the corona, as seen at various Solar Eclipses, may vary within comparatively wide limits, no attempts to measure its intensity appear to have been made prior to the Eclipse of December 22, 1870. On that occasion Professor Pickering employed an arrangement constructed on the principle of Bunsen's photometer. It consisted of a box 9 inches wide, 18 inches high, and 6 feet long, within which a standard candle could be moved backwards and forwards by means of a rod. One end of the box was covered with a piece of thin white paper, on which was a greased spot about half an inch in diameter. The box was adjusted so that the rays from the corona were normal to the plane of the paper, and the lighted candle was moved backwards and forwards within the box until the grease-spot was no longer visible. From a number of observations made during the period of totality of this eclipse, Mr. Waldo O. Ross, acting under Mr. Pickering's direction, found that the standard candle had to be placed at distances varying from 14·4 to 21 inches from the paper before the visibility of the greased spot was reduced to a minimum. ('U.S. Coast Survey Reports,' 1870, p. 172.) The observations were much interrupted by clouds, and are also probably affected by irregularities in the rate of the burning of the candle. The mean of all the readings was 18·5 inches: hence the light of the corona in 1870 was apparently equal to 0·42 of a standard candle at a distance of 1 foot.

A precisely similar arrangement was used by Dr. J. C. Smith during the Solar Eclipse of July 29, 1878. Dr. Smith, observing at Virginia City, Montana, U.S., found from eight observations that the candle had to be placed at a distance of $51\frac{1}{4}$ inches from the screen before the minimum of visibility of the greased spot was obtained.

During the same eclipse, Professor John W. Langley made observations on the intensity of the coronal light as seen from the summit of Pike's Peak, Colorado, by means of a photometric arrangement suggested by Professor S. P. Langley, and intended to measure the relative distribution of light in the corona. The idea was, first, to draw an outline of the corona; second, to measure the light of the corona at several points along a solar radius extended to the outer limits of visibility; and third, to draw one or more iso-photal lines which should give the contour of the corona for varying degrees of illuminating power. The method was to project the image of the corona upon the screen of a BUNSEN's photometer in which glass ground to slight opacity replaced the greased paper; and instead of one translucent spot there was a large number, in order that several hundred samples of coronal light taken from different portions of its surface might be observed simultaneously, and compared with the standard light by drawing iso-photal contour lines.

In the apparatus as finally arranged the screen consisted of a piece of perforated cardboard covered by a sheet of oiled paper. A number of translucent spots separated by opaque interspaces was thus obtained, the spots being sufficiently close together to allow the projected image to be seen with but little loss of distinctness. The light from the corona was reflected from a heliostat and transmitted through a photographic lens of long focus, the aperture of which could be diminished, if required, by means of a cat's-eye diaphragm, and passed down a dark chamber about 41 feet in length, at the end of which was a box carrying the screen and standard candles. Within the box was a railway, on which ran a wagon mounted on brass wheels and bearing the lighted candle, the distance of which from the screen was recorded on a wooden rod placed immediately in front of the observer. The whole apparatus stood on four piers, and it was so arranged that any vibration caused by wind should not be communicated to the lens producing the coronal image. The focal length of the lens was 11·27 metres; and the diameter of the solar focal image was ·104 metre.

The corona as actually observed was excessively faint, and could only be seen to an extent of less than 1′ from the Moon's edge. The coronal light was so feeble that it was found impossible to measure its intensity at several points along an extended radius.

From his observations Professor LANGLEY concludes that the light from the corona at 1′ from the limb of the Moon was equal to that of the standard candle at a distance of 1 metre. From the photometric value of the candle as compared with diffused sunlight, LANGLEY further found that the intensity of the coronal light about 1′ from the limb of the Moon was ·0000132 of that of mean sunlight; at 3′ from the limb it was ·0000000244. Assuming the intensity of mean sunlight to be 500,000 times greater than that of moonlight, the corona at 1′ from the Moon's limb was six times the intrinsic brightness of the Moon; at 3′ it was but one-tenth the intrinsic brightness of the Moon. (Professor S. P. LANGLEY's Report, p. 211, 'Washington Observations' for 1876, Appendix III.)

The photometric observations made during the 1878 Eclipse have also been discussed by Professor W. HARKNESS, of the United States Naval Observatory (*loc. cit.*, p. 386). Combining the observations, he concludes that the total light of the corona was ·072 of that of a standard candle at 1 foot distance, or 3·8 times that of the full Moon, or ·0000069 of that of the Sun. It further appears from the photographs that the coronal light varied inversely as the square of the distance from the Sun's limb.

Probably the brightest part of the corona was about 15 times brighter than the surface of the full Moon, or 37,000 times fainter than the surface of the Sun.

It would further seem that the corona of December 22, 1870, was $7\frac{1}{4}$ times brighter than that of July 29, 1878.

Description of Methods Adopted during the Eclipse of August 28–29, 1886.

The instruments used by us for the measurement of the coronal light on this occasion were three in number. The first was constructed to measure the comparative brightness of the corona at different distances from the Moon's limb. The second was designed to measure the total brightness of the corona, excluding as far as possible the sky effect. The third was intended to measure the brightness of the corona, together with the brightness of the sky in the direction of the eclipsed Sun.

In a paper by one of us, in conjunction with General FESTING,* it was shown that light of any colour can be measured for luminosity in terms of light of any other colour, provided always that the last-named light can be rapidly altered in intensity, so that at one time it is evidently below the intensity of the light to be compared, and immediately afterwards that it is evidently above it. The oscillations of the intensity, if then gradually diminished, finally give the value of the coloured light in terms of the luminosity of the light of which the intensity is rapidly changed.

In a more recent paper† it has been shown that the light of a glow lamp may be used for measuring the intensity of any other light by making a rapid change in the resistance of the circuit. In the photometric measures which are now to be described this plan was adopted for ascertaining the value of the coronal light in terms of a Siemens unit. Before continuing the description it may be well to note that the Siemens unit is very nearly 0·8 of a standard candle. This unit has the advantages that the area of the burner is fixed; that the flame used in the photometric measures can always be made of exactly the same height; that the thickness and shape of the flame are practically invariable; that the material producing the flame can be obtained in commerce; and that any slight impurity in it has no practical effect on the value of the light emitted. Neither the effect of the temperature at the time of trial nor the variation due to difference in barometric pressure has been thoroughly tested, but there are presumably but slight differences due to these causes. At all events, there is nothing to prevent its employment for the object we had in view. One experiment may, however, be quoted as regards the luminosity of the flame when the temperature was varied some 20°. The lamp was carefully adjusted so that the tip of the flame just touched the gauge supplied with the instrument, and its value taken against the glow lamp, which was kept at a bright yellow heat by a current passing through it. A large photograph of the flame was also taken. The

* 'Phil. Trans.,' 1886, "Colour Photometry," ABNEY and FESTING.

† 'Roy. Soc. Proc.,' vol. 43, 1887, ABNEY and FESTING.

lamp was then warmed from 55° to 75°; the flame became longer, but when turned down to the height of the gauge the same value was obtained against the glow lamp as before within 2 per cent. Another photograph was taken of the flame from the same position, and the two compared. The flames in both cases were equal in dimensions.

In the paper last referred to it was also shown that either the BUNSEN or the RUMFORD method of photometry could be adopted. The method of RUMFORD is undoubtedly better than that of BUNSEN when the lights are very different in colour, as in the latter method there is a certain thickness of translucent material through which both lights have to pass, and only after such passage can equality of illumination be estimated; and if the paper employed for the screen is coloured in any degree, this must of necessity affect the results. The light of the corona and that of the glow lamp are very different in colour, the former being stronger in the blue end of the spectrum than the latter. It must be recollected that the greatest luminosity is in the yellow of the spectrum in both cases, and, though the blue end of the spectrum alters the hue, it has very small effect on the luminosity. This being the case, it was thought that no error of any magnitude would be introduced by adopting the BUNSEN plan, since the brightest part of the two spectra would be compared with one another.

It was evidently impracticable to adopt the RUMFORD method in the apparatus in which the intensity of different points in the corona had to be measured. For this purpose a telescope by SIMMS, lent by the Astronomer Royal, was employed. The object glass had a focal length of 78 inches and an aperture of 6 inches, thus forming an image of the Moon ·76 inch in diameter. The image was received on a circular white screen contained in a photometric box and placed exactly in the focus of the object glass. In the centre of the screen was traced a circle of the diameter of the image of the Moon, and during the observation the Moon's disc was made to fall exactly within the circle. As the telescope was equatorially mounted with clockwork, the image was kept stationary within the circle. The screen was of RIVES' paper of medium thickness, and round the pencil-circle a series of small grease spots about $\frac{1}{8}$ of an inch in diameter had been made. There was some difficulty in preparing these small grease spots, but a method was eventually devised which answered admirably. Faint pencil lines were drawn radially from the centre of the circle, and the places where each spot was to be produced were marked with a dot. White blotting paper was soaked in spermaceti, any excess being avoided. Small discs, $\frac{1}{8}$ inch, were punched out, and these discs were put centrally on the dots. Blotting paper was next placed over them when in position, and a hot flat-iron was passed over them. The blotting paper and the small discs were then removed, and clean blotting paper and the flat-iron again applied to remove any slight excess of spermaceti. The screen now presented the appearance shown in fig. 1.

Several screens were made and tested. The test consisted in causing a glow lamp on one side of the screen to balance a glow lamp placed at the same distance on the other side by means of a variable resistance in the circuit. The spots, if correctly made, become invisible at the same time. The majority of screens fulfilled this condition, and the best as regards uniformity of size of spot and freedom from grain in the paper were selected for use. The screen, as will be seen from the figure, was mounted in a circular frame, which could be rotated so as to bring the spots into any desired angular position. It could be removed at pleasure by releasing it from the buttons which held it in position.

Fig. 1.

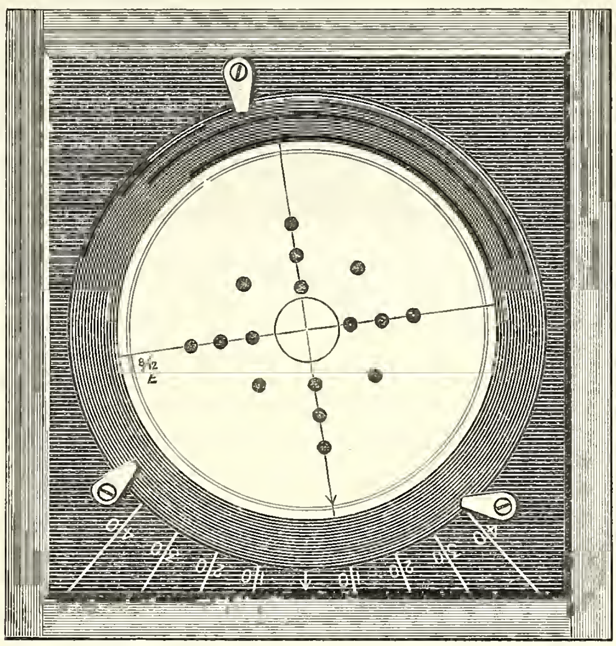
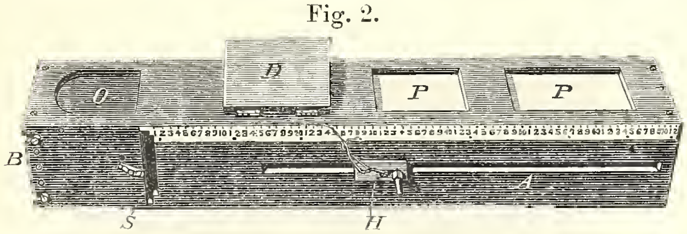


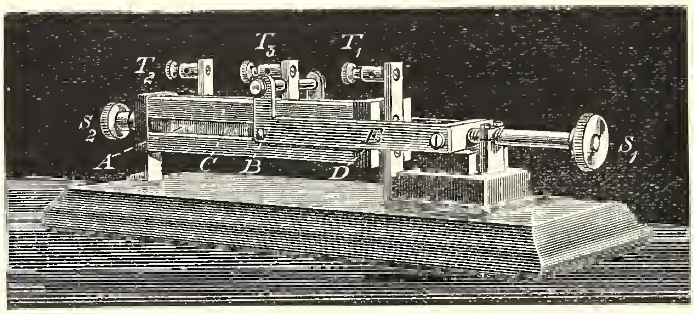
Fig. 2.



To hold the screen a box was constructed, as shown in fig. 2. It was made as light as possible, panels of card (as at P, P) being used instead of wood when practicable. The glow lamp to be employed was fixed in a holder inside the box; this could move along the slot A, and be fixed by a thumbscrew, H, in any desired position. (It may here be remarked that the plane of the filament was at right angles to the axis of the tube.)

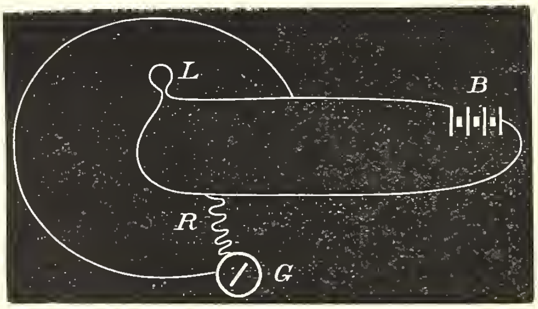
At the end B was an aperture into which the sliding tube of the telescope fitted; at D was a door, which could be opened to adjust the lamp. The screen shown in fig. 1 was inserted at S, and held in position by means of buttons. At O was an opening, which was covered by a black velvet bag into which the head of the observer was inserted during the time of observation. As before said, the image of the Moon was accurately focussed on the screen inserted at S, and was viewed through the opening at O. The wires to the lamp passed through the slot; the carbon-resistance (fig. 3) and also the galvanometer should have been introduced into the circuit.

Fig. 3.



The carbon-resistance used was one supplied by Mr. VARLEY, and the description is taken from a paper already referred to. It consisted of a series of pieces of carbonised cloth, more or less in contact. The carbonised cloth is represented by C (fig. 3), which fills the whole length from A to D when loosely packed. At B is a plate to which T_3 is attached, and which can be separated more or less from a fixed metal plate to which T_1 is connected by the arm E, which is moved by the screw S_1. At A is an insulated block, carrying another plate to which T_2 is attached, and A can be carried backwards or forwards by means of the screw S_2. For some purposes the main current can be brought in at T_3, and leads be taken from T_2 and T_1, thus forming part of a Wheatstone bridge. During the eclipse the terminals T_1 and T_3 were used.

Fig. 4.



The connections were made as shown in fig. 4. The current from 10 cells of a carefully made up Grove battery, B, was passed through the lamp L. A shunt, including the galvanometer, G, and the resistance, R, was made. The brightness of the lamp was thus increased by adding more resistance to the shunt. Consequently, in the measures made, the highest readings of the galvanometer showed the lowest intensity of light. It would have been better had the ordinary plan of putting the galvanometer and resistance in the main circuit been adopted, but when once the value of the lamp illumination by the plan actually employed had been ascertained for varying resistances it became a matter of no moment. The galvanometer used in this case was one of THOMSON'S ammeters, made more sensitive by fixing a permanent magnet alongside the usual magnet, so as partially to neutralise its magnetism. By this plan a very small change in current gave a large deflection, or at all events a deflection which was readable. By reproducing these deflections under exactly similar conditions the illuminating value of the lamp could be measured in the ordinary way.

The second instrument, which we shall call the integrating box, for measuring the total coronal light with as little light from the sky as possible, was constructed on the same principles. It consisted merely of a long deal box coated internally with lampblack, in which a screen with a large grease spot was inserted at S. There was a

Fig. 5.

similar slot for the lamp as in the other instrument. The end A was, however, open, and during the eclipse it was placed at such an angle that the axis of the tube pointed to the centre of the Moon. The aperture, O, for making the observations was in this case also covered with a black velvet cloth, under which the head of the observer was placed.

The third piece of apparatus consisted of an ordinary Bunsen bar photometer, 60 inches in length, with movable disc, made by Messrs. ALEX. WRIGHT and Co., of Westminster. As originally arranged, it was fitted for two standard candles; for the purpose of the eclipse observations, these were replaced by a small glow lamp.

As the plan of the photometric work contemplated by us depended for its execution upon such assistance as we were able to get out at Grenada, it was arranged that we should take advantage of the kind offer of service made by Captain ARCHER and the officers of H.M.S. "Fantôme," which had been told off to assist the expedition, and make the observations at some spot in convenient proximity to the anchorage of that vessel. As the latter end of August falls during the hurricane season in the West Indies, it was desirable to moor the "Fantôme" in the most secure anchorage in the island, viz., in Clerk's Court Bay, which is at the south end of Grenada. It appeared from the charts that a suitable station might be found on the southern end of Caliveny Island, distant about $1\frac{1}{2}$ mile from the spot which would be made use of as the anchorage. Caliveny Island was accordingly included in the list of stations provisionally selected by the Eclipse Committee of the Royal Society, and submitted to the Governor of Grenada. Mr. SENDALL and Captain HUGHES were kind enough to visit the spot, and they reported that a fairly good station might be obtained on the extreme end of the island, but that difficulties might be experienced in landing the apparatus. Captain ARCHER deemed it prudent, therefore, to make a preliminary survey of the place before the "Fantôme" left St. George. No landing was practicable on the leeward side of the island, and, although two or three places were met with on the other side, they could only be counted upon during fine weather. Moreover, as the greater part of the island is covered with dense "bush," the transport of the instruments to and from the station would be very laborious and tedious. There was the further difficulty that it was well nigh impossible to make one's way through the tangle of bush at night, when much of the work of adjustment of the equatorial would have to be done. And, lastly, there was the possibility, even if the instruments were successfully set up, that the noise of the surf and the driving spray in bad weather might seriously interfere with the work of observation.

For these reasons we decided to abandon the Caliveny site, and, after a careful examination of the neighbourhood, we selected a station near a little creek on Hog Island, to the westward of the bay. The position was fairly good; during dry weather it was indeed all that could be desired. The ground was about 10 to 15 feet above the sea level, and was close to a shelving, sandy beach, readily accessible and generally free from swell. In bad weather, another landing could be obtained round a point to the north, with only a few hundred yards of bush to be got through. The position had a good eastern horizon, the sun rising behind the lowland running out to Point Egmont, which here subtended an angle of less than 1°. Its position, as taken off the Admiralty Chart, was lat. 12° 0′ 4″ N., long. 61° 43′ 45″ W.

The "Fantôme" left St. George on the 17th August, and came into Clerk's Court Bay in the afternoon of the same day. All the apparatus was safely got to shore before nightfall, and the positions for the base of the equatorial and for the tents of the party undertaking the integrating work were decided upon. Early next morning the erection of the wooden hut to shelter the equatorial was begun, and a concrete base for the stand made upon the rock, which was found at a depth of a few inches below the surface of the soil. Before the surface of the cement was finally set it was carefully levelled, the base put into position, and the mounting of the telescope proceeded with. The integrating apparatus was placed in a small marquee tent, a few yards to the north of the hut. Small slabs of concrete were also made in convenient situations, to carry the galvanometers, &c. As the photometer box attached to the equatorial added considerably to the length of the apparatus, it was necessary that the hut should be of somewhat larger dimensions than that generally adopted by the rest of the expedition; otherwise it was very much of the pattern of that which the Governor had caused to be constructed prior to our arrival at St. George, and which answered admirably in all respects. Round the hut and tents a deep trench was cut, with an outfall leading down the slope towards the sea to carry off the rain-water collected by the roofs. Our chief difficulties, indeed, were due to the frequent rains and constant humidity. At times the ground became worked into a sort of quagmire. By the 19th everything was in fair adjustment, and during the subsequent ten days the various members of the party were assiduously practised on all available occasions in their duties. The driving clock of the equatorial gave trouble in the outset, but it was eventually got into order, and on the day of the eclipse, and for some days previously, ran sufficiently well. The constant dampness of the ground, and consequent absence of dust, probably contributed to its good behaviour.

The duration of the total phase of the eclipse at Hog Island was about 230 seconds. We found that a simple and sufficiently accurate method of informing the party as to how this time was speeding, and of the amount of time still left at intervals of 15 seconds before the end of totality, could be obtained by observations made with a 14-second sand-glass, such as is employed on shipboard in heaving the log.

The arrangement of the party was as follows :—

Integrating box	Lieutenant ANGUS DOUGLAS.
Galvanometer	Mr. WEBB.
Bar photometer	Lieutenant BAIRNSFATHER.
Recorder	Mr. ROBERT JACKSON.

These instruments were placed close together in a small marquee. Each observer had in addition a man to charge and connect up a battery of Grove cells for the glow lamps.

Equatorial photometer . .	Professor T. E. THORPE.
Galvanometer	Mr. H. A. LAWRANCE.
Disc observations	Captain ARCHER.

On the day before the eclipse the following instructions were issued to the observers in charge of the integrating apparatus :—

Instructions to Lieutenant DOUGLAS. (*Integrating Box.*)

1. The eclipse begins at 6hr. 12m. L.M.T. Totality commences at 7hr. 10m., and lasts about 230 seconds.

2. It is desirable that on the morning of the eclipse you should be ashore not later than 6 A.M. (It is assumed that your integrating box, leads to galvanometer, glow lamp, and resistance apparatus are left in position over night.) Ascertain that the box is in proper azimuth, and test by the Abney level that the inclination is about 19°.

3. See that the connections to the glow lamp are properly made, and that the galvanometer is levelled on the cement foundation and is in adjustment.

4. Give instructions to have a battery of 7 cells in readiness for you, not later than 6.30: 6 cells to be connected up, the seventh to be reserved in case of accidents. See that you have actually 6 cells connected up before you begin.

5. At 6.30 connect up all leads, see that your lamp works properly, and that the intensity of the light responds to the screw of the resistance apparatus.

6. The back edge of the wooden piece carrying the lamp may be conveniently placed at Division 50 of the graduated scale. This, with the resistance apparatus open, will probably give you more light than you require. If at the moment of totality, and with the lamp full on, the coronal light is greater than that of the glow lamp, push up the lamp to Division 30. If you have occasion to move the lamp from 50 (which is very improbable), be very careful to note the particular distance from the screen at which you place it.

7. At 7·0 request Mr. WEBB and the man who records his readings to take up their stations at the cement slab. Take up your own position at the integrating box. Mr. WEBB'S position to be such that he readily hears your command to read.

8. As the light decreases just before totality, that is between 7.0 and 7.10, turn the light up or down with the screw, so as to follow the decrease, so that after the moment that totality begins you may be able to begin your comparisons with the least possible delay.

9. Intimation that totality has begun will be given to the party by Quartermaster FOLLETT, who will call out 230. At intervals of 15 seconds he will call out the number of seconds still to elapse before totality ends.

10. When you have made your adjustment by the screw as carefully as you can, call out "read" to Mr. WEBB; do not turn the screw again until you hear him give his reading to the man who records. Again work the screw, make a second adjustment, and again call out "read," and again wait until Mr. WEBB has given his reading before you begin again. If this point is not attended to, the needle will be in such rapid oscillation that it will be impossible to get an accurate galvanometer reading.

11. Experience shows that 12 readings may be taken in 100 seconds, but it is not advisable that you should attempt to make more than 12 comparisons during "totality." Recollect that a few readings carefully and deliberately done are worth far more than a large number made hurriedly. Do not touch the position of your lamp at the end of your observations.*

12. Be careful not to fatigue your eye by looking too much at the Sun during the first stage of the eclipse. Insist that all talking ceases after 7.

Instructions to Lieutenant BAIRNSFATHER. (*Bar Photometer.*)

1. The eclipse begins at 6hr. 12m.; totality commences at about 7hr. 10m., and lasts about 230 seconds.

2. It is desirable that you should be ashore not later than 6 A.M. (It is assumed that your photometer bar, leads to galvanometer and to glow lamp, stand for galvano-

* This instruction was given in order that the position of the lamp might be verified after the observations were concluded.

meter, &c., are left in position on Saturday evening.) Ascertain that your photometer is in the proper azimuth ; test by the Abney level that its inclination is about 19°. Fasten the front rod securely down, so that on running the lantern backwards and forwards it works smoothly and without shaking the bar unduly.

3. See that the connections to the glow lamp are properly made ; next see that the galvanometer is levelled and in adjustment.

4. Give instructions that a battery of 7 cells should be in readiness for you not later than 6.30. Only 6 cells are to be connected up, the seventh to be in reserve in case of accident.

5. At 6.30 connect up all leads, see that your lamp works properly, and take a reading of your galvanometer. Go round to the back of your tent and see that you have actually 6 cells connected up.

6. At 7.0 request Mr. JACKSON to take up his station at the end of your table, and get ready to record. Go round to the galvanometer again and carefully note the reading of the needle, which Mr. JACKSON is to record. At 7.5 take up your position at the photometer bar.

7. As the light wanes follow up with the lantern, so as to get it into position with the least possible delay after the moment totality begins.

8. The preliminary drill has shown that 12 sets of double readings may be taken during the duration of totality. Do not, however, aim at doing more than 7 or 8 sets (in all 14 or 16 readings). A few readings carefully and deliberately done are worth far more than a large number taken very hurriedly.

9. When you have taken your last reading let the lantern remain as you placed it for the reading, in order that after the eclipse its position may be verified. Be careful to note on which side the middle point of the photometer bar the readings are taken.

10. There will probably be sufficient light during totality to see readily the numbers on the photometer bar ; but, in case you have difficulty, it will be advisable to have a lighted lantern in readiness under your table.

11. At the end of totality, and therefore at the conclusion of your readings, again note the position of the galvanometer needle and record the deflection.

12. Be careful not to fatigue your eye by looking too much at the Sun during the first stage of the eclipse. All talking to cease at 7.0.

Observations on the Day of the Eclipse.

The general character of the weather, as noted during the ten days prior to the date of the eclipse, rendered it very doubtful whether any photometric observations would be at all possible at the time of totality.

The following Table will serve to indicate the condition of the sky at about the hour of the eclipse on successive days from August 17th to the 28th.

The amount of cloud is given on the scale from 0 to 10.

		Cloud.	
Aug.	17	5	Sun seen through haze. Clouded at times.
,,	18	3	Sun unclouded at time of totality.
,,	19	3	,, ,, ,,
,,	20	2	,, ,, ,,
,,	21	4	Sun seen through thin cloud.
,,	22	4–5	Sun seen through haze. Much rain in night.
,,	23	5	,, ,, ,, ,,
,,	24	5–6	Sun frequently clouded.
,,	25	8–9	Sun clouded over; much rain at times.
,,	26	6–7	Sun seen through faint clouds.
,,	27	8–9	Dense clouds. No Sun; much rain at times.
,,	28	7–8	Hazy at time of totality. Strong east wind with showers at times.

The observing party left the "Fantôme" shortly before daybreak on the morning of the eclipse, and in a short time everything was in readiness for the observations. The sky was almost completely clouded over. A light breeze from the E.S.E. drove up sluggishly moving cumuli in great detached masses, some as high as 40° above the horizon, *i.e.*, double the height at which the Sun would be at time of totality. The high land of Grenada was completely enveloped in cloud, and heavy rain was falling in the middle and over the western slopes of the island. Over the low land of Point Fort Jeudy, across the bay, behind which the Sun would come up, was a mass of cumuli, with flattened bases, seemingly motionless. At 6.15 there was a slight shower of rain, and at 6.18 the partially eclipsed Sun was seen for a few seconds. In spite of the fact that the Solar disc was being rapidly obscured, the clouds were gradually breaking up into detached masses. By about 6.40 the greater portion of them had drifted away to the North. The equatorial was then put on the Sun, and the gradually diminishing crescent observed by reflection from the photometric screen. The clock went fairly well, and no adjustment was necessary to keep the limb in contact with the pencilled line of the image on the screen; indeed at no time during the totality was the edge separated from the circle by more than the thickness of the pencil line itself. The shadow of the Moon was plainly visible on the white disc. The moment of totality was 7hr. 10m. 14.6s. (L.M.T.), the calculated time as determined by interpolation from the data furnished by Mr. Hind for Caliveny and Point Salene, was 7hr. 10m. 18s. With respect to the observation, it may be stated that it was determined by means of a chronometer, the error and rate of which had been ascertained from observations of double altitudes made during the preceding 10 days by Captain Archer.

After a few moments' observation of the corona, the structure of which was admirably pictured on the white screen in the equatorial, the photometric comparisons were begun in the order previously fixed upon.

In all, 15 comparisons were made out of the 16 originally intended. At about 60 seconds from the calculated end of totality, a dark cloud swept over the corona, rendering all further observation impossible.

Reduction of the Observations.

The lamp used in the equatorial photometer had the values given in Table I. in Siemens units, reduced to a distance of 1 foot from the screen.

TABLE I.

Measures of potential.	Intensity of light reduced to 1 foot from screen.
24	·005
23	·007
22	·010
21	·0145
20	·020
19	·027
18	·036
17	·050
16	·066
15	·089

The lamp was 21·25 inches from the screen during the eclipse; hence the real values of the lamp were higher, but, as it is an inconvenient distance at which to compare the values obtained by the different instruments, an uniform distance of 12 inches from the screen has been adopted.

The same remark applies to the lamp used by Lieutenant DOUGLAS. The measures of potential in intensity of light are given in Table II.

TABLE II.

Measures of potential.	Intensity of light reduced to 1 foot from screen.
10·0	·0024
9·0	·0036
8·0	·0040
7·5	·0055
7·0	·0074
6·5	·0104
6·0	·0135
5·5	·0183
5·0	·0258
4·5	·0340

During the eclipse this lamp was really 21·6 inches from the screen, and, as in the former case, the actual values of the light were higher.

The numbers in the above tables are graphically represented in the following curves, from which the results of the observations were measured.

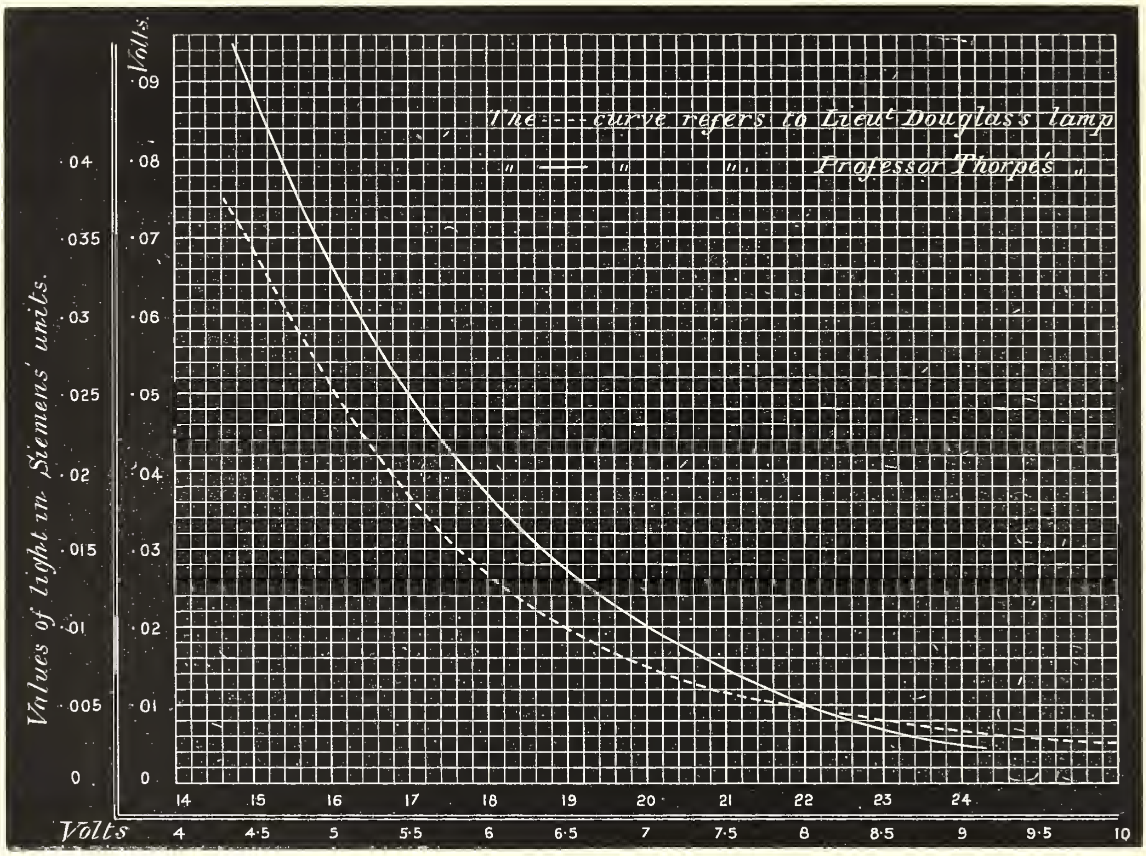


Table III. gives the value in light of the readings on the equatorial photometer. The numbers of the grease spots in Column I. correspond with the order in which they were read. Column II. gives the calculated distance of each spot from the Sun's centre in terms of the Solar diameter. Column III. gives the readings on the voltameter; and Column IV. the corresponding value of the light in Siemens units. Column V. gives the approximate time in seconds after the beginning of totality when the several readings were made.

TABLE III.—Readings on the Equatorial Photometer reduced to the Values of Light Intensity.

I.	II.	III.	IV.	V.
				seconds.
1	1·55	15·8	·070	10
2	2·66	18·3	·033	20
3	3·66	20·3	·019	30
4	1·61	16·5	·058	40
5	2·55	17·5	·043	50
6	3 44	19·4	·024	60
7	1·25	19·8	·021	70
8	2·16	21·4	·012	80
9	2·60	24·0	·005	90
10	1·11	18·0	·036	100
11	2·16	21·5	·013	110
12	3·16	23·5	·005	120
13	2·16	23·0	·007	130
14	2·33	23·5	·005	140
15	2·33	23·5	·005	150

Fig. 6 shows the position of the several spots as seen on the photometer screen. The arrow and shaded segments show the apparent direction of the Moon's path across the Solar disc.

Fig. 6.

The numbers correspond with the order in which the readings were made.

Table IV. gives the value of Lieutenant DOUGLAS's readings, and Table V. those of Lieutenant BAIRNSFATHER. It must be remembered that Lieutenant DOUGLAS's instrument measured the total light from the corona with only a small portion of the light from the sky, whereas the bar photometer as used by Lieutenant BAIRNSFATHER measured the light from the corona together with that from a large portion of the sky. Hence the readings on the bar photometer are necessarily higher than those obtained by the integrating box.

TABLE IV.—Readings on the Integrating Box reduced to Values of Light Intensity.

Voltameter readings.	Value of light at 1 foot from screen in Siemens units.	Approximate time when readings were made from beginning of totality.
5·4	·0197	15
6·2	·0122	30
5·9	·0142	45
7·0	·0075	60
6·8	·0085	75
7·1	·0070	90
7·3	·0065	105
7·3	·0065	120
7·7	·0054	135
7·8	·0051	150
8·3	·0045	165
8·8	·0040	180
8·9	·0035	195
9·3	·0030	210
9·4	·0027	215
9·4	·0027	220

TABLE V.—Readings on the Bar Photometer reduced to Values of Light Intensity.
Value of the lamp = 0·133 unit.

Distance of lamp from screen in inches.	Equivalent value in Siemens unit at 1 foot.	Approximate time of reading.
33·5	·0160	50
34·4	·0152	70
36·2	·0137	90
39·1	·0118	110
39·6	·0115	130
44·1	·0093	150
46·4	·0084	170
47·8	·0079	190
48·0	·0078	210
47·8	·0079	220
48·5	·0077	230

It will be noticed that Lieutenant BAIRNSFATHER's first reading was made when 50 seconds of the totality had passed. The delay was due to the circumstance that it was found necessary to diminish the number of cells connected up after totality had begun, and hence a new reading of the galvanometer was needed before the observations could be commenced. The actual time of beginning could only be very approximately known, but from trials made subsequently it is probably accurate to within 5 or 6 seconds.

It will also be observed that both Lieutenant DOUGLAS and Lieutenant BAIRNSFATHER continued to read after the Moon and corona were actually obscured by cloud. Of course neither of the observers was able to notice the fact of the obscuration without looking up from his instrument. The passage of the cloud was, however, readily noticed on the screen of the equatorial photometer, and the time of obscuration during the phase of totality was noted. It occurred, as already stated, at about 1 minute from the calculated end of totality.

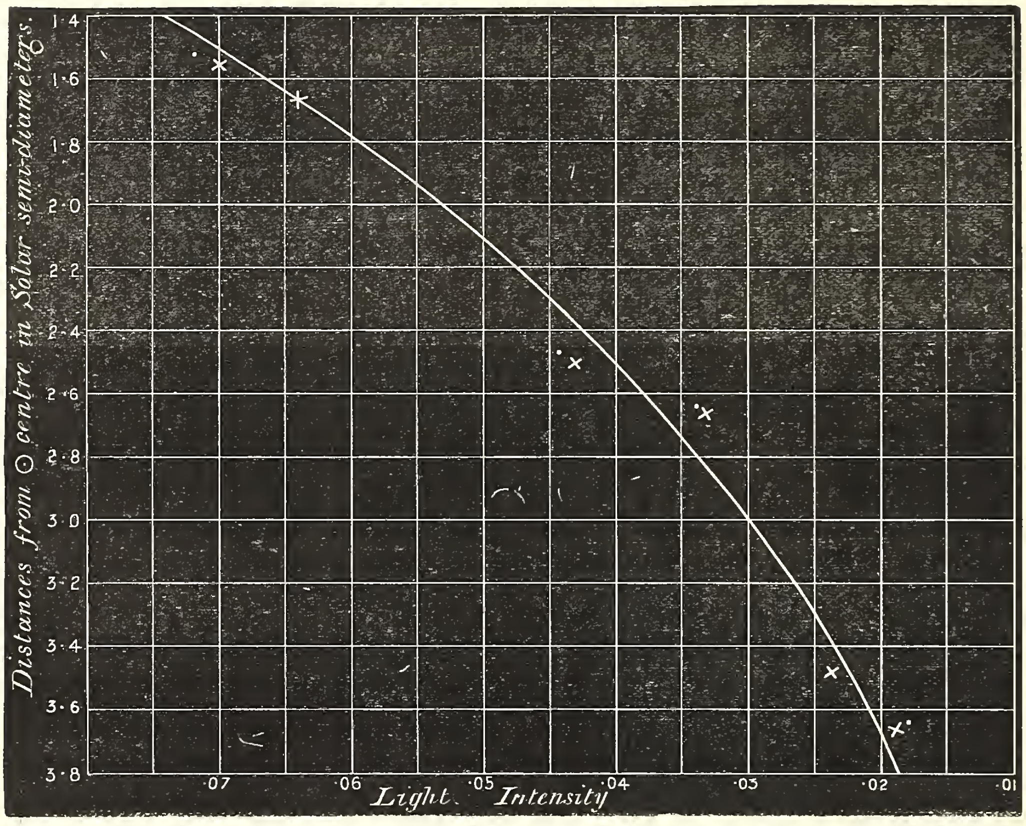
The observations of all these observers show in the clearest manner that the results are affected by haze after the first 60 or 70 seconds of totality. Thus, the first six readings on the equatorial photometer are fairly concordant, but after 1 minute had elapsed the light intensities begin to decrease rapidly. Thus, spots 7 and 10, which were considerably nearer to the limb than spots 1 and 4, show a considerably less intensity, and the differences are far greater than could be accounted for by any possible variation of local intensity in coronal light.* Lieutenant DOUGLAS's readings also show a sudden drop at about 60 seconds from the beginning of totality, and they continue to decrease steadily and in precisely the same manner as those of Lieutenant BAIRNSFATHER.

This result is indeed what might have been anticipated. It must be remembered that the air was practically saturated with moisture ; a slight shower had fallen even a few minutes before totality, and the lowering of the temperature consequent on the obscuration of the Solar disc would inevitably cause the gradual precipitation of moisture from air already charged to saturation.

If we assume, therefore, that we are justified in regarding the first six observations made with the equatorial photometer as valid, we obtain the following curve as showing the photometric intensity of coronal light at varying distances from the Sun's limb expressed in terms of Solar semi-diameters from Sun centre.

* The photographs show that no such variations in local intensity were present.

Fig. 7.



Curve showing relation between photometric intensity of coronal light and distance of corona from Sun's limb.

It will be quite obvious, from the character of this curve, that the diminution in intensity does not vary according to the law of inverse squares. To show the degree of departure we have calculated the intensity, on the basis of the law, at several points and compared them with the values taken from the curve at the same points.

Distances in Solar semi-diameters.	Photometric intensity.	
	Observed.	Law of inverse squares.
1·6	·066	·066
2·0	·053	·042
2·4	·043	·029
2·8	·034	·022
3·2	·026	·016
3·6	·021	·013

In comparing the observations of Lieutenant DOUGLAS and Lieutenant BAIRNSFATHER with those made during the 1878 Eclipse it must be remembered that the conditions of observation on the two occasions were widely different. The observations in the West Indies were made at the sea-level, in a perfectly humid atmosphere, and with the Sun at no greater altitude than 19°. Professor J. W. LANGLEY observed from the summit of Pike's Peak, which is 14,100 feet above the sea, in a relatively dry atmosphere, and with the Sun at an altitude of 39°. Dr. J. C. SMITH's position at Virginia City, Montana, was about 6,000 feet above the sea-level, and the Sun's altitude at the time of observation was about 44°. If we have regard then to the extinction of the coronal light in the Earth's atmosphere, it follows, *cæteris paribus*, that the observations during the 1886 Eclipse should show a much lower photometric intensity than those of 1878, even if the intrinsic brightness of the corona was the same on the two occasions.

It will be a matter of remark that the brightness of the corona at the various points measured is very inferior to that of the Moon's surface. The value of the light from the full Moon has been variously estimated, but it is not an unfair estimate to take it as ·02 of a candle at 1 foot distance. Supposing the whole surface of the Moon to be of equal brightness, the brightness of the Moon's image on the photometric screen used in the equatorial telescope compared with moonlight itself would have been very closely 60 × ·02 candle, or 1·2 candles. No matter what part of the image fell on the grease spots, it would have required this illumination to have made the grease spots disappear. If we take the highest reading of the corona measured, we find that it is ·07 of a Siemens unit, or about ·06 candle. It thus appears that the brightness of the brightest measured part of the corona (1·55 Solar semi-diameters) was 200 times less bright than that of the surface of the Moon, whilst the furthest spot at 3·66 Solar semi-diameters, having a value of ·019 Siemens unit, or ·015 candle, was only $\frac{1}{800}$ of the brightness.

The highest value of the coronal light, measured in the ordinary photometer, was about ·02, or equivalent to that of the full Moon. It is evident therefore that, even when taking into account the greater angular area which the corona occupied, the brightness of the corona was very much greater close to the limb than elsewhere. The photographs which were taken show this fact. A dense image of the Moon might be secured on plates such as were used at the eclipse in the $\frac{1}{20}$th of a second. The photographs which were taken of the corona varied in exposure from half a second to 100 seconds. Even with the first named exposure the corona close to the limb was much over exposed, showing the intense brightness of that part, whilst the image of the highest part of the corona measured was hardly visible. Owing to the variable quantities of cloud during totality, it would be unfair to try to make any comparison between the brightness of the corona at the poles and at the equator, though, had no cloud intervened, the arrangements adopted would have enabled such to be done. Nor is it worth while to endeavour to establish any law for the decrease of brightness of the corona in terms of its distance from the limb. We have, however, in the measures given, obtained results which will be of use in comparing the brightness of the corona on this occasion with that of other future eclipses; and we believe that measurements of the brightness obtained by the plan adopted, or by photography, will become a necessary observation in determining what connection, if any, the Sun-spot periods have with the coronal phenomena.

We have to express our acknowledgments to Mr. H. A. LAWRANCE for the very ready and efficient aid which he rendered in mounting the equatorial, and in assisting in making the photometric measurements.

To Captain ARCHER and his officers, Lieutenants DOUGLAS and BAIRNSFATHER, we are also greatly indebted for the lively and eminently practical interest which they manifested in the work of the expedition generally, and especially for their zealous co-operation in the particular observations which had been entrusted to us. Indeed, the entire crew of the "Fantôme" laboured in the most willing and cheerful manner, often under circumstances of considerable personal discomfort, to promote the success of the expedition in every possible way.

APPENDIX.

Observations made by NEWCOMB'S *Method on the Visibility of Extension of the Coronal Streamers at Hog Island, Grenada, by Commander* ARCHER, *R.N.*

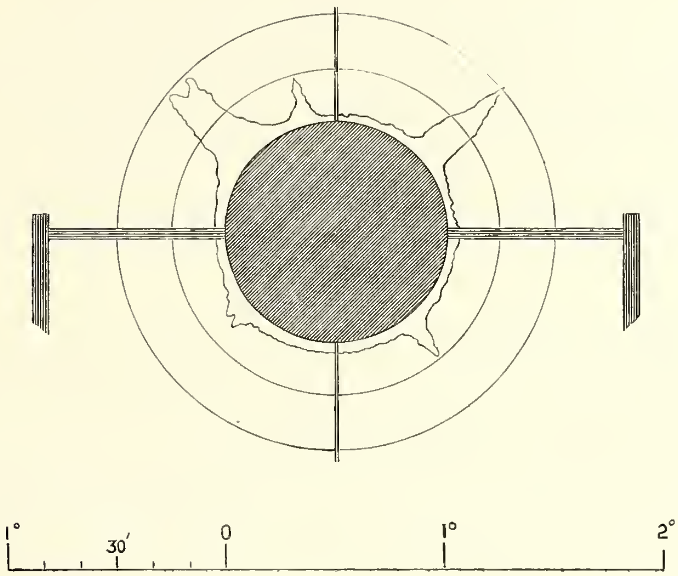
1. A 9-inch disc was erected on a pole fastened to the N.W. corner of the observatory, much in the manner suggested in the instructions; the distance of the eye-piece from a plumb-line hanging under the centre of the disc frame was exactly 40 feet.

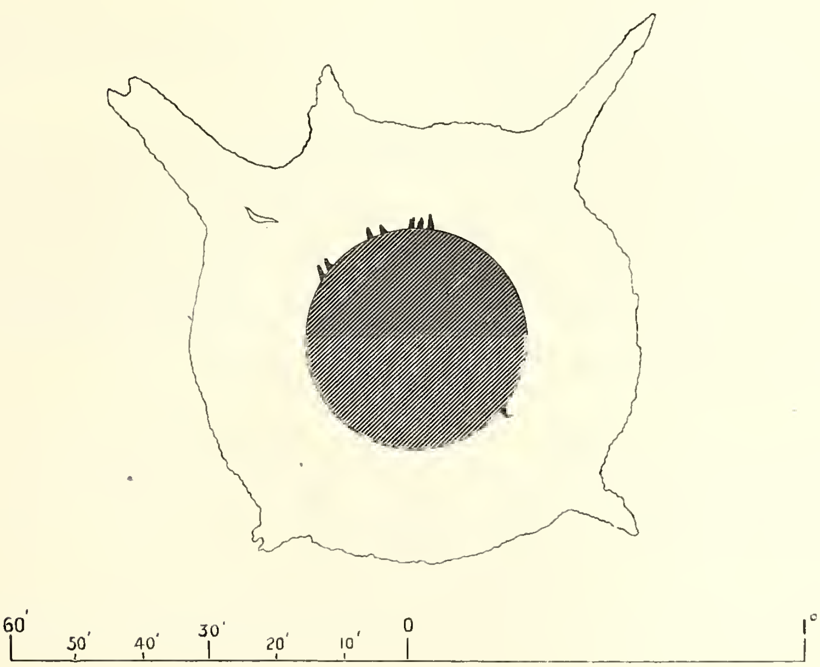
2. As a means of measuring the length of the streamers of the corona, I had fitted two copper circles concentric with the disc, supported on a light iron cross; the diameter of the inner circle was $1\frac{1}{2}$ disc diameter ($13\frac{1}{2}$ inches), that of the outer one 2 disc diameters (18 inches).

3. In order to localise the position of the streamers, I decided to look upon the disc as a compass card, with the North point vertically upwards; the arms of the iron cross therefore indicated the cardinal points.

4. The iron crossbar which carried the eye-piece I had lengthened to 18 inches, in order to increase the limits of adjustment, and also to admit of the Sun being observed when at the proper altitude (18° 48′) on several preceding days; owing to this circumstance, I feel considerable confidence in the Sun's having been nearly concentric with the disc at the beginning of totality, as on two previous days I timed the exact moment when the Sun's centre was behind the centre of the disc, and found the calculated altitudes at the time were between 18° 48′ and 18° 49′, the calculated altitude at commencement of totality being 18° 47′ 51″. The position of the eye-piece was adjusted on the previous day, and required no further alteration.

5. I found the concentric wire circles of great advantage, as the streamers, when judged by eye alone, appeared to be much longer than they really were ; and, had it not been for this device, I feel sure I should have over-estimated the distance to which they extended.





6. Half an hour before the commencement of totality I sat down in the shade, and a quarter of an hour before I had my eyes bandaged with a double thickness of black cloth ; on the signal being given, I removed the bandage and observed through the

eye-hole a narrow fringe of very bright white light all round the wooden disc, with marked extensions to the N.E. and N.W., which both just reached the outer wire, though the outer portion was very faint. The N.W. one was rather forked, the N.E. one pointed. To N.N.W. was a fainter thin extension, which reached to the first wire ; to S.E. was a similar one, also reaching to the first wire ; and to S.W. was a very small double one, hardly reaching half way to the first wire.

7. I then looked through a ship's telescope I had on a stand near me, and I observed red prominences under the N.W., N.N.W., and S.E. extensions, and also some prominences at North. To N.W. was a long tongue of flame about the colour of a candle flame, which appeared to be disconnected from the Sun's limb, and to extend to about 10′ from the limb ; this estimation was made by aid of the afore-mentioned wires.

8. On a cloud passing over towards the end of totality I looked at the stars overhead, but there were so many in sight that I could not pick out what they were ; I should say that, judging by the contrasts between their brightness, some of them must have been of the third or fourth magnitude.

9. At the eye-hole a linear inch on the disc subtended an angle of 68 minutes ; thus, supposing the Sun were concentric with the disc at the beginning of totality, the streamers to the N.W. and N.E. would be about 45′ from the Sun's limb, those to N.N.W. and S.E. about 30′, that to S.W. about 22′, and the general ring of light of the corona probably about 18′.

10. I observed a light film of cloud to be passing over the corona for several seconds before it was hidden by the cloud.

11. Explanation of the diagrams (p. 383) :—

The upper figure represents the wooden disc with its supports and wires, and the irregular ring of the corona and streamers showing round it, as seen at the commencement of totality. The second figure is a reproduction of the first on a larger scale, with the disc removed ; the dark circle represents the Moon, and the black dots show the prominences that were noticed. A scale of degrees and minutes is attached to each figure. The vertex is towards the top of the paper.

ROBERT H. ARCHER, *Commander.*

H.M.S. "*Fantôme,*" *Grenada,*
30*th August,* 1886.

XII. *Report of the Observations of the Total Solar Eclipse of August* 29, 1886, *made at Grenville, in the Island of Grenada.*

By H. H. TURNER, *M.A., B.Sc., Fellow of Trinity College, Cambridge.*

Communicated by the Astronomer Royal.

Received February 23,—Read March 15, 1888.

I. GENERAL ARRANGEMENTS.

THE eclipse party was landed at St. George's, Grenada, on Thursday, August 12. On Saturday, August 14, I proceeded with Professor TACCHINI to Grenville, a village on the east coast, near which it had been decided that we were to take up our stations. Our instruments and baggage, with two huts constructed by a local carpenter at St. George's, were placed on a sloop which was towed round the coast as far as Grenville Bay by H.M.S. "Fantôme"; but, as it was unadvisable for a large ship to attempt to enter the bay, the sloop went in alone, and deposited its freight on the jetty. I proceeded to Grenville by the Grand Etang Pass, which runs nearly east and west across the island from St. George's to Grenville, being anxious to determine whether there was any chance of returning with heavy baggage by the overland route. However, it soon became evident that this would be very difficult, if not impossible, the pass being little more than a bridle-path in certain portions.

The party—consisting of Professor TACCHINI and myself, Lieutenant SMITH, of H.M.S. "Sparrowhawk," a quartermaster from the same ship, and an artificer from H.M.S. "Fantôme"—found very comfortable quarters at Boulogne, the house of Colonel DUNCAN, which is situated about two miles from Grenville Bay, along a fair road.

After some delay, caused by very heavy rain on Monday, August 16, which made the roads nearly impassable for a time, the huts and instruments were carted from the bay to Boulogne, and erected very quickly on a site close to the house, longitude 4h. 6m. 30s. W., latitude 12° 8′·5 N. One bell tent was also erected for contingencies, though this was almost unnecessary, as our instruments were unpacked and the cases left in the "buchan," a kind of superior barn for storing cocoa, which was then nearly empty and was kindly placed at our disposal by Mr. ST. GEORGE, Colonel DUNCAN'S representative.

The huts, which had been constructed before our arrival under the direction of his Excellency Governor SENDALL, required very little modification. They were ten feet square, with a gabled roof, the ridge running north and south. The door for entrance was in the western face. The eastern roof consisted almost entirely of two shutters, which could easily be thrown open with a pole; and, after this, a large portion of the eastern face (which, in my own case, was enlarged still further) could be opened, turning about hinges which ran horizontally across the face. This gave practically a clear view eastwards from the sea horizon up to the zenith, so that it was possible to work on the Sun from sunrise to midday. The instruments inside were further protected from drippings by mackintosh covers. My own instrument consisted of the Simms equatorial No. 1, with a grating spectroscope; and a 4-inch telescope by SIMMS, mounted as a part-counterpoise on the same polar axis. The whole weight was about 15 cwt.; and, as my work was not to be photographic, and no very great steadiness was necessary, it was found sufficient to place the base plate (an iron plate 3 feet in diameter) on the soil, after removing the turf.

After Tuesday, August 17, the mornings were generally fine, and everything was soon in order for observation. Lieutenant SMITH rated the chronometer (ARNOLD and DENT, 965) by sextant observations of equal altitudes of the Sun with an artificial horizon; the local time of totality was found to be correct within a few seconds.

In taking a photograph of the portion of the spectrum under observation, some difficulty was found in preventing the film from becoming detached during development or washing. I found at last that drying the plate thoroughly by the stove before taking the photograph prevented this detaching.

The night of August 28–29 was beautifully fine, but at sunrise clouds began to gather, and the observations of the eclipse were made under considerable difficulties. The following notes will give some idea of the circumstances:—

	h.	m.	s.	
From sunrise to	6	30	0	Cloudy.
	6	30	0	Sun appeared.
	6	31	30	Cloudy.
	6	33	15	Clear.
	6	34	15	Cloudy.
	6	41	20	Clear.
	6	45	30	Slight shower; Sun visible.
	6	47	0	Shower passed.
	6	48	0	Another shower.
	6	49	5	Shower passed.
	6	52	20	Showery.
	6	55	0	Shower passed.
	6	56	30	Cloudy.
	7	0	20	Clear.
	7	10	10	Cloudy for ten seconds.
	7	12	0	Totality.

h.	m.	s.	
7	14	30	Light cloud passing.
7	15	5	Quite cloudy.
7	15	50	End of totality; cloud lifted for five seconds.
7	16	0	Quite cloudy.
7	20	0	Clear.
7	23	15	Cloudy.
7	24	10	Clear.
7	25	17	Clouded up for some time.

After the eclipse the instruments were quickly dismounted, and, with the huts, were placed on board a small steamer, the "Waltham," which, on Wednesday, September 1, conveyed us back round the south coast to St. George's, stopping at Prickly Point on the way, to pick up a lighter containing the instruments and baggage of the southern party. I returned to England with some other members of the expedition by the mail which left Grenada on the following Sunday.

II. Observations.

(a) *The Order of Appearance of Certain Bright Lines of the Chromosphere and Inner Corona.*

In the programme arranged before the expedition left England, I was directed to attempt the confirmation of Mr. Lockyer's observations before and after totality during the Egyptian eclipse of 1882. These are described briefly, with promise of further details, in the 'Roy. Soc. Proc.' for 1882 (pp. 291 *et seq.*), and it will be sufficient here to reproduce the following paragraphs from this paper for convenience (*ibid.* p. 296, §§ 10–13). The observations were intended as a test of two rival hypotheses.

"On the old hypothesis the construction of the Solar atmosphere was imaged as follows:—

"(1) We have terrestrial elements in the Sun's atmosphere.

"(2) They thin out in the order of vapour density, all being represented in the lower strata, since the Solar atmosphere at the lower level is incompetent to dissociate them.

"(3) In the lower strata we have especially those of higher atomic weight, all together forming a so-called 'reversing layer, by which chiefly the Fraunhofer spectrum is produced.

"The new hypothesis necessitates a radical change in the above views. According to it, these three statements require to be changed, as follows:—

"(1) If the terrestrial elements exist at all in the Sun's atmosphere, they are in process of ultimate formation in the cooler parts of it.

"(2) The Sun's atmosphere is not composed of strata which thin out, all substances being represented at the bottom; but of true strata, like the skin of an onion, each different in composition from the one either above or below.

"(3) In the lower strata we have, not elementary substances of a high atomic weight, *but those constituents of all the elementary* bodies which can resist the greater heat of these regions.

* * * * * * *

"It was stated in (6): While discussing the conditions of observation, that whether we were dealing with strata of substances extending down to the Sun, or limited to certain heights, the spectral lines would always appear to rest on the Solar spectrum, and that the phenomena would *in the main* be the same. This, however, is true in the main only; there must be a difference, and this supplies us with a test between the rival hypotheses of the greatest stringency.

"For take three concentric envelopes, A, B, C, so that only A rests on the photosphere, B rests on A, and C on B. The stratum B, being further removed from the photosphere than the stratum A, will be cooler, its lines will be dimmer, and the lines of C will be dimmer than the lines of B, and so on. So, if we could really observe the strata, *the longer a line is, i.e.*, the greater the height at which the stratum which gives rise to it lies, *the dimmer the line will be.*

"Now, our best chance of making such an observation as this is during a total eclipse. We do not see the lines ordinarily, in consequence of the illumination of our air. As during an eclipse, before totality, the intensity of this illumination is rapidly diminishing, the lines first visible should be short and bright, and should remain short, while the new lines which become visible as the darkness increases should be of gradually increasing length.

"Further, the short lines which first appear should be lines seen in prominences and not in spots, and relatively brighter in the spark than in the arc, while the longer lines added should be lines affected in spots and *not* in prominences."

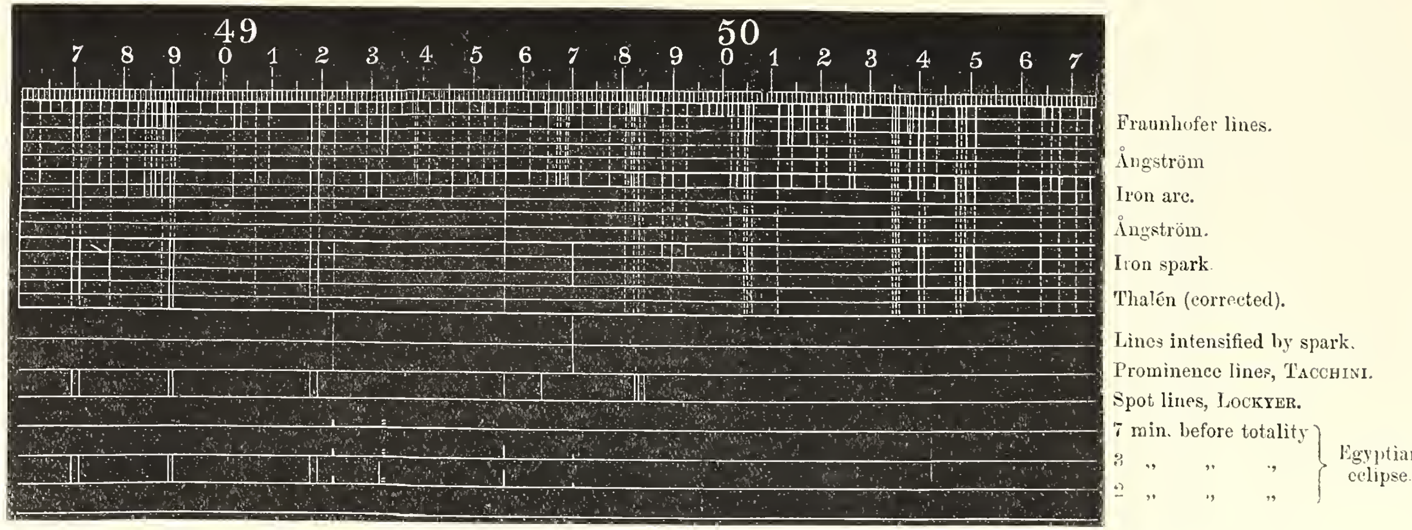
The manner in which these expectations were realised is shown at once by the subjoined diagram from the same paper (fig. 1).

For convenience of reference I have used small letters to denote the lines shown in the lowest line of the diagram, beginning with *a* on the left. Thus—

The two	lines	at	4870–71	are	called	*a* and *b*.
,, ,,	,,	,,	4890	,,	,,	*c* and *d*.
,, three	,,	,,	4918–23	,,	,,	*e*, *f*, and *g*.
,, two	,,	,,	4932–33	,,	,,	*h* and *i*.
,, one	,,	,,	4956	,,	,,	*k*.
,, ,,	,,	,,	4970	,,	,,	*l*.

In this nomenclature, then, lines *g* and *l* are seen by TACCHINI in prominences,

Fig. 1.



The Ba line at λ 4933·4 is a line seen thirty times by YOUNG at the maximum spot period, and not recorded by TACCHINI at the minimum. The lower longer line not seen till five minutes afterwards is a Te line λ 4932·5.

and the other lines (excepting *h* and *i*) are seen by LOCKYER in spots. In the Egyptian eclipse—

g and *i*	were seen 7	minutes before totality ;	
k and *l*	,, ,, 3	,, ,,	,,
and all the other lines	,, ,, 2	,, ,,	,,

In making my own observations, the slit of the spectroscope was placed radial to the limb at the cusp some minutes before totality. The point of the cusp was brought on to the slit, and the motion of the Sun allowed to carry it gradually away. The Solar spectrum, as seen in the spectroscope, thus became narrower as the point of the cusp approached, and finally disappeared. It was hoped to catch the bright lines just at this point, the dark lines of the vanishing Solar spectrum serving as a set of fiducial marks for the identification of the bright lines. But there was also inserted in the focus of the eyepiece a photograph of this region of the spectrum taken the day before. Some twenty minutes before totality it was suspected that, in the increasing darkness, the lines of the photograph would not be well seen, and the places of the lines were therefore carefully marked by scratching the film with a sharp penknife ; the scratches were seen to accord well with the proper lines in the Solar spectrum. But in the actual observations it was not found necessary to refer to the photograph, and the identifications mentioned below were referred to the vanishing spectrum. For the period of the observations the cusp does not travel very rapidly along the limb,* so that the slit was nearly radial until a few seconds before totality. I now subjoin the observations, as recorded at the time by an assistant to my dictation.

Time by chronometer. h. m. s.	
6 55 30	No bright line visible at cusp.
6 56 30	Cloudy ; no bright line visible up to the time clouds appeared.
7 0 20	Clear again.
7 7 45	F line appeared.
7 8 55	*g* appeared : very short.
7 10 10	Cloudy for ten seconds.
7 11 30	*g* and *i*.
	Immediately after many lines appeared.
7 12 0	Totality.
7 15 50	End of totality ; cloudy.
7 20 50	Only F, *g*, and *i* visible at times.

* The rate of motion is very nearly inversely proportional to the duration of totality. For a three-minute eclipse the angular distances of the cusp from the point of disappearance are respectively 75°, 60°, 51°, $33\frac{1}{2}$°, at $4\frac{1}{2}$ mins., 90 secs., 54 secs., 18 secs. before totality.

7	21	45	*g* still suspected, and *k*.
7	22	28	*k* certainly visible.
7	23	15	Cloudy.
7	24	10	Clear again.
7	24	42	No line visible.
7	25	17	Cloudy for some time.

In addition, I wrote the following note immediately afterwards :—

"Just before totality all the lines were glimpsed, but all were much shorter than was expected; *g* and *i* were certainly seen before this, but no others. F was very bright from time noted, and prominent after totality. After totality was over, cloud still remained for nearly a minute. Then F line only was prominent, but *g* and *k* were afterwards seen; *i* was not seen; *k* appeared to remain longest."

The telescope used was one by SIMMS, of 6 in. aperture and 6 ft. focal length. To this was attached a grating spectroscope by HILGER; collimator, $1\frac{1}{2}$ in. aperture and 18 in. focal length; telescope, $1\frac{1}{2}$ in. aperture and 9 in. focal length; magnifying power of eyepiece, 10. The grating kindly lent to me by Captain ABNEY was by RUTHERFURD, and contained 17,200 lines to the inch. The second order of spectrum was used.

It will be seen that these observations confirm Mr. LOCKYER'S in only a few points. The phenomena were apparently of greater intensity in Egypt; the bright lines *g* and *i* appeared there 7 minutes before totality, the first of which I saw 3 minutes and the second only 30 seconds before totality. The second stage of the appearance of *k* and *l* in the Egyptian observations is not represented at all in my own, being indistinguishable both from the third stage and from the general flashing out of many bright lines which took place just before totality, and is represented in my own notes by "many lines appeared."

Bright lines are recorded after totality for nearly 6 minutes; and, as the appearance of *k* was unexpected, it was looked at carefully, with the result noted, "*k* certainly visible." I could not help feeling some doubt afterwards as to the observation; and it is possible that it is really spurious, and due to the straining of the eyes, to the imagination, or other causes. But it is only differentiated from the other observations by the fact of coming rather later in a somewhat exciting half-hour, and I have left the record untouched. It is perhaps only natural that the clear atmosphere of Egypt and the great altitude of the Sun, as compared with the vapour-laden air of the West Indies and an altitude of only 18°, should modify the particular phenomena under discussion. And, again, it must be remembered that in 1882 the Sun's activity was nearly at its maximum; whereas in 1886, judging at least by the paucity of spots, the minimum was appearing somewhat prematurely. This may have had a considerable effect on these phenomena.

This diminution of the period of time over which the phenomena were distributed, and especially the concentration of the later changes in the spectrum into the space of a few seconds, made a most essential difference in the observation. The record of the Egyptian eclipse gave rise to the hope that the observations could be made somewhat leisurely—which, indeed, is almost essential if the eye is to carefully compare faint lines (the few early lines were too faint on this occasion to be held steadily by the eye, but could be seen by glimpses in the manner familiar to observers of faint objects) in different parts of the field. Instead of this, I found that the phenomena were sudden, and, with the few exceptions mentioned, the change which the spectrum underwent was confined to what has been called "the flash," as I understand it. Mr. LOCKYER seems to have had the good fortune to see this "flash" in stages, extended for analysis. For a complete confirmation of his results it is possible that better conditions may be necessary than those of the 1886 eclipse.

III. OBSERVATIONS DURING TOTALITY.

During totality I had been directed by the Committee to examine the corona, with a view to the detection of currents. For this purpose, a 4-in. telescope by SIMMS was attached to the same mounting as the 6-in. used for the spectroscope. The power used was 140. With this instrument I made a careful examination of the corona all round the limb. It did not seem to me to vary essentially in appearance from point to point. The structure was radial, and on following the rays outwards from the limb I could not detect any appearance of curvature, to join another ray in the form of a loop. I believe such forms were represented in some of the naked-eye drawings. The great prominence was a striking feature; it seemed to me of a rosy tint throughout. Concerning the particular object of the search—indications of any sort of current—I can only report a negative result.

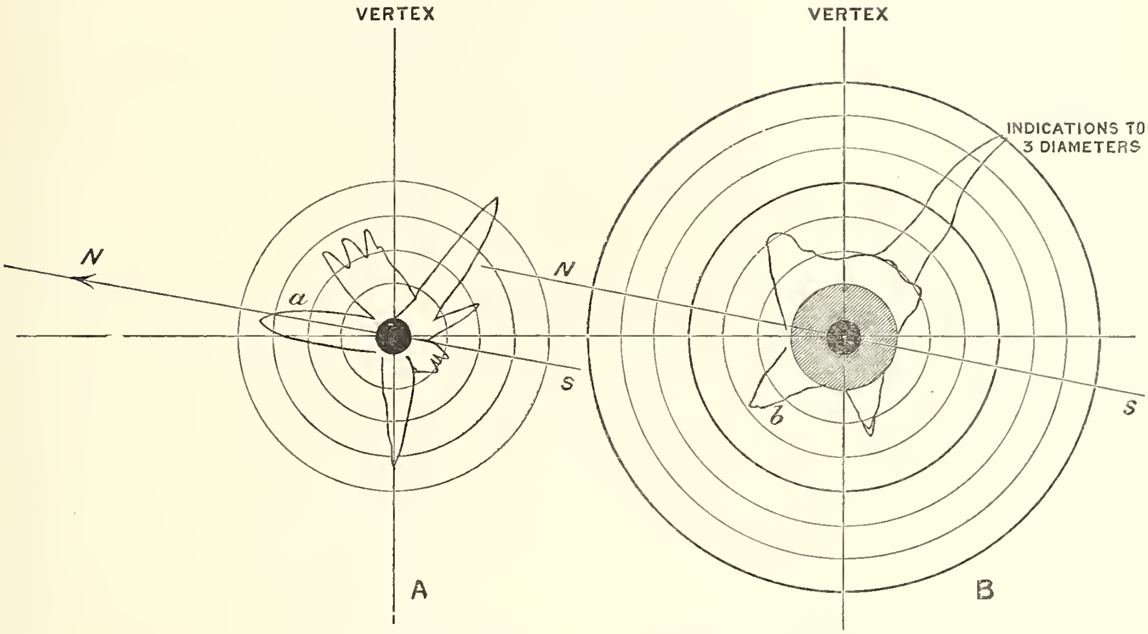
There was, to my eye, scarcely any distinguishing feature in *structure* by which the orientation could be recognised; though some of the rays were, of course, longer than others—that is to say, I looked specially for the structure characteristic of the poles, and failed to notice any very marked difference from the structure in other parts of the circumference.

I then returned to the spectroscope, with a view to examining the brightness of the lines at different distances from the limb; but the eye examination had taken some considerable time, and the clouds which obscured the Sun for the last minute of totality were already approaching.

IV. DRAWINGS.

The drawing marked A was made by Mr. ST. GEORGE with an opera glass. His eyes were not specially made sensitive before totality.

The drawing B was made by Lieutenant SMITH, of H.M.S. "Sparrowhawk," from naked-eye observations; but his head was wrapped in a black mackintosh 15 minutes before totality commenced.



Mr. St. George's drawing. Lieutenant Smith's drawing.

The black centre represents the Moon; the shaded circle in B represents the disk (three times size of Moon) which obscured the brighter portions of the corona.

Long. of station, 4h. 6m. 30s. W.; lat. 12° 8′·5 N.

No attempt is made in either drawing to give details, but merely the distances to which coronal extension could be traced, as estimated in diameters of the central black disk, which in the first case represents the Moon, and in the second a disk which was so placed as to screen the brighter portions of the inner corona from the observer's eyes, and subtended an angle of 3 diameters. It will be seen that the chief discrepancy in the drawings is in the orientation of the rays marked respectively *a* and *b*, in one of which there would seem to be some error; otherwise the correspondence is remarkably good, except that Lieutenant Smith obviously traced the extension much further than Mr. St. George. It may be mentioned that special rehearsals were conducted on the two days before the eclipse, in drawing on such skeleton forms of concentric circles pictures of coronæ held before the eye for $3\frac{1}{2}$ minutes. The two gentlemen mentioned above were found to reproduce the direction of the rays very accurately, and, as regards distance, Mr. St. George seemed to be liable to slightly over-estimate the extensions.

XIII. *Revision of the Atomic Weight of Gold.*

By J. W. MALLET, *F.R.S.*, *Professor of Chemistry in the University of Virginia.*

Received April 15,—Read May 9, 1889.

UNTIL lately gold ranked among the elementary substances of which the general properties had been well ascertained, but in regard to the atomic weights of which our knowledge was least satisfactory. That this constant should be determined as accurately as possible for gold was desirable in view of its bearing on the precise place assigned the metal in the "periodic" classification of the elements based on the ideas of NEWLANDS, ODLING, MENDELEJEFF, and L. MEYER. Furthermore, an exact knowledge of the atomic weight of gold might be conveniently applied in the determination of the atomic weights of some of the other elements. A practical laboratory reason for desiring to possess a trustworthy value for this constant was also presented by the facility with which gold compounds of many organic substances may be prepared, and the ease with which their composition may be ascertained by simple ignition in the air and weighing of the residual gold, the results leading to a knowledge of molecular composition when the atomic value of the weight of the metal obtained is assumed to be known.

For the last three years and a half I have been occupied, during a large part of such time as has been available for original work, in devising and carrying out experiments aiming at the redetermination of the constant in question. The difficulties met with have been greater than were at first looked for, and have led to much time and labour being consumed in attempts to overcome them. About two years ago, when this work was already well under way but still in progress, there appeared the results of experiments aiming at the same end, by KRÜSS in Germany and by THORPE and LAURIE in England—experiments made with the care and accuracy of modern methods, and apparently deserving of much confidence. My own work, however, was continued, as we cannot have too many careful independent determinations of atomic weights by different workers, and as I had used to a considerable extent other procesess than those on which the newly published determinations were based, while the chemists named had employed, in the main, one and the same method. A preliminary notice of my work was read in the Chemical Section of the British Association at the Manchester meeting of 1887. The details of my experiments and the results which I have reached are now laid before the Royal Society.

Earlier Determinations of the Atomic Weight of Gold.

In the work of L. MULDER, 'Historisch-kritisch Overzigt van de Bepalingen der Æquivalent-Gewigten van 24 Metalen,' Utrecht, 1853, and in the recent papers of KRÜSS and of THORPE and LAURIE, there are abstracts of reports upon a number of experiments by chemists of the earlier part of this century bearing on the value to be assigned to the atomic weight of gold, such as those of PROUST,* RICHTER,† DALTON,‡ THOMSON,§ OBERKAMPF,‖ PELLETIER,¶ FIGUIER,** and JAVAL;†† but of these none deserve any attentive consideration at the present day, the methods used having in some cases been such that accurate results could not be expected from them, and the actual figures obtained in other cases differing so widely from each other that no importance can be attached to them.

Before the year 1887 but two chemists—BERZELIUS and LEVOL—had published the results of experiments furnishing fairly admissible data for calculating the atomic weight in question.

A. *Experiments of* BERZELIUS, 1813.‡‡—The method adopted was the precipitation of a solution of auric chloride by metallic mercury, determining the quantities of mercury dissolved and gold thrown down. In the original paper but a single experiment is reported, but later the author appears to have made a second,§§ so that for the two MEYER and SEUBERT, in their recalculation of the atomic weights of the elements,‖‖ give as the sums of the amounts of mercury and gold found to be equivalent to each other 24·240 grm. of the former and 15·912 grm. of the latter. Taking these quantities to represent the ratio between the weights of three atoms of mercury and two atoms of gold, we have for the weight of the single atom of the latter ($H = 1$)

If we assume $Hg = 199{\cdot}8$ (L. MEYER and SEUBERT‖‖). . 196·73
„ „ „ 199·712 (F. W. CLARKE¶¶). . . . 196·65

This method recommends itself as advantageous on several grounds, and the experiment deserves repetition as soon as the atomic weight of mercury becomes

* 'Journal de Physique,' vol. 62, 1806, p. 131; 'N. GEHLEN, Journal,' vol. 1, 1806, p. 477.

† 'SCHWEIGGER, Journal,' vol. 7, 1813, p. 43.

‡ 'New System,' part 2, 1810, p. 253.

§ 'First Principles,' vol. 1, 1825, p. 440.

‖ 'Annales de Chimie,' vol. 80, 1811, p. 140.

¶ 'Annales de Chimie,' vol. 15, 1820, pp. 5, 113.

** 'Annales de Chimie,' vol. 19, 1821, p. 177.

†† 'Annales de Chimie,' vol. 17, 1821, p. 337; 'SCHWEIGGER, Journal,' vol. 33, 1821, p. 238.

‡‡ 'Stockholm, Kgl. Vetensk. Akad. Handl.,' 1813, p. 185.

§§ BERZELIUS, 'Lehrbuch,' vol. 3, p. 1212, str. 70.

‖‖ LOTHAR MEYER u. KARL SEUBERT, 'Die Atomgewichte d. Elemente aus d. Originalzahlen berechnet,' Leipzig, 1883, p. 191.

¶¶ "The Constants of Nature," 'Smithsonian Miscellaneous Collections,' Washington, D.C.

known with greater certainty than at present. But until this condition is fulfilled the result for gold cannot be depended upon as of the first rank in exactness. In any renewed attempt to apply this method several questions would have to be examined as to the precise nature of the solution used, and of the reaction itself.

[BERZELIUS* also precipitated gold by means of a known quantity of phosphorus from a solution of the chloride used in excess, and his results, as calculated by F. W. CLARKE,† lead to the atomic weight 195·303 for gold; but this process appears ill adapted to give very exact results, even in such hands as those of the great Swedish chemist, and the value obtained is certainly too low in the light of more modern researches.]

B. *Experiments of* BERZELIUS, 1844.‡—In these experiments potassium auri-chloride, which, it was found, could not be completely dried without loss of chlorine, was ignited in hydrogen, and the residue was treated with water to dissolve potassium chloride, the quantity of which was determined, as well as that of the metallic gold left undissolved. Five experiments were made, and the aggregate amounts obtained of potassium chloride and gold were 3·7800 grm. and 9·9685 grm. respectively. These figures, if we assume K = 39·03 and Cl = 35·37, give for the atomic weight of gold 196·20, the lowest result from one of the individual experiments being 196·11, and the highest 196·27.

Among possible sources of error in this process we may note as deserving consideration the conceivable retention by the potassium auri-chloride of hydrogen auri-chloride, and the difficulty of directly determining with accuracy the potassium chloride extracted by water. The former would lead to a higher result for gold than should be obtained; the latter might either give too low a result in consequence of imperfect drying, or too high if there were partial loss by volatilization, either during the ignition in hydrogen or in subsequently recovering the potassium chloride from solution. The quantities of material used were smaller than is probably desirable.

C. *Experiments of* LEVOL, 1850.§—A weighed quantity of gold was dissolved as auric chloride, the metal reduced from the solution by means of sulphur dioxide, and the sulphuric acid formed was determined as barium sulphate. In two experiments, reported as giving exactly the same result, 1 grm. of gold gave 1·782 grm. of barium sulphate. Hence, if Ba be taken = 136·86, S = 31·98, and O = 15·96, we have for the atomic weight of gold the number 195·86.

Of the sources of error to which this method is liable probably the most important are atmospheric oxidation of sulphurous to sulphuric acid and imperfect washing out of soluble compounds of barium from the barium sulphate. Both would tend to give too low a result for gold.

* BERZELIUS, 'Lehrbuch,' 5. Aufl., vol. 3, p. 1188.

† "The Constants of Nature," 'Smithsonian Miscellaneous Collections,' Washington, D.C.

‡ BERZELIUS, 'Lehrbuch,' 5. Aufl., vol. 3, p. 1212.

§ 'Annales de Chimie,' [3], vol. 30, p. 355.

For all these earlier experiments details are wanting as to the exact mode of purification of the gold and other materials used, and in the weighings there appears to have been no correction introduced for atmospheric buoyancy; the results doubtless represent apparent, not absolute, weights.

[There is also to be quoted the statement of JULIUS THOMSEN,* that he found in hydrogen brom-aurate ($AuBr_3.HBr.5H_2O$) 32·11 per cent. of gold and 52·00 per cent. of bromine, from which he concluded that Au = probably about 197. Taking Br = 79·76, and calculating from the ratio of Br_4 : Au, the number is 197·01.]

Recent Careful Determinations of the Atomic Weight of Gold.

A. *Experiments of* GERHARD KRÜSS, 1886.†—The author has described in detail the means resorted to for the preparation of pure metallic gold, and especially for its separation from silver and the metals of the platinum group, with an account of the spectroscopic examination of the gold employed. He has then given a full account of:—*a.* His determinations of the gold and chlorine (the former reduced by a stream of sulphur dioxide; the latter precipitated and weighed as silver chloride) in a neutral solution of auric chloride, prepared by the action of water on the so-called auro-auric chloride (Au_2Cl_4),‡ itself prepared by the direct action of chlorine on metallic gold; *b.* Like determinations of gold and chlorine in sublimed auric chloride, made by direct action of the elements on each other with careful regulation of the temperature; *c.* Determinations of the gold in a weighed quantity of potassium auri-bromide ($KAuBr_4$), the metal in some experiments reduced from a solution of the salt by sulphurous acid, in others reduced from the dry salt by heating in a stream of hydrogen; *d.* Determinations of the gold and bromine (the former thrown down by sulphurous acid; the latter precipitated as silver bromide) in the same salt, potassium auri-bromide; *e.* Determinations of the loss of weight (representing 3 atoms of bromine for 1 of gold) undergone by heating potassium auri-bromide gradually to 320° C., towards the end in a stream of hydrogen; *f.* Determinations of the quantity of potassium bromide recovered from the residue left in the experiments of *e* by treatment of this residue with water, separation of the metallic gold, careful evaporation of the liquid, and final cautious heating of the potassium bromide over a free flame. In the experiments of series *a* account was taken of the somewhat different processes of purification of the gold used, but, no corresponding differences

* 'Journ. Prakt. Chem.,' vol. 13, 1876, p. 345.

† 'LIEBIG'S Annalen,' vol. 238, p. 30; and separate publication, G. KRÜSS, 'Untersuchungen über das Atomgewicht des Goldes,' München, 1886.

‡ KRÜSS has in a later paper ('Berichte Deutsch. Chem. Gesell.,' vol. 20, p. 2634) denied the existence of auro-auric chloride as a definite compound, but admits that the substance so described by JULIUS THOMSEN yields on treatment with warm water a solution of pure neutral auric chloride, with separation of metallic gold.

being observable in the results obtained, no further record was made in the remaining series of the history of the gold used in these.

After correction of the weighings for atmospheric buoyancy in such cases as seemed to the author to involve a correction worth noticing, the following results were calculated from the figures obtained, these figures agreeing in general closely with each other in each series :—

Series *a*.	Mean of	8	experiments.	Atomic weight of gold = 196·622
,, *b*.	,,	4	,,	,, ,, = 196·143
,, *c*.	,,	9	,,	,, ,, = 196·741
,, *d*.	,,	5	,,	,, ,, = 196·743
,, *e*.	,,	4	,,	,, ,, = 196·619
,, *f*.	,,	4	,,	,, ,, = 196·620

Leaving out the results of series *b* on the ground of the very small quantity of sublimed auric chloride available, and the considerable discrepancy of one of the results (that in which most material was used) from the rest, the author calculates from the remaining 30 experiments the general mean 196·669; but, taking into account the greater or less closeness of agreement of the figures obtained by the several methods, he comes to the conclusion that 196·64 may better be assumed as the true atomic weight of gold. In these calculations Ag was assumed = 107·660, Cl = 35·368, Br = 79·750, and K = 39·040.

As regards possible sources of constant error in Krüss's experiments, it may be observed that—

1. In series *b* very small quantities of sublimed auric chloride were used—the whole amount available for all four experiments being only about seven-tenths of a gramme—and it is probable that a little free chlorine may have been physically retained by the chloride in spite of the long-continued passage over it of dry air. The experiment in which the largest quantity of material was used gave the atomic weight = but 194·79. On these grounds the author himself excludes the series from consideration in calculating his general mean.

2. In series *c*, *d*, and *e* the evidence is pretty strong, but perhaps not conclusive, to show that potassium auri-bromide can be rendered absolutely dry by exposure to air in a vessel containing phosphorus pentoxide, either at ordinary or higher temperatures, without, at the same time, undergoing any loss of bromine. The attainment of constant weight by the salt does not positively prove the entire removal of water. If moisture were retained the atomic weight of gold found would be brought out lower than it should be.

3. Krüss himself observed that in all cases in which he dissolved potassium auri-bromide in water a small residue of metallic gold was left, and, determining in a single experiment the amount of this (about ·05 per cent.), he used it as a corection for all

his results. As pointed out by THORPE and LAURIE* this partial decomposition of the salt was probably due to the action of dust from the air. If the results obtained from the solution were used, without any correction, to establish the atomic weight of gold, the tendency would of course be to a value lower than the truth. Although the correction introduced is small, it can hardly be supposed that it should be taken as constant in amount in all the experiments.

4. In series *e* it may be questioned whether traces of potassium bromide may not have been volatilized at the highest temperature used, or the residual potassium bromide may not have, to a small extent, exchanged bromine for oxygen while heated in air (before the use of the stream of hydrogen), the latter change being one to be guarded against whenever haloid salts are strongly heated in the presence of free oxygen. The tendency in both cases would be to a lower atomic weight for gold.

5. In series *f* there was risk of slight loss of potassium bromide during filtration and evaporation of its solution, and during exposure of the salt to the heat of a free flame, when there might possibly have been again slight replacement of bromine by oxygen, thus causing the atomic weight sought to come out too high, or else, on the other hand, risk of imperfect drying, which would give too low a value for the atomic weight in question.

On the whole it seems probable that the tendency of most of the constant errors to be suspected in connection with KRÜSS's experiments—experiments carried out with remarkable patience, skill, and apparent freedom from merely "fortuitous" errors—was in the direction of an atomic weight for gold somewhat below, rather than above, the true value.

B. *Experiments of* THORPE *and* LAURIE, 1887.†—In these experiments potassium auri-bromide was used, and determinations were made :—*a*. Of the weight of the residue left on heating the salt over a Bunsen flame till bromine ceased to be given off (this residue consisting of metallic gold and potassium bromide), and the weight of the gold left by such residue after all potassium bromide had been washed out of it by water ; *b*. Of the weight of silver necessary to be added as nitrate to the solution of potassium bromide obtained in *a* in order to just precipitate the bromine present ; *c*. Of the weight of the silver bromide so precipitated. All suitable experimental precautions seem to have been taken, and the weighings were corrected for atmospheric buoyancy. The individual results in each series agreed with each other even more closely than in KRÜSS's research.

The results obtained were as follows, using in calculation the numbers Ag = 107·66, Br = 79·75, and K = 39·03 :—

Series *a*.	Mean of 8 experiments.	Atomic weight of gold	= 196·876
,, *b*.	,, 9 ,,	,, ,, ,,	= 196·837
,, *c*.	,, 8 ,,	,, ,, ,,	= 196·842

* 'Chem. Soc. Journ.,' Dec., 1887, p. 866.

† 'Chem. Soc. Journ.,' June, 1887, p. 565, and Dec., 1887, p. 866.

The general mean of these values, giving equal weight to the different series, is 196·852.

As regards possible sources of constant error specially belonging to these experiments, it is to be noticed—

1. There is an advantage, as observed by the authors themselves, over the greater part of the experiments of KRÜSS in the nature of the relations employed not requiring that the potassium auri-bromide should be perfectly dry, the exact quantity of the original salt not needing in fact to be known.

2. In series *a* it is conceivable that there might have been slight volatilization of potassium bromide, or interchange in it to a small extent of bromine for oxygen, during the heating of the original salt, or retention of traces of potassium bromide by the metallic gold when washed—the latter but little probable. Any of these defects, if existing, would cause the method to give a higher value for Au than the true one.

3. In series *b* the probability seems to be in favour of not quite the whole of the original potassium bromide being actually used, and minute loss of silver solution having perhaps occurred, so that rather more of this solution was counted as used than the true quantity. If so, the former defect would tend to raise, the latter to lower, the atomic weight of gold.

4. In series *c*, in view of the evidence adduced to prove complete drying of the silver bromide, it is more likely that its weight as obtained was below, rather than above, the truth. Hence we should suspect, if any constant error exist, that it would rather tend towards an unduly high value for Au.

On the whole, there seems to be less reason to fear sources of constant error of any considerable amount in connection with the experiments of THORPE and LAURIE than with those of KRÜSS, and the drift is in the opposite direction, tending rather to give too high than too low a value for the atomic weight to be determined.

It should be mentioned that KRÜSS* has claimed that in the potassium auri-bromide used by THORPE and LAURIE there was probably as much free gold as he considered to exist in the salt used by himself, and on this assumption has calculated that the three series of experiments by the English chemists should, if corrected on this account, lead to the numbers 196·616, 196·559, and 196·575 respectively for Au. From this conclusion the latter chemists altogether dissent,† and express their confidence that in none of the preparations used by them was there free gold sufficient to account for the difference between their own results and those of KRÜSS.

* 'Berichte Deutsch. Chem. Gesell.,' vol. 20, p. 2365.

† 'Chem. Soc. Journ.,' Dec., 1887, p. 868.

General Results of Former Determinations most deserving Confidence.

These recent researches, unquestionably by far the most valuable up to the present time, give us, when taken separately and together, the following values for the atomic weight of gold :—

1.	General mean of 5 series by KRÜSS, as calculated by himself .	196·640
2.	,, ,, 3 series by THORPE and LAURIE, as calculated by themselves	196·852
3.	,, ,, 1 and 2, giving equal value to each	196·746

Difficulties to be overcome in Determining the Atomic Weight of Gold.

Besides the special difficulties connected with each method which may be adopted, the determination of any high atomic weight with a degree of accuracy which enables the result to be accepted to a given decimal place is clearly a much less easy matter than would be the attainment of an apparently equal degree of precision for an atomic weight represented by a small number. In obtaining the atomic weight of lithium, the first with which, many years ago, I had any personal experience, a difference of unity in the first decimal place corresponded to about $\frac{1}{70}$th of the whole value considered to be correct. In getting the atomic weight of aluminum, worked on later, a like difference represented approximately $\frac{1}{270}$th of the whole value. But, in the case now considered, of the atomic weight of gold, unity in the first decimal place means but about $\frac{1}{1970}$th of the whole value. So that, looking at the matter in this light, it may be said that a degree of precision is demanded more than seven times as great as in the case of aluminum, and twenty-eight times as great as in the case of lithium.

There is also to be noticed, as the most obvious *general* difficulty to which all methods for determining the atomic weight of gold are more or less exposed, the instability of compounds of this metal ; not merely the ease with which complete decomposition occurs, with separation of free gold, but the much more insidious and less easily detected trouble arising from the comparative ease with which aurous pass into auric compounds, and the reverse.

New Experiments by the Author.

The general difficulties just alluded to, and the special points to be investigated in regard to each method of determination tried, have demanded much time and work, and I cannot feel even now that all has been done that is desirable and possible ; but the experiments projected have been so far completed as to seem to justify publication, and I am not likely soon to be able materially to extend them.

General Principles kept in View.

The improvements made of late years in manipulative methods and apparatus have tended to reduce very much the magnitude of what are commonly called "fortuitous" errors in our quantitative determinations of matter, and to increase greatly the accuracy of such determinations. Probably no modern work has had more influence in this direction than the classic researches of STAS on certain atomic weights—the precautions taken by him, and his remarkable manipulative skill, causing his results to bear almost the same relation to those of his immediate predecessors as did those of BERZELIUS to the work of the chemists of his earlier day. No one nowadays would undertake the determination of an atomic weight of one of the better known elements without taking such elaborate precautions as practically ensure pretty close concordance of results, when obtained by the same method, applied in the same hands. In the present state of the question of atomic weights and improvements in their determination, advances in mere delicacy of manipulation and success in merely securing close agreement of results by the same method are not alone sufficient. It cannot be too much insisted upon that we need, besides, well-directed and laborious investigation of possible sources of *constant errors*, and the adoption of means to guard against them. Careful preliminary study is required, in a general way, of the precise nature of each reaction employed, and how it may be influenced by the conditions of the experiment. We learn more and more of late that many of the reactions—perhaps it should rather be said all of the reactions—which have been generally supposed to be of the simplest nature are in reality complex.

The following are among the general principles which seem to be most important, as tending to greater accuracy and trustworthiness in atomic weight determinations; they have been in part stated in the author's earlier paper on the atomic weight of aluminum:—

1. In purifying the materials used, both the element of which the atomic weight is to be investigated (or any special compound containing it) and all substances used to react thereupon, resort should in all cases be had to "fractional" methods, assuming materials to be pure only when earlier and later fractions give no signs of any constant difference in the results which they yield.

2. Different and independent processes should be applied to the determination of the same atomic weight, and the results used to check each other. It is desirable that as many such different processes be applied as can be devised, provided each be reasonably free from apparent sources of error, even though it be usually impossible to properly assume that all are equally advantageous in this respect, and therefore of equal value. In the comparison of results obtained it should be noticed whether a given method tends on the whole to yield results probably higher or lower than the truth, though it may be gravely doubted whether the practice is commendable of

attempting any numerical estimate of relative value, by so-called "weighting" of the results in calculation.

3. In connection with each process there should be careful study of the reactions depended upon for the final determination of an atomic weight, looking especially to the possibility of the occurrence of secondary or subsidiary reactions.

4. Each process adopted should be as simple as possible, both in the nature of the chemical reaction or reactions and in the known liability to merely manipulative errors.

5. Each process should be carried out with, in some experiments larger, and in others smaller, quantities of material. But, on the whole, the quantities used should be kept within such limits as are most likely to admit of most accurate determinations being had under the conditions of the special process.

6. In the reactions depended upon only such other elements should be concerned as may be counted among those of which the atomic weights are already known with the nearest approach to exactness.

7. It is particularly desirable that, if possible, the atomic weight to be investigated shall be, by at least one process, compared directly with that of hydrogen, now almost universally taken as the basis for the whole list of the elements. It is remarkable for how very few of the elementary substances—not more than three or four—this direct comparison has been accurately made.

8. In the greater number of the processes available for atomic weight determinations the comparison with hydrogen must perforce be made indirectly. When this is the case, it is desirable that as few other elements as possible, the assumed atomic weights of which will have to be taken into account, shall be involved in each single reaction depended upon.

9. In selecting *different* processes to be applied to the determination of the atomic weight of a given element, in order that the results may check each other, it is desirable that, not the same, but as many different other elements as possible, shall be concerned in the several reactions, provided all such elements count amongst those of which the atomic weights may be considered in the first rank as to the accuracy with which they are known.

Means and Methods of Weighing Employed.

These were in the main the same as those which I had in former years used in determining the atomic weight of aluminum.

The balance chiefly used, made by BECKER, was carefully cleaned, and all its parts adjusted, especially as to the position of the centre of gravity for each load to be used. A second balance by the same maker, of larger size, capable of taking a load of a kilogramme in each pan, was employed in weighing certain of the solutions experimented on, and was in like manner carefully adjusted and tested. All weighings were made by observation of the oscillations of the index on either side of the position of rest. A difference of ·0001 grm. with the smaller balance, and ·0002 grm. with the larger instrument, was easily and distinctly observable with any load which the research required.

The same kilogramme weight was made the basis of a comparison with all my other weights which had been before used in the same way. This had been compared at Washington with the "star kilogramme" of the United States Coast Survey, the value of which is known in terms of the original "kilogramme of the Archives" at Paris. All the smaller weights were carefully rechecked against this and against each other, and their real values ascertained as referred to a vacuum. The necessary determinations were made of the specific gravity of all materials and vessels which were to be weighed, and the barometer and thermometer were read at the time of each weighing, so that all weights recorded in this paper represent real values *in vacuo*. In order to reduce to a minimum errors due to varying deposition of hygroscopic moisture, vessels of like material, shape, and size with those used to contain substances to be weighed were used as tare.

History and Mode of Purification of the Gold used in this Research.

Most of the metal needed was prepared by myself, with precautions presently to be mentioned; a part was obtained, as "proof gold," from the United States Mint at Philadelphia; another part from the United States Assay Office at New York; and a single specimen of English "trial plate" gold from the Royal Mint in London.

1. *Purification of Gold by the Author.*—It may fairly be concluded from the general history of the gold of commerce that the impurities most to be suspected, and most requiring special precautions for their removal, are silver and the metals of the platinum group. My preliminary experiments led me to believe that the greatest difficulty in the way of obtaining perfectly pure gold consists in getting rid of the last traces of silver, the chloride of this metal not being quite insoluble in a solution of auric chloride. For the removal of silver, I have chiefly depended upon evaporation of the gold solution with a little hydrobromic acid, followed by large dilution with water, and long continued clearing by subsidence. As regards the platinum metals, my results agreed substantially with those of HOFFMANN and KRÜSS,* but I have been inclined to lay some stress on reduction of the gold from its solution *with exclusion of light*, and on *fractional* reduction, using only the middle portion thrown down. I avoided altogether the use of ferrous salts as reducing agents, in view of the difficulty of preparing them in large quantity with assurance of their purity, and the trouble of thoroughly washing the precipitated gold. For the final precipitation of the gold formic acid seemed to offer real advantages; its volatility admits of easily getting it

* 'LIEBIG'S Annalen,' vol. 238, p. 66.

free from any metallic contamination, and the reduction is more easily effected than with oxalic acid.

Starting with United States gold coin, it was first heated to bright redness in a muffle, as a precaution against the presence of any traces of mercury, and to remove any grease, &c., from the surface, and then dissolved in a mixture of pure hydrochloric and nitric acids in the right proportions. The solution was evaporated with excess of hydrochloric acid nearly to dryness, the auric chloride redissolved in a considerable quantity of water, and the solution allowed to settle for four or five days. The greater part of the clear liquid, drawn off with a syphon, and filtered through very fine siliceous sand,* was again evaporated nearly to dryness, adding towards the end a few drops of pure sulphuric acid, in case of the conceivable, though unlikely, presence of such traces of lead as this might reveal; much pure water was added, the solution again cleared by subsidence for several days, and the greater part of the clear liquid again drawn off and filtered. This solution was now rendered pretty strongly acid with hydrochloric acid, and fractionally precipitated by sulphurous acid (SO_2 was evolved from sodium sulphite), at as low a temperature as possible, and *in the dark*, putting aside the first and last portions of the metal thrown down, and reserving for further treatment the (largest) middle portion. The gold thus obtained was well washed with water, boiled with nitric acid alone, again washed, boiled with hydrochloric acid alone, again washed, dried, and heated strongly with fused acid sulphate of potassium in a porcelain crucible, boiled with dilute hydrochloric acid, and then with water. The metal was redissolved in aqua regia, the solution evaporated nearly to dryness, with addition of pure hydrobromic acid towards the end, very largely diluted with water, and allowed to stand for two days, well protected from dust, before again syphoning off as much of the clear portion as could be safely removed without risk of disturbing the remainder at the bottom, using a conical precipitating jar with greatest diameter below, and filtering the liquid through siliceous sand as before. The evaporation with hydrobromic acid was repeated twice more, and the clear solution—allowed the last time to stand a month before being syphoned off and filtered—was then reduced, once with oxalic acid (neutralizing the liquid with pure sodium hydroxide from the metal), once (after re-solution) with sulphurous acid, and once with formic acid, washing the reduced metal well each time before redissolving in aqua regia. In the first and second of these reductions a little of the metal first and last thrown down was rejected, and in the final reduction with formic acid the first portion precipitated, about one-fifth of the whole, was reserved for use, labelled A, *a*, the middle portion, about three-fifths, was labelled A, *b*, and the last portion, the remaining one-fifth, was also preserved for use, marked A, *c*, so that it might be seen whether any difference in the character of the metal could be detected in the

* The sand was carefully purified beforehand by boiling with nitric and hydrochloric acid, thorough washing with water, and heating to redness in the air.

atomic weight determinations. All of these fractions received a very thorough final washing with water.

Such part of the purified metal as was to be used in the preparation of gold compounds was not fused, but was heated in a glazed porcelain tube to moderate redness in a Sprengel vacuum. A small part of the metal used in the free state, and desired in compact form, was fused in a perfectly clean Beaufaye crucible with a little acid sulphate of potassium and borax, the button flattened, boiled with strong nitric and then strong hydrochloric acid, thoroughly washed with water, and, finally, heated in the Sprengel vacuum. Throughout the long process of purification, and especially towards its close, the most scrupulous care was taken to exclude dust, and to prevent grains of sand from the bottoms of beakers or any other impurities getting into the precipitated gold, upon which the acids used would not act, so as to obviate the risk of merely mechanical contamination, which, if overlooked, might lead to that being weighed as part of the gold which was, in fact, foreign to it.

2. *Purification of "Proof Gold" obtained from the United States Mint at Philadelphia.*—I owed to the kindness of Mr. J. B. ECKFELDT, Chief Assayer to the Philadelphia Mint, a liberal supply of the "proof gold" used in checking the gold assays there made, and he furnished me the following statement of the manner in which this purest metal is prepared, under his directions:—"The best cornets from the gold assays selected and dissolved in aqua regia. Solution evaporated, with additions of HCl, to nearly crystallization, diluted largely with water, and allowed to stand for three or four weeks. About seven-eighths of the solution drawn off from the silver chloride, and passed through several thicknesses of various filters. Solution somewhat concentrated, and alcohol and potassium chloride added, allowed to stand for some time (precip. traces of platinum*), and carefully filtered. Gold precipitated by addition of pure ferrous sulphate. Reduced gold washed repeatedly in boiling HCl, until washings show no iron, then well washed in pure water. Gold dissolved, and solution evaporated to crystallization, with repeated additions of hydrobromic acid,† diluted, and again allowed to stand for some time; filtered. Through the solution was passed pure SO_2 until all the gold was reduced; washed. Gold again dissolved, evaporated with HCl, diluted, and oxalic acid added, and heated, until all gold is down. Melted in white clay crucible with potassium chlorate and nitrate, afterwards with pure sodium carbonate and borax." Mr. ECKFELDT also informed me verbally that the proof gold thus purified is cast into a small bar in a perfectly clean and bright cast-iron mould; the bar is boiled in nitric acid, washed and dried, rolled between fine

* The platinum of South American native gold, and of scrap gold from dentists, is at the Philadelphia Mint separated solely by alloying with enough silver, and dissolving out the latter metal with nitric acid. The platinum dissolves with the silver.

† I had, many months before, independently adopted and used hydrobromic acid to remove traces of silver more effectually than by hydrochloric acid, when I learned from Mr. ECKFELDT that he had thus habitually employed it.

steel rolls quite free from grease, and the strip finally cleaned for use with hot hydrochloric and then nitric acid.

In a letter of later date he wrote, "In preparing the 'proof' I seldom make over 10 oz. in one lot; from 8 to 10 oz. is the usual amount. There is comparatively little trouble in making 999·9 fine, but beyond that it is rather troublesome; and it seems that, with all the care, the final result is sometimes a little in doubt."

The fine gold received from the Philadelphia Mint is designated as B in this paper, in connection with the experiments in which it was used.

3. *Purification of "Proof Gold" obtained from the United States Assay Office at New York.*—Dr. H. G. Torrey, Chief Assayer in this office, was obliging enough to let me have several samples of his finest proof gold, used in checking the regular assays in his department. He informed me that this proof gold was independently prepared at New York, but was occasionally compared with that of the Philadelphia Mint. He furnished the following brief statement as to its preparation—"The process used in preparing the gold is to dissolve 'cornets' (or gold from assays) in nitro-hydrochloric acid, and after filtration precipitating by oxalic acid, and after thorough washing melting under borax. The operation is conducted with the utmost care throughout."

The gold from this source is designated as C in this paper.

4. *Gold from the "Trial Plate" of Fine Gold of the English Mint.*—Professor Roberts-Austen, Chemist to the Royal Mint, was so kind as to let me have a specimen of a few grammes of gold cut from the trial plate of the pure metal prepared by him in 1873. In its preparation use was made of potassium chloride and alcohol to separate any platinum present in the original material, a long period of subsidence was allowed for the deposit of any silver chloride from the solution, and the whole process was applied on a large scale, resulting in the purification of some 70 ounces of fine gold, of which Professor Roberts-Austen himself has said: "I have not been able to prepare, or to obtain from any source, gold of greater purity, even in small quantities." It seems, however, that the apparent standard of this gold was slightly reduced in rolling, the finished plate being counted as 999·95 fine in comparison with the same gold before rolling. A memorandum given me by Professor Roberts-Austen states that this trial plate gold is 999·98 fine as compared with the purest gold obtained by Stas for the Belgian Mint.

This specimen of English trial plate gold is designated as D in the present paper.

All the samples of gold received from others—B, C, and D—were, before using them, carefully boiled in nitric acid to remove any possible traces of silver or other metal derived from the shears used in cutting the plates. They were also previously well washed with ether, to remove any grease, and afterwards with pure water, and were finally heated to redness in the Sprengel vacuum.

It may be remarked, in advance, that I have not been able to trace any probable connection between the history of the several samples of gold used and the values obtained for the atomic weight of the metal. Within the limits of accuracy attained, the results appear to have been sensibly the same by each method for all the gold used. Nor is there apparent in the results of KRÜSS, or those of THORPE and LAURIE, any evidence of a difference fairly traceable to the nature of the metal employed by them.

A considerable part of the gold prepared by myself was, after having once served for a determination of the atomic weight, redissolved and reprecipitated, and was afterwards more than once used in subsequent determinations, and yet no sign was obtained of any resulting influence upon the later values of the atomic weight as obtained, evidence being thus furnished of the purity, not only of the gold itself, but of the reagents used to act upon it, so far as any contamination of the metal was concerned. It may, therefore, be concluded with reason that the gold used in these experiments was of uniform character, and uniformly free from any known impurities, to such an extent, at any rate, as to sensibly change the results obtained.

It is to be noted that the only known elements having higher atomic weights than that of gold are mercury, thallium, lead, bismuth, thorium, and uranium. The presence of any of these in the gold experimented on, even in traces too minute to weigh, is in a very high degree unlikely. The presence of any other element or elements than these would, for analogous compounds, tend to lower the value obtained for the atomic weight of gold; so that, in considering the chances of error due to the nature of the metal used as gold, we should be inclined to say that the risk was rather in the direction of too low than too high a result being reached. But, if the possibility be observed of compounds not analogous being erroneously compared, the contrary error will be seen to be possible. Thus, in case the composition of an auric haloid salt obtained from a given amount of metallic gold should be examined, if any unsuspected silver were present there would be required for the same amount of the halogen three atoms of silver instead of one atom of gold, and, therefore, the apparent weight of gold as compared with that of the halogen would be increased instead of diminished, and a higher value obtained for the atomic weight sought.

General Precautions Observed in the Experiments for Determination of the Atomic Weight.

All the reagents used were prepared or purified by myself, and most carefully tested for any traces of such impurities as might reasonably be suspected, and as could affect their application to the purpose in view. Particular care was bestowed upon the examination of the distilled water, acids, and other materials used in large quantity. To remove organic matter from the water required, it was distilled from a small amount of potassium permanganate and sulphuric acid.

Scrupulous care to exclude atmospheric dust was observed. In the evaporation of some of the gold solutions the process was carried out in a glass bottle of considerably larger capacity than the volume of liquid to be treated, furnished with a well ground-glass stopper of special construction, as shown in figs. 1 and 2, the latter representing the stopper in place. Air, purified by passing through a red-hot tube, then through a solution of potassium permanganate and sulphuric acid, and dried by passing through concentrated sulphuric acid and over solid potash, was introduced by the tube *a*, which went down to near the level of the liquid to be evaporated, while this air, charged with vapour of water from the liquid, was withdrawn through the tube *b* by means of a water-jet pump; the bottle was moderately heated by immersion to the greater part of its height in a water bath.

Fig. 1.

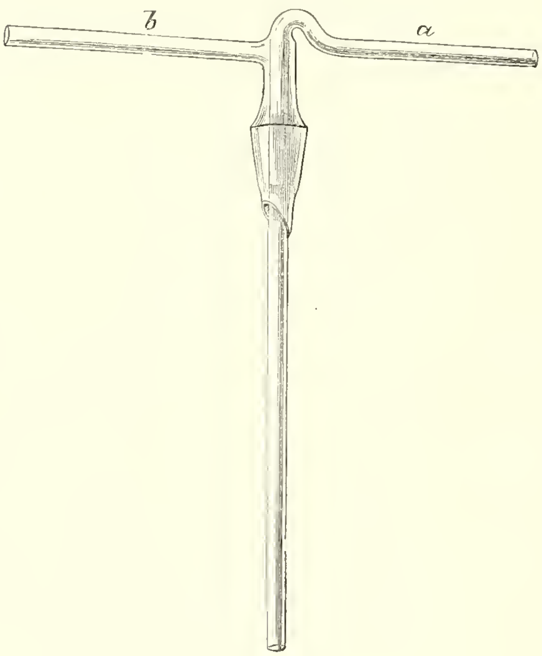
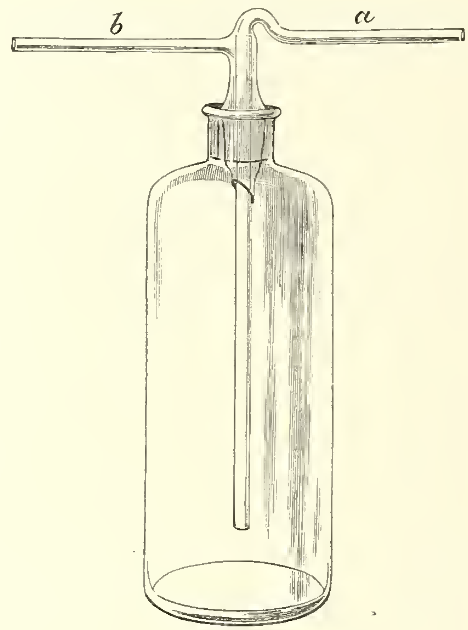


Fig. 2.



In filtering the gold solutions no paper or other organic material was used, but fine white siliceous sand, previously boiled in nitric and hydrochloric acids, washed with water, and well ignited to burn off any organic matter, was substituted, supporting it on coarser sand and larger fragments of quartz, similarly purified, and the whole arranged so as to prevent the possibility of any sand grains being mechanically carried into the filtered liquid. Vessels of hard glass and Berlin porcelain were employed. Care was taken to work in a clean laboratory atmosphere, free from gases or vapours which might affect the materials dealt with.

First Series of Experiments.

A neutral solution of auric chloride was prepared by cautiously heating auric chloride, made, as suggested by JULIUS THOMSEN, by the direct action of pure chlorine upon finely divided metallic gold, until such an amount of chlorine had been given off that on treating the residual material with moderately warm water, metallic gold only remained undissolved, which was then filtered off. This neutral solution having been rendered uniform by agitation, two approximately equal portions of it were weighed off, using, of course, stoppered vessels to prevent evaporation during the weighing. From one of these portions the gold was thrown down in the metallic state by pure sulphurous acid with the aid of heat, carefully collected, well washed, dried, ignited in a Sprengel vacuum, and weighed. To the other portion there was added the carefully prepared solution in a minimum of nitric acid of an accurately weighed quantity of pure silver, a little more than equivalent to the chlorine present, the liquid and precipitate digested together for a considerable time with gentle warming in a stoppered glass flask, well agitated from time to time, and the precipitate (of silver chloride, containing also the gold) filtered off upon siliceous sand, and thoroughly washed, avoiding throughout the decomposing influence of light. The clear filtrate was nearly neutralized with pure sodium hydroxide (from metallic sodium), evaporated down to a small bulk, using the vessel represented in fig. 2 (p. 410), and finally the remaining silver was determined (with all the needful precautions of the silver assay) by means of a weighed quantity of a weak solution of pure hydrobromic acid standardized against pure silver. This mode of determining chlorine by means of silver and hydrobromic acid was suggested to me in a letter, of the 27th January, 1887, with which I was favoured by M. STAS,* who advocates it as the most exact process available. The pure silver required was prepared in the same way as that used in my experiments on the atomic weight of aluminum,† and was heated in the Sprengel vacuum to remove all occluded gas. The hydrobromic acid was prepared as directed by STAS in his published paper—"De la détermination du rapport proportionnel entre l'argent, les chlorures et les bromures."‡

In reporting the results obtained, the quantity of gold stated is that actually weighed, but the quantity of silver corresponding thereto has, for the sake of simplicity, been given as that required for an exactly equal quantity of the auric chloride solution, while, as stated above, the quantity of liquid weighed off was very nearly, but not exactly, equal to that from which the gold was thrown down, the difference being allowed for in calculation.

With this explanation the results of the first series of experiments were as follows :—

* In this letter M. STAS says, "Je me permets de vous recommander l'emploi de l'acide bromhydrique pour la précipitation de l'argent resté dans un liquide après une double décomposition opérée à l'aide d'un *chlorure* et d'un sel d'argent. On réussit à condition que l'eau mère renferme un excès d'argent dont le poids est le triple du métal qui peut rester en solution à l'état de chlorure d'argent."

† 'Phil. Trans.,' 1880, p. 1020.

‡ 'Mémoires de l'Acad. Royale des Sciences de Belgique,' vol. 43, 1882.

Experiment.	Character of gold used.	Gold.	Silver required to precipitate Cl.
		grm.	grm.
I.	A, *a*	7·6075	12·4875
II.	A, *b*	8·4212	13·8280
III.	B	6·9407	11·3973
IV.	A, *c*	3·3682	5·5286
V.	C	2·8244	4·6371

In regard to conceivable sources of error connected with this method, it is to be observed that, in preparing the original auric chloride solution, if there should be any reaction between this gold salt and the water, leading to the formation of traces of hydrogen auri-chloride and precipitation of a little auric oxide or hydroxide, which might escape observation in admixture with the metallic gold left undissolved, the tendency would be to lower the atomic weight found for gold. If, by reaction between this residual metallic gold and the auric chloride solution, any traces of aurous chloride were produced and taken up by the solution of the higher chloride, the effect would be to raise the apparent value of the atomic weight.*

If, in the reaction of the silver solution upon that of auric chloride, *partial* withdrawal of chlorine should lead to the formation of any traces of aurous chloride, precipitated along with the chloride of silver, and not afterwards decomposed during the digestion of the precipitate with the remaining solution, the resulting error would also be in the direction of too high an atomic weight. The probability of the last supposition is diminished by an excess of silver for the whole amount of chlorine present having been added at once. It is not very likely that any one of these defects actually belongs to the method and affects its results to a sensible extent. Of the three I should be more inclined to suspect the possibility of the second than either of the two others.

Second Series of Experiments.

A neutral solution of auric bromide was prepared by a like process to that used in making the auric chloride of the first series: acting upon pure metallic gold with pure bromine (prepared with the precautions recommended by STAS), evaporating the solution to dryness out of reach of dust, cautious heating of the residue, re-solution of auric bromide, and filtration from undissolved metallic gold.

Two nearly equal portions of the solution were accurately weighed off, and treated as described above: in one reducing the gold to the metallic state and determining its weight; treating the other with a small excess of silver in solution as nitrate, filtering off the precipitate, concentrating the filtrate with the precautions already described, and determining in it the excess of silver by means of hydrobromic acid.

* These two remarks apply, of course, also to KRÜSS's first series of experiments.

Reducing the amounts of silver actually used to the corresponding quantities for portions of auric bromide solution exactly equal to those from which in each case the gold was obtained, the results in six experiments stood as follows :—

Experiment.	Character of gold used.	Gold.	Silver required to precipitate Br.
		grm.	grm.
I.	A, *b*	8·2345	13·5149
II.	A, *c*	7·6901	12·6251
III.	B	10·5233	17·2666
IV.	A, *a*	2·7498	4·5141
V.	C	3·5620	5·8471
VI.	A, *b*	3·9081	6·4129

In these experiments the sources of constant errors which suggest themselves as possible are essentially similar to those for the first series; but, if any such really exist, there is, of course, the likelihood of some difference being introduced by the substitution of bromine for chlorine. Hence the desirability of multiplying experiments in this modified form.

Third Series of Experiments.

For these experiments potassium auri-bromide was prepared with great care from an excess of metallic gold treated with bromine and potassium bromide, purified in accordance with STAS's suggestions, and the double salt five times recrystallized. The last crystallization was conducted fractionally, in closed vessels, with special care to exclude dust, by gradual but pretty rapid cooling with agitation, and the earlier and later portions separated out were kept apart in after use.

For each atomic weight determination an unweighed quantity of this potassium auri-bromide was dissolved in water, the solution rendered uniform by agitation, and divided into two nearly equal parts, which were severally weighed with accuracy, and in one the gold reduced to metal as in the experiments of the first and second series, and in the other the total bromine precipitated by silver solution as before, the comparison being made once more between the weight of the gold and that of the silver equivalent to the bromine (in this case representing 4 atoms) existing in the double bromide.

Again stating the quantities of silver corresponding to portions of the auri-bromide solution exactly equal to those used in determining the gold, the following were the results obtained :—

Experiment.	Character of gold used.	Fraction of crystallized auri-bromide used.	Gold.	Silver required to precipitate Br.
			grm.	grm.
I.	A, *b*	First	5·7048	12·4851
II.	A, *b*	Second	7·9612	17·4193
III.	B	First	2·4455	5·3513
IV.	B	Second	4·1632	9·1153

Of the tendencies to constant error which may be imagined in connection with the experiments of the first two series, and which have been noticed above, the first may probably be considered as not applying to the method pursued in this third series, while the second and third might still be applicable. But the superior stability of the double salt constitutes an advantage in its favour, and, as it formed the chief material for the experiments of Krüss and of Thorpe and Laurie, a comparison with their results is desirable, the mode of treatment pursued by me in ascertaining the composition of the salt not having been quite the same as that used by these chemists.

Fourth Series of Experiments.

A weighed quantity of trimethyl-ammonium auri-chloride [$N(CH_3)_3HAuCl_4$] was decomposed by heating in the air, and the weight of the residual metallic gold determined. This trimethylamine salt was selected because the base is of simple and well established constitution, and may with reasonable probability be counted upon as obtainable in a state closely approaching purity, and because the gold salt is easily crystallized, possesses a considerable degree of stability, and contains approximately half its weight of gold, so as to offer the most favourable chance of determining with accuracy the ratio between the metal left behind and the sum of the remaining constituents driven off on ignition. Although its use in fixing the atomic weight of gold involves the atomic weights of three other elements—carbon, nitrogen, and chlorine—all three of these constants deserve to be ranked amongst those already known with the nearest approach to precision at present attainable.

In order to obtain pure trimethyl-ammonium chloride, the impure commercial salt, derived from the *vinasse* of beet-root sugar making, was used, first setting free and distilling off a considerable quantity of trimethylamine and condensing at about the right temperature, and subsequently purifying the product by Hofmann's method of treatment with ethyl oxalate and renewed distillation. The purified trimethylamine was several times fractionally distilled, and the portion of correct and most constant boiling-point finally neutralised with pure hydrochloric acid. The concentrated solution of trimethyl-ammonium chloride was now precipitated by a strong solution of auric chloride, the mother liquor decanted off, and the gold salt redissolved in hot water, and recrystallized several times. The bright yellow crystalline powder was dried, first over sulphuric acid and afterwards over phosphorus pentoxide, until it ceased to lose weight; towards the end of the drying the temperature of the vessel was raised to about 50° C. Preliminary experiments seemed to indicate the probable existence of this salt crystallized with a single molecule of water, but most of that prepared contained no constituent water, and it appeared easy to attain complete drying without any decomposition of the salt itself. Throughout its treatment the salt, which was not in any high degree hygroscopic, was well guarded from dust and from any possible decomposing effect of light.

The portion of the salt to be used in each experiment was contained in a small glass-stoppered weighing flask, which was removed just before it was needed from the phosphorus pentoxide desiccator, the stopper having been inserted; the flask was weighed, the greater part of its contents transferred quickly to a weighed porcelain crucible, the stopper at once replaced, and, the flask being again weighed, the quantity of gold salt taken from it was found by difference.

In order to avoid mechanical loss by spattering on igniting the crucible and its contents, the auri-chloride lying together at the bottom of the crucible was covered by a layer, nearly a centimetre deep, of clean, carefully purified, and just previously well-ignited siliceous sand, the weight of this sand being known by taking it from a weighing flask in which it had been cooled over phosphorus pentoxide, and noting the loss of weight of this flask. In applying heat to the crucible and its contents it was found necessary to heat gently for a long time, raising the temperature slowly, in order to prevent extensive charring at the bottom. Then, before the temperature had become too high, but after a considerable part of the volatile matter had been driven off, the sand was carefully stirred in with the remaining material so as to produce pretty uniform mixture, in order that the gold might not undergo partial welding together at a higher temperature, which might have led to wrapping up particles of carbon and their protection from combustion. In this operation a very small porcelain stirrer was used, as a platinum wire would have welded on and taken up some of the metallic gold; the weight of this stirrer was determined in advance, and checked after use. Finally, the contents of the crucible were submitted to very careful and prolonged heating to moderate redness, with free access of air and occasional cautious stirring, so as to burn away every trace of carbon. After cooling in a desiccator, the crucible and its remaining contents were weighed, giving the weight of the residual gold by subtraction of the weights of the crucible itself and the siliceous sand. As an additional safeguard against any particles of carbon left unburned escaping detection, the gold was afterwards dissolved out with aqua regia, and the white sand carefully looked over with a lens.

The results of five experiments thus conducted were as follows :—

Experiment.	Character of gold used.	Character of gold salt used.	Salt ignited.	Residual gold.	Loss by ignition.
			grm.	grm.	grm.
I.	A, *b*	Earlier crop of crystals .	14·9072	− 7·3754	= 7·5318
II.	A, *b*	Middle „ „ „ .	15·5263	− 7·6831	= 7·8432
III.	A, *b*	Last „ „ „ .	10·4523	− 5·1712	= 5·2811
IV.	C	Middle „ „ „ .	6·5912	− 3·2603	= 3·3309
V.	C	Last „ „ „ .	5·5744	− 2·7579	= 2·8165

In these experiments the most probable source of error may be fairly taken as arising from the presence of traces of methyl-ammonium or dimethyl-ammonium aurichloride with the trimethyl-ammonium salt. I know of no direct evidence that any such impurity was present, and the absence of any such evidence in the results from the earlier as compared with the later crops of crystals rather tells against the supposition of its presence, but one cannot feel certain of its entire absence. If present, its effect would be to raise the atomic weight obtained for gold. It is also conceivable that there may have occurred volatilization of gold to a minute extent as auric chloride, in accordance with the observation of KRÜSS that this salt may be sublimed in small quantity at moderate temperatures in a stream of chlorine; but, there being no such stream of chlorine in these experiments, and on the contrary the decomposing action of the hydrogen of the trimethylamine salt, this does not seem likely; the effect would, of course, be to raise the atomic weight obtained for gold. Another possible cause of error might consist in imperfect drying of the gold salt used, but the constancy of weight attained on drying renders it unlikely that any other than extremely minute error should come of this, though not altogether excluding the possibility of its occurrence; its tendency would, of course, be to lower the atomic weight obtained. Any trouble from hygroscopic moisture on the surface of the porcelain crucible and sand was, I think, satisfactorily guarded against by the use of a corresponding tare crucible, and by more than one weighing after a near approach to the true figures had been obtained, the crucibles having meanwhile been restored to the desiccator and kept therein for some time. The precautions taken seemed to afford sufficient protection against any merely mechanical loss during the ignition.

Fifth Series of Experiments.

In these experiments an attempt was made to determine the ratio between the weights of metallic gold and metallic silver deposited by the passage of one and the same electric current successively through solutions of the two metals. The simplicity and accuracy with which the direct weighings may be made seemed to present decided advantage, but various difficulties were encountered, and, after the expenditure of a very large amount of time and labour upon the method, it cannot be said, on the whole, to have satisfied me with its results.

While taking due note of the recent literature on the subject of the quantitative electro-deposition of metals from their solutions, especially the reports of work by A. CLASSEN,* Lord RAYLEIGH and Mrs. SIDGWICK,† Dr. GORE,‡ THOS. GRAY,§ and W. N. SHAW,‖ the author of the present paper made for himself a somewhat extended preliminary examination of the effect of varying conditions on such depositions, so far at least as seemed to be required for his immediate purpose.

The general arrangement of apparatus adopted consisted of a horizontal strip, 4 mm. thick, of vulcanite, or hard vulcanized india-rubber, about 26 cm. long by 3 cm. wide, near each end of which and in the middle of the width were two small holes, through which passed short bits of brass rod, each having attached to it above a binding screw, and below a forceps-like clip, which could be opened by pressure on two little outside studs, but closed firmly, on release of this pressure, by the elasticity of the metal. In these clips were supported the plates of metal to be immersed in the electrolysed solutions, and to serve as anode and cathode terminals respectively, there being two pairs of such plates, one pair near each end of the vulcanite strip, with four corresponding binding screws. The electric current passed from the first binding screw through one of two metallic solutions—as, for instance, that containing gold—between the first pair of plates, consisting of the same metal as that in this solution, then from the second binding screw to the third (at the other end of the vulcanite strip) by a stout copper wire above, and then through the second of the two solutions—as, for instance, that of silver—between the second pair of plates, consisting again of the same metal as that in the solution in which they were immersed, thus reaching the fourth and last binding screw, the first and last binding screws being, of course, connected by wires with the terminals of the galvanic cells used to develop the current. Fig. 3 shows the disposition in question. The source of the electric current was for the most part galvanic cells of the Meidinger pattern, but in some of the experiments small Daniell cells, and also a Clamond thermo-electric battery, were used. The lower parts of the clips were heavily electroplated with the same metal as that in the solution to which they respectively belonged, in order to avoid any risk of contamination of the solution, in case there should be spattering or accidental immersion, even for a moment, of any part of the clip.

It was decided to place the plates vertically in the liquids, but to make the vertical height small in proportion to width, so as to preserve as far as possible a uniform condition of the solution in depth. The form adopted for the plates was that of fig. 4, the shaded part of the surface being coated with hard paraffine, with a view to

* A. CLASSEN, 'Quantitative Chemische Analyse durch Electrolyse,' 2te aufl., Berlin, 1886.

† 'Phil. Trans.,' 1884, p. 411.

‡ 'Nature,' March 16, 1882; Feb. 1 and Feb. 15, 1883.

§ 'Phil. Mag.,' Nov., 1886, p. 389; and March, 1888, p. 179.

‖ 'Phil. Mag.,' Feb., 1887, p. 138.

preventing the strip by which the anode plate was suspended from its clip being cut across by solvent action at the surface of the liquid. This coating of paraffine was put on after the plates were first weighed, and carefully removed before the second weighing. The four plates for each experiment were of equal size as to length and breadth; in most of the experiments the immersed surface (of one side) measured about 25 square centimetres, though in some cases plates of double this size were used. The thickness was the same for plates of the same metal, but those of the

Fig. 3.

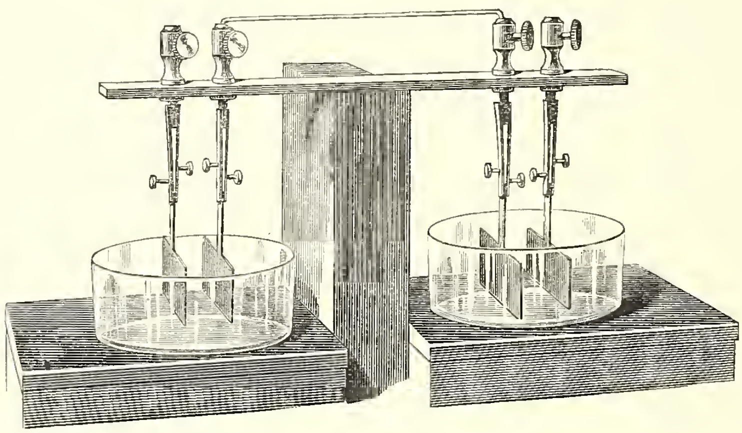
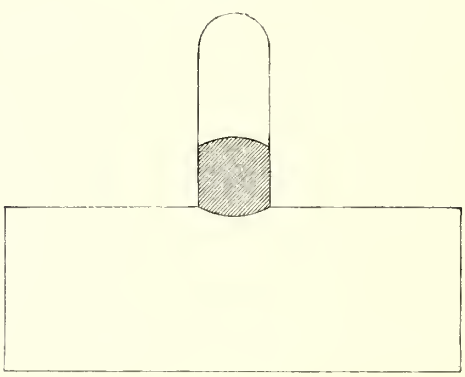


Fig. 4.



different metals to be compared were made to differ in thickness to such an extent as to allow for the different rate of solution to be expected of the anode plate. I was indebted to the kindness of Mr. ECKFELDT, of the Philadelphia Mint, for having plates of "proof" gold and silver specially rolled for me, with all necessary precautions as to perfect cleanliness of the rolls, &c., so as to obtain the determinate thicknesses desired.*

* Mr. ECKFELDT informed me that his method of preparing the proof silver used for these plates was as follows:—"Nitrate of silver from the gold assay parting is, after careful filtering, precipitated with

By heating in a Sprengel vacuum I found traces of oxygen in the rolled silver plates, and extremely minute traces of gas, apparently also oxygen, were likewise obtained from the gold plates, before either had been used.

The middle of the vulcanite strip was supported at a suitable height, so as to allow of equal immersion of the two pairs of plates in their respective solutions, which were contained in small vessels of good hard glass, free from lead. Care was taken to keep the vulcanite strip dry, so that there should be no practical defect of insulation between the two plates of each pair; the necessity for this precaution having been shown in some of the very early preliminary experiments with copper plates, using a wooden supporting strip; some puzzling results being traced back to a little accidental moistening with sulphate of copper solution of the part of the strip between one pair of plates, while those of the other pair were well insulated as to the strip from which they hung.

In all the experiments the two pairs of plates, previously ignited in the Sprengel vacuum, cooled, and weighed, were placed in position in the clips, the distance between the parallel surfaces of the plates of each pair being the same, and in most of the experiments measuring about 2·5 cm., and connection was made with the terminals of the galvanic cell or cells used before immersion of the plates in the metallic solutions. All four plates were immersed at the same moment, and at the end of the experiment were in like manner lifted out of the solutions at the same moment, before the current had been broken. They were immediately introduced into one after another of several portions of distilled water, before removal from the clips, thorough washing, heating in the Sprengel vacuum, and final weighing.

A preliminary course of experiments was carried out with plates of pure electrotype copper (both pairs) in solutions of cupric sulphate, in order to test the effects, if any, of the following differences in the conditions of the two electrolysis cells compared.

1. *Effect of Difference in the Degree of Concentration of the Two Solutions.*—The solution in one of the two vessels in which the plates were immersed being made to contain but one-tenth the proportion of cupric sulphate existing in the other, acidification and all other conditions being the same for both, only a very minute difference was found between the quantities of copper deposited in the same time on the two cathode plates, and the difference was not invariably in the same direction. The tendency however, seemed on the whole to be toward a slightly larger amount thrown down

hydrochloric acid, and the chloride of silver, after a thorough washing with pure water, is dried and reduced in the melting pot with pure carbonates of soda and potash and carbon in the shape of wheat flour, the melting being done in a clay crucible. The resulting silver bar is then dissolved in dilute nitric acid, and after standing some time filtered, precipitated, and reduced as before; then remelted with the addition of pure nitrate of potash and borax. This generally gives a bar somewhat brittle (crystalline in fracture). It is then remelted, and stirred with a pine stick, and chloride of ammonium added; when the chloride has disappeared the metal is poured. I find this method more satisfactory than any other I have tried."

from the stronger than from the weaker solution. In every case there was decidedly more copper dissolved from the anode than was deposited on the cathode plate.

2. *Effect of Difference in Acidity of Two Solutions, otherwise of the Same Strength.*—With the same proportion of cupric sulphate in both solutions, one was made to contain but one-tenth as much free sulphuric acid as the other; all other conditions remained the same for both. As before, the difference of result was insignificant, and somewhat variable in direction, with an apparent tendency towards a very slightly greater deposit on the cathode plate in the less acid as compared with the more acid solution. As before, there was in every case a distinctly greater loss of copper from the anode than gain on the cathode plate, especially in the more acid solution.

3. *Effect of Difference in Temperature of the Two Solutions.*—The proportion of cupric sulphate and of free acid being the same for both solutions, and all other conditions the same, one of the two, however, being maintained at 72°, 47°, or 37° C., while the other was at 2° C., thus establishing a difference in temperature of 70°, 45°, or 35° respectively, there was distinctly in every instance rather more copper thrown down on the cathode plate in the colder than in the warmer solution. The loss of weight of the anode plate was always greater than the gain at the cathode, and the difference in this respect was greater in the warmer than in the colder solution.

4. *Effect of Difference in the Size of the Plates.*—All other conditions being the same in both the electrolysis cells, the plates in one were made to present but one-fourth the surface of those in the other, so that the "current density" was proportionally increased in the former. Under these circumstances there was a constant, though but small, difference in the amount of copper deposited on the two cathodes, the quantity being greater on the cathode plate with smaller surface. The tendency seemed to be towards a greater excess of metal removed from the anode over that deposited on the cathode plate in the case of the larger plates, as compared with the smaller.

5. *Effect of Difference in the Distance between the Plates.*—The plates of both pairs being equal in size, and all other conditions being uniform, the plates in one of the two electrolysis cells were placed at a distance apart only one-fifth that intervening between those of the other pair. It was not clear that any constant difference of result could be detected, but the tendency seemed to be rather toward a very slightly greater deposit on the cathode plate in the case in which the plates were farther apart as compared with that in which they were nearer together. There was no recognisable difference in the proportion of metal dissolved off from the anode plate.

Similar experiments were made with two pairs of plates of pure silver, thus checking the results obtained with copper, and contrasting the behaviour of one at least of the less chemically alterable metals with that of the more easily alterable copper. It was intended to make a set of similar experiments also with gold plates only, but the available supply of pure gold in the form of rolled plates was not sufficient for the numerous experiments required. The silver solution used was one of potassium argento-cyanide, and the substitute for the free sulphuric acid of the copper experiments was an excess of potassium cyanide. The results obtained were essentially similar to those of the copper experiments, the effect of difference in temperature between the two solutions being, however, less decided, and the slight effect of difference in the size of the plates ("density of current") less constant and distinct.

In all the preceding experiments it was found that the most constant results under otherwise similar conditions were obtained by using feeble currents rather than those of greater strength, especially in the case of the silver solutions. There seemed, however, to be a limit to this. On the whole, the most satisfactory results were obtained (both in these preliminary experiments, and in those aiming at the atomic weight determination) with currents not exceeding $\frac{1}{100}$th of an ampère per square centimetre of surface of (one side of) the opposed plates, and in some cases a current but one-fifth of this maximum was used.

Having in view the indications afforded by the preliminary experiments, it was determined to use tolerably strong solutions of the metals to be deposited, with not more than a moderate excess of free acid, or, in the case of the double cyanide solutions, excess of potassium cyanide, to maintain the same temperature in both the electrolysis cells and to have this temperature as low as possible (about 2° C.), and to have the plates of the two metals to be compared equal in size, and at equal distances apart, using a weak electric current, and keeping watch over its strength by means of an ordinary hydrogen voltameter in the circuit.

In the actual experiments on the deposition of gold as compared with silver it was originally proposed to use a solution of potassium auri-cyanide against one of potassium argento-cyanide, with the expectation that 3 atoms of silver would be thrown down for 1 atom of gold. But the first attempts made showed clearly that this reaction could not be obtained. The comparison as to gain in weight of the gold and silver cathode plates gave results leading to an atomic weight for gold impossibly high if the silver deposited were taken to represent 3 atoms, and much too low if it were taken to represent but 1 atom. Hence it appeared that the potassium auri-cyanide had been partially, but not completely, reduced to auro-cyanide by the action of the current, and an intermediate result obtained as to the equivalent quantity of silver between that due to the one or the other gold salt if exclusively present.

A change was therefore made to the auro-cyanide in the preparation of the solution to be electrolysed. A pure form of potassium cyanide was prepared with the aid of alcohol, and carefully tested as to the absence of any metal capable of deposition from the watery solution on electrolysis. Auric chloride was precipitated by ammonia, the fulminating gold, after washing, dissolved in a strong solution of this potassium cyanide with the aid of heat, and the auro-cyanide crystallized out by cooling. The crystals were washed, redissolved in water, aurous cyanide separated from the solution by evaporation with hydrochloric acid, and the crystalline powder after cautious washing again dissolved in potassium cyanide solution, using for the purpose the barely necessary amount of the solvent liquid, but afterwards adding a further quantity, so as to have potassium cyanide in excess. Potassium argento-cyanide was prepared by precipitation of a solution of pure silver in nitric acid with the purified potassium cyanide, washing the precipitate, and re-solution with the aid of the necessary quantity of potassium cyanide, of which finally a moderate excess was added. The solutions of the gold and silver salts were made of equivalent strength, for the most part at the rate of 7 grm. of metallic gold for each 100 c.cm. of solution, and an approximately corresponding amount of silver, taken atom for atom. Both solutions received the same excess of potassium cyanide, generally equal to one-half of that already present in the double salt, but in some of the experiments it was found necessary to add yet more during the electrolysis in order to preserve the purely metallic character of the surface of the plates. As an additional security against admixture of auri-cyanide with the auro-cyanide of the gold solution, it was subjected for some time to electrolysis with unweighed gold plates immersed, these being reversed two or three times in position, just before the introduction of the weighed plates for a quantitative experiment. A number of attempts were made to substitute for the solution of potassium auro-cyanide one of sodium auro-thio-sulphate, of potassium or sodium auri-chloride, and of simple auric chloride, in the last two cases employing at the same time a solution of silver nitrate, but these efforts led to no success.

In many of the experiments made with the double cyanide solutions the cathode plates, both of gold and silver, after removal from the electrolysis cells and thorough washing, were found to curl up on being heated, the deposit, which in these cases was rather hard and brittle, swelling up in a remarkable way, with formation and bursting of little blebs or minute bubbles of the metallic surface, and parting off to some extent of the deposit from the original plate underneath. When the heating was carried out in the Sprengel vacuum small but quite appreciable amounts of hydrogen were found to be given off, having been occluded in the metal deposited. It seemed necessary to throw aside the results in all cases in which this condition of the deposit was well marked. Other experiments were vitiated by the gold deposit not being thoroughly compact, and still others by the surface not being clearly metallic, aurous cyanide making its appearance from the solution. It was hoped that in the experiments, free from apparent defect, any irregular behaviour of the gold solution at first might be got rid of by continued electrolysis, with reversal of the anode and cathode plates when necessary, until the ratio of gold to silver deposited should become constant; but confidence in this was greatly shaken when an instance occurred, followed afterwards by others, of sudden change in this ratio, attended with much less loss from the anode gold plate than the gain of the opposed cathode plate, pointing to deposition of gold from the auro-cyanide with simultaneous formation of auri-cyanide in the solution.*

* HITTORF ('POGGENDORFF, Annalen,' [4], vol. 16, p. 523), in the simultaneous electrolysis of gold and

Altogether but five experiments made in this way yielded results which seemed worthy of being used to determine the atomic weight of gold, and it is of course unsatisfactory to know that these were selected out of a much larger number, mainly because, while not known to be in any way vitiated by apparent defects, they lead to values for the atomic weight in question close to those obtained by other methods and other experimenters. It is possible that this near approach to agreement may merely result from a balance of errors in opposite directions, which taken separately would have caused the experiments to be rejected. Some other experiments, under apparently similar conditions, gave figures for the atomic weight differing from those reported by one or two whole units.

These only admissible results are the following :—

Experiment.	Character of gold in solution.	Character of gold in plates.	Gold deposited.	Silver deposited.
			grm.	grm.
I.	A, *b*.	B	5·2721	2·8849
II.	,,	,,	6·3088	3·4487
III.	,,	,,	4·2770	2·3393
IV.	,,	,,	3·5123	1·9223
V.	,,	,,	3·6804	2·0132

Aside from other difficulties liable to be encountered in carrying out this electrolytic method, the two most important sources of possible inherent error which suggest themselves are the occlusion of hydrogen by the metallic deposit and the instability of the atomicity of gold in the solution electrolysed.

The separation of hydrogen on the cathode plate, whether in bubbles (which may be avoided by proper regulation of the current) or occluded by the metal (which does not seem to be completely avoidable with any current, although the amount of occluded gas was extremely small in a number of my experiments), must be ascribed to decomposition, simultaneous with that of the cyanide of gold, either of water or, more probably, of cyanide of potassium, with secondary action of the potassium on the water. In either case, it is by no means clear that the proportion of current giving rise to this liberation of hydrogen can be counted upon as the same in the gold solution and in that of silver; and hence, even though it be fairly assumed that FARADAY'S principle of equivalent electrolysis by the same current is strictly correct for the *ensemble* of chemical actions in the two cells, the portion of current actually concerned in depositing gold or silver only in each of the respective cells may conceivably not be

silver solutions, the gold as potassium auri-chloride, obtained results which showed that this metal was deposited at the rate of 1 atom for 3 of silver. Calculating on this basis from his two experiments, the atomic weight of gold comes out = 196·311 and 194·197; for silver = 107·66.

In one experiment of my own, using sodium auri-chloride, the result showed that the gold was thrown down for the most part as a triad, but partly as a monad, element.

quite the same, so that the weights of the two metals thrown down may not be strictly equivalent.* It was, therefore, deemed important to work with feeble currents, and, while heating all the plates in a Sprengel vacuum before weighing, to reject the results of all those experiments in which the quantity of gas thus discharged amounted to more than the merest trace. But, if the source of error in question still exist at all, it might affect the atomic weight of gold in comparison with that of silver, either by making the former appear higher or lower than the truth.

The source of error most to be feared, however, in connection with the application of this electrolytic method to the determination of the atomic weight of gold, is the uncertainty of having all the gold throughout the process in the form of potassium auro-cyanide in the solution, in view of the transition observed to auri-cyanide during electrolysis, although change in the opposite direction occurs with even greater ease. Each of the two salts appears to admit of electrolytic decomposition, and the presence of any traces of the auri-cyanide, in which the gold has triad character, while the calculation is based on the supposed presence of monad gold only would, of course, tend to make the atomic weight of the metal appear lower than the truth.

Sixth Series of Experiments.

These experiments consisted merely in the further application of electrolysis to the deposition of metallic gold from a solution of potassium auro-cyanide, comparing the weight of the metal thrown down, however, not with the weight of silver, but with the volume of hydrogen gas liberated by the action of the same current, the object being to thus secure, with an assumed knowledge of the density of hydrogen, a direct comparison of the atomic weight of gold with that of the element most generally taken as the basis of the numerical constants in question.

A cell containing the same solution of potassium auro-cyanide as was used in the

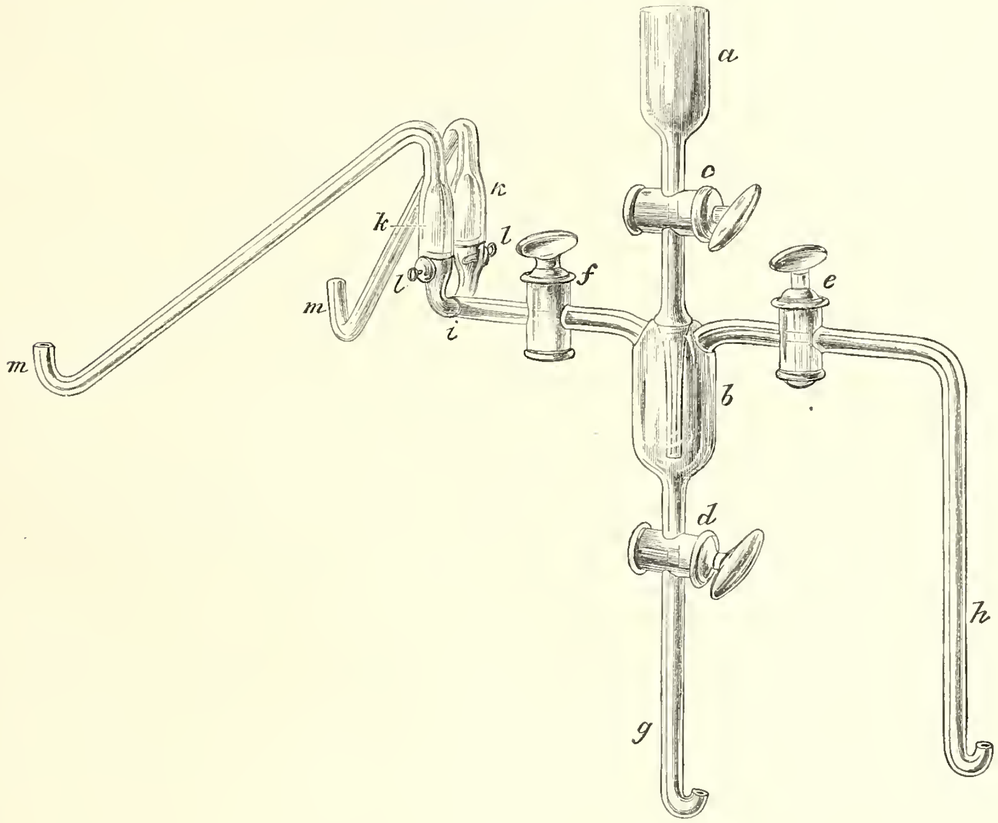
* As bearing on the question of the simultaneous decomposition of two electrolytes in the same solution, the following results may be recorded of an experiment made with a solution of mixed zinc and copper sulphates, with excess of potassium cyanide, the anode plate being of brass and the cathode plate of platinum, and an analysis made of the proportions of the two metals in the anode plate, in the solution as first taken, and in the alloy deposited on the cathode plate and subsequently dissolved off from it by means of nitric acid.

Proportion of copper to zinc.	In the brass anode plate.	In the solution electrolysed.	In the alloy deposited on the cathode plate.
Copper	68·74	13·81	73·34
Zinc	31·26	86·19	26·66
	100·00	100·00	100·00

Different results would undoubtedly have been obtained by substituting some other metal for one of those taken.

fifth series of experiments, and having immersed in it a pair of plates of "proof" gold, as already described, was employed for the deposition of the gold. The same current which traversed this cell was passed through a hydrogen voltameter of special construction,* made of glass, in a single piece, the general character of which will be seen from fig. 5.

Fig. 5.

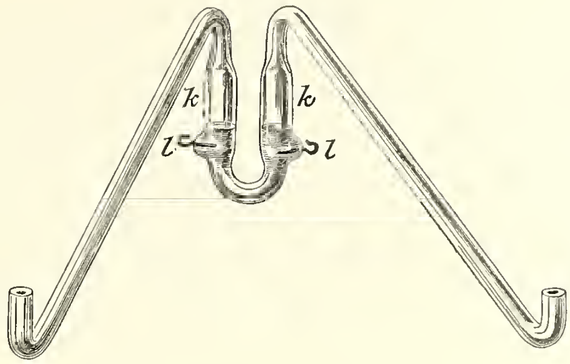


When this instrument was to be prepared for use, it was cautiously heated pretty strongly in an air-bath to remove the film of moisture and air from the internal surface, drawing dry air through by means of an aspirator. Clean mercury, previously heated, was then poured in through the funnel *a*, going down to nearly the bottom of the cylindrical vessel *b*, until this vessel—about 30 mm. in diameter and 60 mm. in height—was completely filled, and also the tubes and stop-cocks *c*, *d*, *e*, and *f*, each of these in succession being opened to allow escape of air, and afterwards closed; *f* was a three-way stop-cock, which could either be made to open communication between the parts of the tube on either side of it, or to simply close this tube, or to close this tube and establish communication between the vessel *b* and the outside air through the base of the stop-cock; it was in this last-named way that air and surplus mercury were allowed to escape, filling the tube between *b* and *f* with mercury, but not allowing of any of the

* This piece of apparatus—an excellent specimen of skilful glass-blowing—was made, from drawings furnished by me, by Mr. EMIL GREINER, of 63, Maiden Lane, New York.

metal going further along the tube towards *i*. The stopcock *c* was closed, with the tube on which it was situated completely full of mercury, and leaving surplus mercury in the funnel *a*. In filling *b* and its connected tubes care was taken to leave no visible bubbles of air. Pure water mixed with one-twelfth its weight of pure sulphuric acid was boiled for some time in a small flask to expel all dissolved air, keeping up the volume by additions from time to time of water kept boiling in a second flask ; the lower turned-up end of the tube *h* was then immersed in the dilute acid, and the lower end of *g* in a cup of mercury ; on opening the stopcocks *e* and *d* mercury ran out from *g*, and the dilute acid came in through *h*, filling about half full the cylinder *b*. Closing *d* and *e*, opening *c*, and keeping up a supply of mercury in the funnel *a*, *f* was now turned so as to force out through the base of this stopcock the little mercury in the tube behind it, and fill this tube with the acidulated water. Then *f* was turned so as to allow of this acidulated water being forced on to the bend *i* and into the two little voltameter tubes *k* and *k*, filling these about one-third full. While these tubes were being thus filled the extremities of the delivery tubes *m* and *m* were in communication with a Sprengel pump, so that they were very nearly exhausted of air. The stopcock *f* having been closed, *e* was opened, and by suitable tilting of the apparatus, and running in of mercury from the funnel *a*, nearly all of the acidulated water from *f* backwards was expelled through the tube *h*. A repetition of the procedure by which the cylinder *b* had been partially filled with acidulated water now served to partially fill it with well-boiled and still hot distilled water to which no acid had been added. The two delivery tubes *m* and *m* were severally detached from the Sprengel pump, after allowing (by a special separate arrangement of tubes with stopcocks) hydrogen to enter one of the two and oxygen the other, and when thus filled the ends of these two tubes were dipped under mercury, and the two platinum wires, *l* and *l*, sealed into the voltameter tubes were connected by the little rings on their outer ends with the terminals of the galvanic cells whence the electric current was to be derived, taking care, of course, to connect to the negative pole the wire of the tube already filled in its upper part with hydrogen, and to the positive pole the wire of the oxygen tube. Viewed from the front, the two voltameter and delivery tubes presented the appearance shown in fig. 6. The little voltameter tubes *k* and *k* had an external diameter of about 12 mm. and a length of 40 mm. The platinum wires, *l* and *l*, serving as electrodes were 1 mm. in diameter, and extended beyond the interior surface of the glass (into which they were sealed) for only 3 mm. in length. They could be well covered, and the voltameter tubes filled to one-third their capacity, with only about 2 c.cm. of the acidulated water. By careful tilting of the apparatus laterally it was found to be possible to so regulate the pressure of mercury at the ends of the delivery tubes, and therefore the gaseous tension in the two voltameter tubes, that the acidulated water was not forced over from the one to the other, which, had it occurred, would have allowed admixture of the two gases ; this required constant watching, however, and there was needed from time to time a little tapping of the apparatus to get rid of the effect of irregular adhesion of the liquid to the walls of the voltameter tubes.

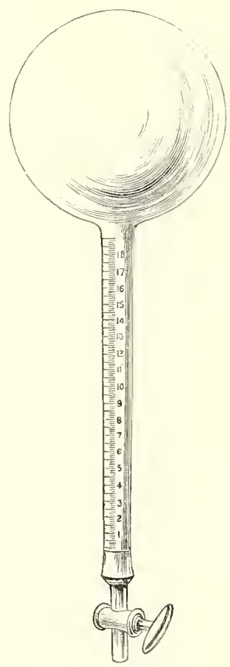
Fig. 6.



It will be seen that, with the arrangement described, the electrolysis could be effected of acidulated water, thoroughly deprived in advance of dissolved air, and in quantity so small as to be capable of retaining in solution but infinitesimal quantities of the hydrogen and oxygen electrolytically separated. As the decomposition proceeded, the quantity of liquid in the voltameter tubes could be maintained constant by opening the stopcock *c*, with a supply of mercury in the funnel *a*, and then cautiously opening *f*, so as to feed forward a little of the air-free water from the cylinder *b*, thus leaving the proportion of acid unaltered. The surface presented by the platinum wire electrodes was so small as to allow of occlusion of the gases to only an extremely minute extent, and both the hydrogen and oxygen were allowed to escape for some time before any was collected for measurement.

The hydrogen only was collected and measured. I had hoped to apply this form of voltameter to a more exact determination of the relative volumes of hydrogen and oxygen derived from water by electrolysis than is possible with the voltameters of more common construction. But I have not yet seen my way to getting over the difficulties connected with the presence of ozone, hydrogen dioxide, BERTHELOT's persulphuric acid, or other by-products in the oxygen gas evolved at the positive pole. If this could be accomplished, a useful contribution might possibly be made to the question, revived and worked upon of late by several chemists, of the exact atomic weight of oxygen. The vessel for collecting and measuring the hydrogen, shown in fig. 7, consisted of a spherical globe of tolerably stout glass, with a capacity of about 250 c.cm., having a neck of about 1 cm. internal diameter, and 22 cm. long. This neck had etched upon it a simple linear scale of millimetres. At the mouth it was fitted with a well ground perforated glass stopper, forming part of a glass stopcock with an outer orifice of about 1 mm. bore. The exact capacity of the whole globe and neck was ascertained by heating it in an air-bath to remove air and moisture condensed on the interior surface, drawing dry air through with an aspirator, then filling the globe with heated mercury, allowing it to cool to an accurately noted temperature, immersing the body of the globe in an outer vessel of mercury so as to prevent extension or flexure of the glass by the weight of the contained metal, filling up to the very mouth with mercury, inserting the stopper with the stopcock open, thus forcing out through its orifice the last of the air, closing the stopcock, removing from the orifice tube, by an iron wire, the drop or two of mercury remaining in it, and then emptying the flask, and carefully weighing in successive portions the mercury which it had held. The hydrogen from the voltameter was collected in this flask, without its stopper, the flask having been previously filled with mercury, with the needful precautions for removal of all air, and inverted over a mercury trough. In each experiment the process of electrolysis was arrested when the hydrogen had filled

Fig. 7.



the body of the globe and reached to a point rather more than half way down the length of the neck, the gold plates being of course withdrawn at the same moment from their cell of gold solution, set away to soak in distilled water, and afterwards thoroughly washed, dried, heated in the Sprengel vacuum, cooled, and weighed. The portion of hydrogen collected was dried by successive balls of fused potash introduced and withdrawn by means of platinum wire. The neck of the flask having, in advance of the collection of hydrogen, been passed through a cork, this was used to close the mouth, placed downwards, of a vessel through which a stream of water was caused to flow rapidly from the pipes supplying the University buildings. The atmospheric temperature of the day on which the electrolysis experiment was made having been such as not to differ too much from the temperature of the water from the pipes, the gas occupied such a volume after effectual exposure to this latter temperature that the mercury marked a point somewhere within the length of the neck, which point was noted by the millimetre scale, the thermometer immersed in the flowing water, and the barometer and its attached thermometer being read at the same time. It remained only to insert the stopcock stopper under the mercury of the little mercury trough, close the stopcock, withdraw the flask from the trough, reject the drop or two of mercury from the stopcock orifice by means of a wire, remove the portion of mercury left in the neck of the flask, and weigh it carefully. Its weight, with consideration of its temperature when the stopcock was closed, gave the volume of the portion of the flask not occupied by hydrogen, and this, subtracted from the whole volume of the interior of the flask, as found by the original calibration, gave the volume, under known conditions of temperature and pressure, of the hydrogen which had been collected. From two calibrations at different temperatures a correction was obtained for the expansion of the glass of the flask, but it was hardly necessary to take this into account, in view of the small limits within which temperature varied in all the experiments made.

But three experiments carried out by this method led to results which seemed worthy of confidence. These results were as follows :—

Experiment.	Character of gold in solution.	Character of gold in plates.	Gold deposited.	Hydrogen liberated.	
				Vol. at 0° C. and 760 mm.	Weight.
			grm.	c.cm.	grm.
I.	A, *b*	C	4·0472	228·64	·02053
II.	A, *b*	C	4·0226	227·03	·02039
III.	A, *b*	C	4·0955	231·55	·02079

In calculating the weight of hydrogen from its observed volume, REGNAULT'S value for the weight of a litre of this gas at 0° C. and 760 mm. was taken as the basis. The correction, of which Lord RAYLEIGH not long since pointed out the need—namely, for the compression of the vacuous glass flask by atmospheric pressure—was adopted from the experiments of J. M. CRAFTS ('Comptes Rendus,' vol. 106, p. 1662) ; and his corrected value, ·08988 grm., was still further corrected for the difference in the force of gravity at Paris and at the University of Virginia (in C. G. S. units, 980·94 : 979·95), giving as the value to be used ·08979 grm.

The electrolysis of the water was carried on very slowly, so as to keep the density of the current low with such small electrodes as were used. Nevertheless, as the hydrogen voltameter required constant watching, it became necessary to bring the whole time of an experiment within moderate limits, and hence a considerably stronger current was used than in the simultaneous deposition of gold and silver in the fifth series, this circumstance being less favourable to the satisfactory deposition of the gold. It would have been desirable to use a larger flask, and to collect a greater volume of hydrogen ; but this, on account of the time required, would have made an experiment exceedingly troublesome and difficult.

In the work of this series the same unsatisfactory need for selecting only such results as came fairly close to the figures expected, and rejecting several others on the ground of very considerable departure therefrom, and the same sources of possible constant error in regard to the gold deposit present themselves which have already been noticed under the head of the fifth series. As regards the hydrogen, one is led to consider possible diffusion of hydrogen and oxygen between the two little voltameter tubes, and slight imperfection in the drying of the hydrcgen obtained. The former would, on the whole, probably tend to diminish the volume of gas collected, and hence to raise the apparent value of the atomic weight of gold. The latter would have the opposite tendency. That neither can have had more than an extremely minute influence was fairly proved by testing a part of the hydrogen obtained, on the one hand by passing it through a red-hot glass tube, and on the other by submitting it to more extended drying by contact with phosphorus pentoxide both before and after such heating; in neither case was there appreciable change of volume.

Notwithstanding the desirability of comparing the atomic weight of any other element directly with that of hydrogen, the difficulty is not to be overlooked of doing this for an element having so high an atomic weight as that of gold. There is a manifest objection to the necessity of dealing with such minute quantities of hydrogen as those concerned in these experiments. A very small error in the determination of the hydrogen greatly affects the value found for an atomic weight nearly two hundred times as large. It is true that the measurement of the volume of the hydrogen admits of being made with such precision as to leave room for but an extremely minute error in the corresponding weight, yet this measurement is not one of *limitless* delicacy, particularly if the difficulty be properly appreciated of ascertaining with certainty the precise temperature of the gas at the time its volume is read. Moreover, in measuring the volume of the gas, and thence deducing its weight, there is need not merely for a knowledge of changes of temperature and pressure, but for *absolutely* correct readings of the barometer and thermometer, so that there must usually be a degree of hesitation in accepting the readings of even fairly standard instruments, when temperature and pressure come to be placed in comparison with these conditions as affecting the results of REGNAULT for gaseous density. Nor can the results of that great physicist be assumed as themselves free from all possible need of further correction.

The error of direct comparison with so small an atomic weight as that of hydrogen is, however, after all only *masked* by substituting an *indirect* comparison through some larger atomic weight, since the assumed value of the latter is uncertain within limits which depend upon *its* comparison with the atomic weight of hydrogen.

Seventh Series of Experiments.

In pursuance of the attempt to connect directly the atomic weight of gold with that of hydrogen, metallic zinc was prepared as nearly as possible in a state of purity, and, a known quantity of the metal having been dissolved in dilute sulphuric acid, the amount of hydrogen evolved was determined by volume. A solution of pure auric chloride or bromide was then treated with a known quantity of the same zinc, more than sufficient for the complete precipitation of all the gold present; the excess of zinc was dissolved by dilute sulphuric acid, and the volume of hydrogen given off was determined. The precipitated gold was carefully collected, washed, dried, ignited, and weighed. The difference between the volume of hydrogen which the zinc gave when thus partly used to replace a known quantity of gold and the volume which it would have given if replacing hydrogen alone represented, of course, the volume of a quantity of hydrogen equivalent to the gold precipitated and weighed. From this volume, under known conditions of temperature and pressure, the weight of the hydrogen was calculated on the basis of REGNAULT's results for the density of the gas, after application of the needful corrections, as in the sixth series of these experiments.

In a preliminary notice of my work read before the Chemical Section of the British Association at the Manchester meeting of 1887, it was pointed out that the method just described has certain advantages in principle. It does not require that the weight of the gold salt in solution be known, so that all difficulties in regard to drying such salt without decomposition are disposed of. It does not depend upon a knowledge of the atomic weight of the halogen in combination with gold, or upon a knowledge of the atomic weight of zinc. It does not even require that the zinc be of assured purity, provided only it be uniform in character, so that a given weight of it can be trusted to yield always the same quantity of hydrogen, and there be no impurities present capable of interfering with the collection of the whole of the precipitated metallic gold in a state of purity. The chief difficulty consists in the accurate ascertainment of the total volume of hydrogen evolved from the solution of a satisfactorily large quantity of zinc; when the gold solution comes to be used, as the volume of hydrogen given off on solution of the *surplus* zinc may be made quite small, its measurement becomes both easy and exact.

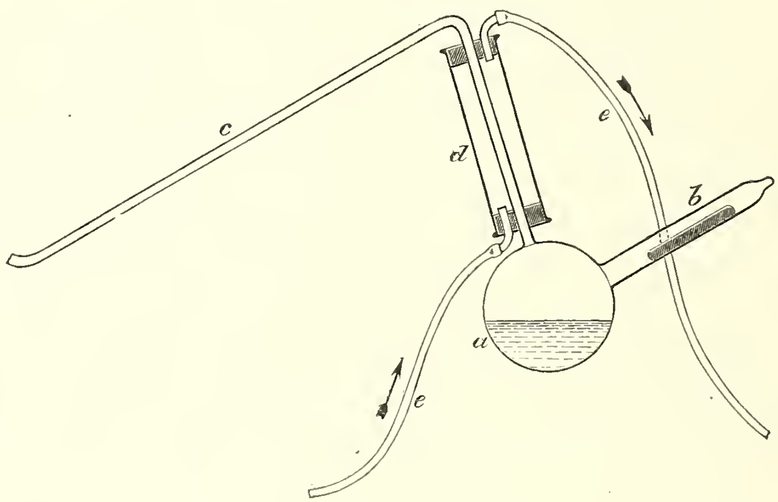
The pure zinc required was obtained by fractionally distilling in a Sprengel vacuum some very nearly pure metal from the Bertha Zinc Works, in South-western Virginia, using a long combustion-tube of hard Bohemian glass, and substantially the same arrangement of apparatus as that described by MORSE and BURTON* in connection with their work on the atomic weight of zinc. The original metal was found, by an analysis in the laboratory of the University of Virginia, to contain less than ·04 per cent. of foreign matter, almost solely consisting of lead and iron. It was four or five

* 'American Chemical Journal,' vol. 10, p. 312. Tubes of glazed porcelain, closed at one end, had been specially procured for use in thus distilling zinc, but it was found that they were quite unnecessary.

times redistilled *in vacuo*, rejecting each time about one-third of the quantity treated. The process is easily carried out, and in the final product, completely soluble in dilute sulphuric acid without visible residue, no trace of detectable impurity could be found.

For the evolution of hydrogen on solution of this zinc in acid the little piece of apparatus represented in fig. 8 was used, the same that I had used in my work of several years ago on the atomic weight of aluminum.* The description formerly given of the details of an experiment with this apparatus may be repeated with but trifling change of language. A rather more than sufficient quantity of diluted

Fig. 8.



sulphuric acid, its volume accurately measured, having been introduced into the bulb *a* by means of a little tube-funnel passed through the tube *b*, the outer end of which was originally open, taking care to leave the surface of *b* clean, the metallic zinc, in a single piece of elongated shape, and having a little bit of slender platinum wire wrapped round it, was passed into *b*, held nearly horizontal, so that the metal did not slip down into the bulb, but rested 40 or 50 mm. from it; *b* was now drawn off in the lamp flame, and sealed with a well-rounded end. The bulb was touched for a moment or two with the hand, so as to expel a very little air, and the outer end of the small tube *c* was introduced into the mercury of the trough, taking care that *b* was still kept in such a position as to prevent the zinc coming in contact with the dilute acid. After a sufficient lapse of time for the apparatus to have acquired the temperature of the room, the barometer and thermometer and the difference of level of the mercury in the trough and in *c* were read off;† so that, knowing the volume of dilute acid introduced and of metallic zinc (the latter from its weight), calibration of the bulb and tubes after the experiment was over completed the data necessary to determine the volume of air which the apparatus contained at the beginning. The

* 'Phil. Trans.,' 1880, p. 1026.

† All readings were, of course, made from a distance with the aid of a small telescope.

piece of zinc was now made to slide down into the bulb, the end of the gas delivery-tube c having been brought under the mouth of the measuring flask. Over-rapid evolution of hydrogen and any considerable rise of temperature were prevented, partly by tilting the bulb so that the little piece of zinc rested against one side and exposed but a part of its surface to the action of the liquid, and partly by cooling the outside of the bulb with water. To guard against more than traces of aqueous vapour being carried away with the hydrogen, a rapid current of ice-water was kept up through d.

As soon as the last of the zinc had disappeared, leaving the liquid quite clear, c was brought up into a nearly vertical position, and the apparatus left to itself until the temperature of the room had been attained. The barometer and thermometer and the height of the mercury in c above that in the trough were now read and recorded.

Lifting c straight up from the trough, the mercury in this tube was got out by running a wire up and down in it, and, inverting it, the whole of the remaining space in a, b, and c was filled up with solution of zinc sulphate and free acid of the same strength with that already contained, this liquid being run in from a graduated burette through a slender tube-funnel, and the volume used noted, so as to show how much liquid had been already present.

The apparatus having been now emptied, washed out, and calibrated (with water, instead of mercury, on account of the difficulty of getting the interior quite dry), the volume of gas remaining in it at the close of the experiment was had from the difference between the total capacity (to the level of the mercury in c) and the volume of liquid which the bulb had contained at the close of the experiment, these taken together with the data for pressure and temperature.

The dilute acid was saturated with pure hydrogen just before being used (and in the experiments with auric chloride or bromide the main portion of water holding this salt in solution was similarly treated), and a preliminary experiment showed that there was but an extremely minute difference between the amount of gas removable from such liquid by heating in a Sprengel vacuum and from that containing zinc sulphate after the solution of the metal ; so that, practically, the question of retention of gas in solution by the liquid might be neglected.

The sulphuric acid was diluted to 25 per cent. by weight, only a small bit of platinum wire was wrapped round the zinc, and the temperature of the bulb was not allowed to rise beyond about 20° C. Thus the risk of evolving other gaseous products than hydrogen*—as hydrogen sulphide or sulphur dioxide—was avoided, and on testing for these impurities the hydrogen collected no traces of them were found.

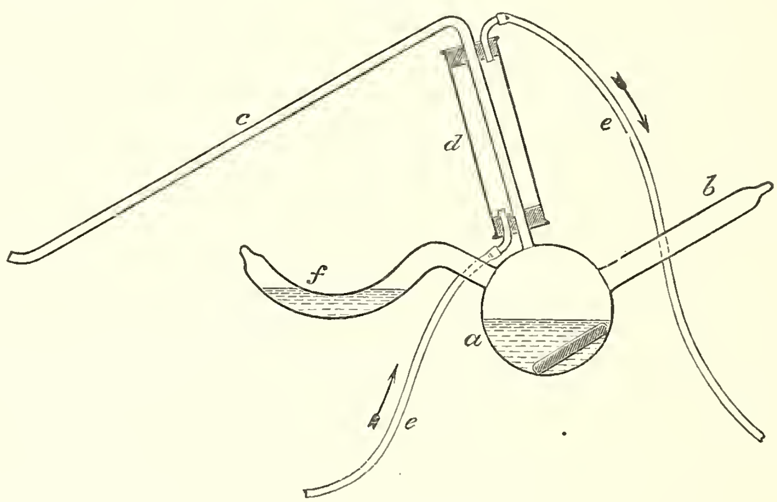
The measuring flask used to collect the hydrogen was of the same character as that used for the experiments of the sixth series, but of much larger size, holding

* MUIR and ADIE: "On the Interaction of Zinc and Sulphuric Acid," 'Chem. Soc. Journ.,' Jan., 1888, p. 47.

about a litre. The quantity of zinc taken for each experiment was calculated to give a volume of gas which, under the conditions of temperature and pressure of the day, would bring the mercury to somewhere near the middle of the neck, and the gas, previously dried by balls of fused potash, was measured after the temperature had been rendered as nearly as possible fixed by the circulation round the outside of the flask of an active stream of water from the laboratory supply pipes. On account of slight rise of temperature during the solution of the metal, the volume of hydrogen left in the bulb and tubes was always less than that of the air in the same at the beginning; and, after reduction to normal temperature and pressure, the difference had to be subtracted from the gas collected in the flask.

In the experiments with auric chloride or bromide the quantity of hydrogen given off on solution of the *surplus* zinc was so small that it could be easily measured in a little gas tube, the same method of double calibration with mercury being used as for the larger volumes. In these experiments the bulb used had a second side tube, *f*, as shown in fig. 9, to hold the sulphuric acid, while *a* contained the aqueous solution

Fig. 9.



of the gold salt; this acid was already somewhat diluted, and was introduced into *a*, after complete precipitation of the gold, very gradually, so as to avoid any considerable rise of temperature. The quantity of water used was such as to make the whole volume of liquid very nearly the same in the experiments with zinc alone and in those with zinc and the auric salt. Care was taken to ascertain, after measurement of the hydrogen, that it had been effectually freed by the potash balls not only from moisture, but from any traces of hydrochloric acid formed and carried over.

In order to connect the weight of the zinc with that of the hydrogen produced by its solution, it was necessary that the weight of the metal should be *absolute*, or in terms of equal value with those used in REGNAULT's researches on the density of hydrogen; hence, as has been already stated, the weights used were such as had had their real values determined, and the precaution of double weighing was applied.

The quantities of metal used being small, the centre of gravity of the balance beam was so adjusted as to give great sensitiveness. In calculating the weight of the hydrogen from its volume, the same value for the weight of a litre of the gas was assumed as has been already stated, viz., ·08979 grm., being the result of REGNAULT'S determinations, with the correction pointed out by Lord RAYLEIGH and numerically estimated by CRAFTS, and further corrected for the force of gravity at the University of Virginia.

The haloid salts of gold were prepared as for the experiments of the first and second series, and the careful filtration of their solutions was followed by long continued standing at rest before the portions required were gently drawn off for use. Great care was taken in removing the last traces of precipitated gold from the bulb—to facilitate which the connected tubes were all cut off short—and in repeatedly washing the metal, first with dilute sulphuric acid, then with pure hydrochloric acid, and, finally, with water, before drying, heating (in the Sprengel vacuum), cooling, and weighing.

The results obtained by this method were much freer from irregularity, and much more satisfactory, than those of the electrolytic experiments. All are reported, except one or two cases obviously vitiated by mechanical defects of manipulation, and, in consequence, not carried out to the end.

Experiments with Zinc alone.

Experiment.	Zinc dissolved.	Hydrogen obtained, at 0° C. and 760 mm.		Hydrogen, at 0° C. and 760 mm., for 1 grm. of zinc.
	grm.	c.cm.		c.cm.
I.	2·6990	922·64	Equivalent to	341·85
II.	2·6771	915·33		341·91
III.	2·7029	924·20		341·93
IV.	2·7117	927·51		342·04

Or a total amount of 10·7907 grm. of zinc gave 3689·68 c.cm. of gas,* equivalent to 341·93 c.cm. of hydrogen for 1 grm. of zinc. This value was adopted in calculating the fifth column of the following table.

* These figures represent an atomic weight for zinc = 65·142, taking the weight of a litre of hydrogen at 0° C. and 760 mm. as ·08979 grm., and assuming the zinc used to have been absolutely pure, and the quantity of hydrogen collected to have been strictly equivalent to it; neither of the two latter assumptions is essential to the use made in this paper of the experiments. REYNOLDS and RAMSAY in their recent paper ('Chem. Soc. Journ.,' Dec., 1887, p. 854) on the atomic weight of zinc arrive at a somewhat higher value, on the basis of a like comparison of the weight of the metal with the volume of hydrogen liberated by it, but they assume the weight of the litre of hydrogen under normal temperature and pressure as ·0896 grm., which must be considered too low in view of the recently applied correction of Lord RAYLEIGH.

Experiments with Gold Salt and Zinc.

Experiment.	Character of gold used.	Character of gold salt.	Gold precipitated	Hydrogen, at 0° C. and 760 mm.		Hydrogen equivalent to gold.	
				Corresponding to total zinc.	Obtained from residual zinc.	Vol. at 0° C. and 760 mm.	Weight.
			grm.	c.cm.	c.cm.	c.cm.	grm.
I.	A, *b*	$AuCl_3$	10·3512	1779·44	−23·34	=1756·10	=·15768
II.	A, *b*	$AuBr_3$	8·2525	1428·99	−28·61	=1400·38	=·12574
III.	A, *b*	$AuCl_3$	8·1004	1393·43	−18·56	=1374·87	=·12345
IV.	C	$AuCl_3$	3·2913	582·82	−24·18	= 558·64	=·05016
V.	C	$AuBr_3$	3·4835	606·20	−15·27	= 590·93	=·05306
VI.	D	$AuBr_3$	3·6421	643·31	−25·20	= 618·11	=·05550

In considering possible causes of constant error in the experiments of this last series it seems most likely that they would affect the exact determination of the weight of the precipitated gold, either by mechanical loss of some minute particles of the metal, tending to lower the atomic weight, or by incomplete washing out of the zinc salt, with an influence in the opposite direction. Any failure to remove the last traces of moisture from the hydrogen was, I think, effectually guarded against, at any rate within such limits as would have sensibly affected the resulting atomic weight; and any error due to retention of hydrogen in solution by the liquid must also have been inappreciably small, in view of the precautions taken and the close similarity of conditions in the experiments with zinc alone and with zinc and the auric salt.

Calculation of Results.

In calculating the atomic weight of gold from the data furnished by the experiments which have been described, I have thought it best to conform to the most general usage of those who have been working on questions of this sort of late years, so as to facilitate comparisons with the results of others. Hence, although the atomic weight has been calculated separately from the figures of each experiment reported, the value deduced from each series has not been taken as the arithmetical mean of the separate results, nor has the probable error of these or of the mean been calculated by the method of least squares, as was done in my paper on the atomic weight of aluminum, but, instead, the general result for each series has been obtained, as in the calculations of MEYER and SEUBERT, from the aggregate quantities of the materials employed, though I am by no means convinced that this mode of reckoning is in all cases sound in principle, giving, as it does, weight to each experiment in proportion to the quantity of material employed.

The atomic weights assumed for the other elements involved are those which have been most generally accepted in calculations of this kind, based for the most part on the experiments of STAS, and representing, with greatest probability, the values as at present known to us. They are as follows :—

$$H = 1.$$
$$Ag = 107{\cdot}66.$$
$$Cl = 35{\cdot}37.$$
$$N = 14{\cdot}01.$$
$$C = 11{\cdot}97.$$

Calculated Results.

The following are the values obtained for the atomic weight of gold from the different series of experiments :—

FIRST Series.

($Ag_3 : Au :: 322{\cdot}98 : x$.)

Experiment.	Silver.	Gold.	Atomic weight of gold.
	grm.	grm.	
I.	12·4875	7·6075	196·762
II.	13·8280	8·4212	196·694
III.	11·3973	6·9407	196·688 Lowest value
IV.	5·5286	3·3682	196·770 Highest value
V.	4·6371	2·8244	196·723
	47·8785	29·1620	196·722

SECOND Series.

($Ag_3 : Au :: 322{\cdot}98 : x$.)

Experiment.	Silver.	Gold.	Atomic weight of gold.
	grm.	grm.	
I.	13·5149	8·2345	196·789
II.	12·6251	7·6901	196·731 Lowest value
III.	17·2666	10·5233	196·843 Highest value
IV.	4·5141	2·7498	196·746
V.	5·8471	3·5620	196·756
VI.	6·4129	3·9081	196·828
	60·1807	36·6678	196·790

Third Series.

(Ag_1 : Au : : 430·64 : x.)

Experiment.	Silver.	Gold.	Atomic weight of gold.	
	grm.	grm.		
I.	12·4851	5·7048	196·772	
II.	17·4193	7·9612	196·817	Highest value
III.	5·3513	2·4455	196·799	
IV.	9·1153	4·1632	196·685	Lowest value
	44·3710	20·2747	196·775	

Fourth Series.

($N(CH_3)_3HCl_4$: Au : : 201·40 : x.)

Experiment.	Loss by ignition of trimethyl-ammonium auri-chloride.	Gold.	Atomic weight of gold.	
	grm.	grm.		
I.	7·5318	7·3754	197·218	
II.	7·8432	7·6831	197·289	Highest value
III.	5·2811	5·1712	197·209	
IV.	3·3309	3·2603	197·131	Lowest value
V.	2·8165	2·7579	197·210	
	26·8035	26·2479	197·225	

Fifth Series.

(Ag : Au : : 107·66 : x.)

Experiment.	Silver.	Gold.	Atomic weight of gold.	
	grm.	grm.		
I.	2·8849	5·2721	196·747	
II.	3·4487	6·3088	196·945	Highest value
III.	2·3393	4·2770	196·837	
IV.	1·9223	3·5123	196·709	Lowest value
V.	2·0132	3·6804	196·817	
	12·6084	23·0506	196·823	

SIXTH Series

(H : Au : : 1 : x.)

Experiment.	Hydrogen.	Gold.	Atomic weight of gold.
	grm.	grm.	
I.	·02053	4·0472	197·136
II.	·02039	4·0226	197·283 Highest value
III.	·02079	4·0955	196·994 Lowest value
	·06171	12·1653	197·137

SEVENTH Series.

(H_3 : Au : : 3 : x.)

Experiment.	Hydrogen.	Gold.	Atomic weight of gold.
	grm.	grm.	
I.	·15768	10·3512	196·941
II.	·12574	8·2525	196·894
III.	·12345	8·1004	196·851
IV.	·05016	3·2913	196·848 Lowest value
V.	·05306	3·4835	196·956 Highest value
VI.	·05550	3·6421	196·865
	·56559	37·1210	196·897

General Mean of Results.

If each of the foregoing series of experiments be represented by the result calculated from the aggregates of material used, and if equal weight be attached to the results of all the methods, the general mean derived from the whole of the 34 experiments will be as follows :—

First series	196·722	Lowest value.
Second series	196·790	
Third series	196·775	
Fourth series	197·225	Highest value.
Fifth series	196·823	
Sixth series	197·137	
Seventh series	196·897	
General mean	196·910	

The results of the fifth and sixth series, obtained by electrolysis, are, I am convinced, much less entitled to confidence than any of the others. If these two be excluded, the general mean of the remaining series will be **196·882**, a number differing but little from the mean of all.

The highest value is that derived from the fourth series—ignition of trimethyl-ammonium auri-chloride. It has been seen that the individual results of this series agree fairly well with one another, and, when examined in connection with the facts as to the different crops of crystals of the salt used, do not seem to present any evidence of want of uniformity in the material. But, as it may still be suspected that traces of dimethyl- or of monomethyl-ammonium auri-chloride may have been present, and have caused the apparent value of the atomic weight of gold to come out higher than the truth, if we exclude also this series, the general mean of the remaining four will be **196·796**.

Finally, if for the sake of comparison with the results of KRÜSS and of THORPE and LAURIE the general mean be taken for the first three series only, in which auric chloride and bromide were examined, the result is **196·762**—intermediate between the general means of the two previous researches, but rather nearer to that derived from the work of THORPE and LAURIE than of KRÜSS.

It will be observed that, although there is pretty close agreement among the means of results obtained by altogether different methods, this agreement is not so close as that presented by the results of the nearly similar methods pursued in the first three series. This cannot but suggest the probability of there being still sources of minute errors inherent in the methods themselves, and not dependent upon mere imperfections of manipulation in carrying these methods out. Although there is thus to be noticed a slight tendency on the part of each method to yield high or low figures severally, with the exception of the results of the fourth series there does not appear to be any considerable reason to see in the values obtained confirmation of the special suspicions in connection with each method which have been stated. There is no clear evidence of any difference in the results which can be traced to the history of the particular samples of gold used; a larger number of somewhat low results seem to have been yielded by the metal designated as (C)—*i.e.*, obtained from the United States Assay Office at New York—than by the others, but the difference is not marked or constant enough to warrant any trustworthy conclusions as to the character of this material.

Concluding Remarks.

The atomic weight of gold as deduced from the experiments reported in this paper is entirely in accord with the place occupied by the metal in MENDELEJEFF'S "periodic" classification of the elements, but this is equally true of the slightly different values obtained by KRÜSS, and by THORPE and LAURIE, and the only difficulty at one time apparent as to this point—namely, the relative positions of gold on the one hand and of platinum, iridium, and osmium on the other—has been removed, not by any change in the atomic weight of gold, but by changes affecting the values to be assigned the three other metals, as these values have been determined by SEUBERT.* It is very desirable that, in order to a fuller and more exact examination of the Mendelejeff table of the elements, there be accomplished as soon as possible a general revision of the atomic weights of *all* the elements of well determined individuality, so many of which are still very imperfectly known.

As to any bearing of the results of the present paper on the so-called hypothesis of PROUT,† the general mean of all my results, or even the general mean with exclusion of the values obtained by electrolysis, approaches the integer number 197 rather more nearly than does the final number arrived at by THORPE and LAURIE, and still more nearly than does that considered by KRÜSS to express the final result of his experiments. If the results of the fourth series be also rejected, my general mean will be nearer the integer than is the KRÜSS number, but not quite so near as that of THORPE and LAURIE. I feel that somewhat greater confidence may be placed in my own work, simply on the ground of its involving the use of more completely different and independent methods—a principle which I believe to be of the first importance in any attempts at increased accuracy in the determination of atomic weights.

At the same time, as has already been pointed out, this work seems to me to furnish some probable evidence that not all inherent defects of method have been eliminated. Whether or not such defects may exist to an extent sufficient to account for the remaining difference between the value obtained and the integer multiple of the atomic weight of hydrogen there does not seem to be ground on which to express a positive opinion. But this research does not supply any clear evidence contradictory of such a possibility.

On this point, and generally on the attainment of what is sometimes rather too easily spoken of as the greatest possible accuracy in the determination of an atomic weight—particularly of an element for which the value is as high as that for gold—any one who actually works in a conscientious way at such determinations will be pretty sure to feel more strongly the difficulty of the task, and to express himself with more caution, than do some compilers of results in assuming at any time that the last word has been spoken.

* 'Berichte Deutsch. Chem. Gesell.,' vol. 11, p. 1770; vol. 14, p. 868; vol. 21, p. 1839.

† Soon after the publication of my paper on the atomic weight of aluminum, I was criticised by a writer of abstracts for the German Chemical Society on account of my use of the expression "PROUT's law," amazement being indicated that I should have called the "hypothesis" of PROUT a *law*. If this writer had noticed my use of inverted commas, and still more what was said in the course of two or three pages of the paper, he would have seen that the use of the expression "PROUT's law" was by no means equivalent to assuming this to be "a law of nature."

XIV. *Magnetic and other Physical Properties of Iron at a High Temperature.*

By JOHN HOPKINSON, *M.A., D.Sc., F.R.S.*

Received April 16,—Read May 9, 1889.

[PLATES 12–20.]

IT is well known that for small magnetising forces the magnetisation of iron, nickel, and cobalt increases with increase of temperature, but that it diminishes for large magnetising forces.* BAUER† has also shown that iron ceases to be magnetic somewhat suddenly, and that the increase of magnetisation for small forces continues to near the point at which the magnetism disappears. His experiments were made upon a bar which was heated in a furnace and then suspended within a magnetising coil and allowed to cool, the observations being made at intervals during cooling. This method is inconvenient for the calculation of the magnetising forces, and the temperature must have been far from uniform through the bar. In my own experiments‡ on an impure sample of nickel the curve of magnetisation is determined at temperatures just below the temperature at which the magnetism disappears, which we may appropriately call the critical temperature.

AUERBACH§ and CALLENDAR|| have shown that the electrical resistance of iron increases notably more rapidly than does that of other pure metals. BARRETT,¶ in announcing his discovery of recalescence, remarked that the phenomenon probably occurred at the critical temperature. TAIT** investigated the thermo-electric properties of iron, and found that a notable change occurred at a red heat, and thought it probable that this change occurred at the critical temperature.

It appeared to be very desirable to examine the behaviour of iron with regard to magnetism near the critical temperature, and to ascertain the critical temperatures

* ROWLAND, 'Phil. Mag.,' Nov., 1874.

† 'WIEDEMANN, Annalen,' vol. 11, 1880.

‡ 'Roy. Soc. Proc.,' June, 1888.

§ 'WIEDEMANN, Annalen,' vol. 5, 1878.

|| 'Phil. Trans.,' A, 1887.

¶ 'Phil. Mag.,' Jan., 1874.

** 'Edinburgh Roy. Soc. Trans.,' Dec., 1873.

for different samples. It also appeared to be desirable to trace the resistance of iron wire up to and through the critical temperature, and to examine more particularly the phenomenon of recalescence, and determine the temperature at which it occurred.

The most interesting results at which I have arrived may be shortly stated as follows :—

For small magnetising forces the magnetisation of iron steadily increases with rise of temperature till it approaches the critical temperature, when it increases very rapidly, till the permeability in some cases attains a value of about 11,000. The magnetisation then very suddenly almost entirely disappears.

The critical temperatures for various samples of iron and steel range from 690° C. to 870° C.

Heating iron a little above the critical temperature does not entirely wipe out all effects of previous magnetisation.

The temperature coefficient of electrical resistance is greater for iron than for other metals; it increases greatly with increase of temperature till the temperature reaches the critical temperature, when it suddenly changes to a value more nearly approaching to other metals. Recalescence does occur at the critical temperature. The quantity of heat liberated in recalescence has been measured and is found to be quite comparable with the heat required to melt bodies.

Since making the experiments and writing the preliminary notes which have already appeared in the 'Proceedings of the Royal Society,' my attention has been called to two papers which deal in part with some of the matters on which I have been experimenting. PIONCHON* has shown that the specific heat of iron is very much greater at a red heat than at ordinary temperatures. W. KOHLRAUSCH,† in an interesting paper, shows that, whereas the temperature coefficient of resistance of iron is much greater than usual for temperatures below the critical temperature, it suddenly diminishes on passing that temperature. He also identifies the temperature of recalescence with the critical temperature. So far as resistance of iron is concerned, W. KOHLRAUSCH has anticipated my results, which I give, however, for the sake of completeness.

Magnetic Experiments.

The method of performing the magnetic experiments was the same as that used by ROWLAND. The copper wire was, however, insulated carefully with asbestos paper laid over the wire, and with layers of asbestos paper between the successive layers of the wire. The insulation resistance between the primary and the secondary coils was always tested, both at the ordinary temperature and at the maximum temperature used. At the ordinary temperature this resistance always exceeded a megohm; at

* 'Comptes Rendus,' vol. 103, p. 1122.

† 'WIEDEMANN, Annalen,' vol. 33, 1888.

the maximum temperature it exceeded 10,000 ohms, and generally lay between 10,000 and 20,000 ohms. The ring to be examined, with its coils of copper wire, was placed in a cylindrical cast-iron box, and this in a Fletcher gas furnace, the temperature of which was regulated by the supply of gas. The temperatures were estimated by the resistance of the secondary coil. It was observed that the resistance of this coil at the ordinary temperature increased slightly after being raised to a high temperature; this I attribute to oxidation of the wire where it leaves the cast-iron box. However, it introduced an element of uncertainty into the determination of the actual temperatures, amounting, perhaps, to 20° C. at the highest temperature. This error will not affect the differences between neighbouring temperatures, with which we are more particularly concerned.

The resistance of the ballistic galvanometer is 0·43 ohm; to this additional resistances were added to give the necessary degree of sensibility. The ratio of two successive elongations of the galvanometer is $(1+r)/1 \doteq 1{\cdot}12/1$. The time of oscillation T and the sensibility varied a little during the experiments, but so little, that the correction would fall within the limits of errors of observation in these experiments.

The total induction $= \left\{\left(1+\frac{r}{2}\right)\frac{C}{\alpha}\frac{T}{2\pi}\right\}\frac{1}{2n}\,R.A.10^8$, where C is the current which gives the deflection α, n is the number of turns in the secondary coil, R the resistance of the secondary circuit, A the mean of the first and second elongations on reversal of the current in the primary.

The magnetising force $= 4\pi mc/l$, where m is the number of turns in the primary, l the mean length of lines of force in the ring, c the current in absolute measure in the primary.

With my galvanometer as adjusted, a Grove's cell, the E.M.F. of which was at the time determined to be 1·800 volt, gave a deflection of 158·5 divisions through a resistance of 50,170 ohms, whence

$$\frac{C}{\alpha} = \frac{1{\cdot}800}{158{\cdot}5 \times 50{,}170} = 0{\cdot}0000002264,$$

$$T = 13{\cdot}3.$$

Hence

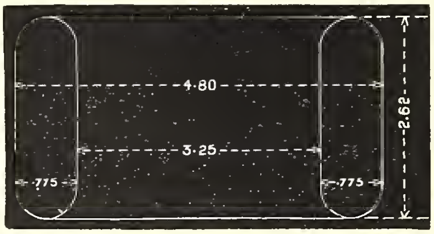
$$\left(1+\frac{r}{2}\right)\frac{C}{\alpha}\frac{T}{2\pi} = 5{\cdot}09 \times 10^{-7}.$$

The ring method of experiment is open to the objection that the magnetising force is less in the outer than in the inner portions of the ring. The results, in fact, give the average results of forces which vary between limits.

Wrought Iron.—The sample of wrought iron was supplied to me by Messrs.

MATHER and PLATT. I have no analysis of its composition. I asked for the softest iron they could supply.*

The dimensions of the ring were as shown in the accompanying sketch :—



The area of section is 1·905 sq. cm. The area of the middle line of the secondary coil is estimated to be 2·58 sq. cms. This estimate is, of course, less accurate than the area of section of the ring itself.

The secondary coil had 48 convolutions, the primary 100 convolutions.

At the beginning of the experiments the insulation resistance of the secondary from the primary was in excess of 1 megohm; the resistance of the secondary and the leads was 0·692, the temperature being 8°·3 C.

The resistance of the leads to the secondary and of the part of the secondary external to the furnace was estimated to be 0·04.

A curve of magnetisation was determined at the ordinary temperature on the virgin sample with the following results, shown graphically in Curve I.; in each case the observation was repeated twice with reversed direction of magnetising currents, and the kicks in the galvanometer were found to agree very closely together :—

Magnetising force .	0·15	0·3	0·6	1·2	2·2	4·4	8·2	14·7	24·7	37·2	69·2
Induction per sq. cm.	39·5	116	329	1,560	6,041	10,144	12,633	14,059	14,702	15,149	15,959

The ring was next heated and observations were made with a magnetising force of 8·0 to ascertain roughly the point at which the magnetism disappeared. After the magnetism had practically disappeared and the temperature was roughly constant, as indicated by the resistance, being 2·92 before the experiment and 2·85 after the experiment, corresponding with temperatures of 838° C. and 812° C., the induction was determined for varying magnetising forces.

Magnetising force . . .	2·4	4·2	8·0	21·0	49·8
Total induction	small	12·3	22·7	58·2	143

This shows that the induction is, so far as the experiment goes, proportional to the inducing force.

Taking the total induction as 143, corresponding to a force of 49·8, we have

* [*Added July* 2, 1889.—Sir JOSEPH WHITWORTH and Co. have since kindly analysed this sample for me with the following result:—

	C	Mn	S	Si	P	Slag†
Per cent. . . .	·010	·143	·012	Nil	·271	·436

† Containing 74 per cent. SiO_2 (Silica).]

induction in the iron 109, or 57 per sq. cm., giving permeability equal to 1·14, showing that the material has suddenly become non-magnetic.

The ring was now allowed to cool, some rough experiments being made during cooling. When cold the resistance of the secondary and the leads was found to be 0·697 ohm. The ring was again heated till the resistance of the secondary reached 2·845 and the magnetism had disappeared. It was next allowed to cool exceedingly slowly, and the following observations were made with a magnetising force of 0·075 C.G.S. unit :—

Resistance of secondary . .	2·81	2·80	2·79	2·78	2·765
Temperature	796°	792°	788°	785°	781°
Induction per sq. cm. . . .	0	0	0	126·8	

showing that magnetisation returns at a temperature corresponding to resistance between 2·78 and 2·765.

Systematic observations then began. The results are given in the following tables and the curves to which reference is made. The curves are in each case set out to two scales of abscissæ, the better to bring out their peculiarities.

TABLES 1–4.

	Table 1, Curve II.	Table 2, Curve III.	Table 3, Curve IV.	Table 4, Curve V.
Resistance of secondary before experiment	2·76	2·75	2·72	2·67
Temperature of secondary before experiment	778° C.	775° C.	763° C.	744° C.
Resistance of secondary after experiment	2·75	2·73	2·695	2·66
Temperature of secondary after experiment	775° C.	767° C.	754° C.	741° C.

Magnetising force.	Induction per sq. cm.	Magnetising force.	Induction per sq. cm.	Magnetising force.	Induction per sq. cm.	Magnetising force.	Induction per sq. cm.
0·075	511·8	0·075	494	0·075	328	0·075	227
0·15	1313·9	0·15	1033		260		180
0·3	2482·6	0·30	3286	0·15	710	0·15	473
0·6	3257·4	0·6	4520		635		425
1·2	3659·2	1·2	5367	0·3	2304	0·3	1281
2·4	4104·0	2·4	5668	0·6	5281		1172
4·4	4520·0	4·2	6056	1·2	6544	0·6	5377
		7·8	6228	2·2	7318	2·2	8165
		12·8	6587	7·6	8036	7·6	9295
		45·2	6945	13·0	8323	47·2	9781
				46·6	8581		

TABLES 5–8.

Table 5, Curve VI.		Table 6, Curve VII.		Table 7, Curve VIII.		Table 8, Curve IX.	
Resistance of secondary before experiment	2·61	2·47		2·29		2·0	
Temperature of secondary before experiment	722° C.	670° C.		603° C.		494° C.	
Resistance of secondary after experiment	2·61	2·47		2·21		1·94	
Temperature of secondary after experiment	722° C.	670° C.		573° C.		472° C.	
Magnetising force.	Induction per sq. cm.	Magnetising force.	Induction per sq. cm.	Magnetising force.	Induction per sq. cm.	Magnetising force.	Induction per sq. cm.
0·075	163 125	0·075	77	0·075	68 50	0·075	54·7 35·8
0·15	305 278	0·15	162	0·15	128 108	0·15	98 75
0·30	762 726	0·3	427	0·30	307 275	0·3	245 195
0·6	4,004	0·6	1,516	0·60	908 834	0·6	742 590
2·2	8,952 8,895	2·2	9,381	2·2	9,604	2·2	9,433
7·6	10,410	7·6	11,562	7·6	11,992	7·6	12,273
47·2	11,224 11,111	47·8	12,859	50·6	14,470	53·5	15,201

At this stage the ring was allowed to cool down, and on the following day a determination was made of the curve at ordinary temperature of 9°·6 C. (Curve X.)

Magnetising force	0·075	0·15	0·3	0·6	1·2	2·2	4·0	6·8	11·4	17·3	57·0
Induction per sq. cm.	21·6 13·0	41·1 32·0	116 93	308 273	1,482	6,912	10,341	12,410	13,640	14,255	15,623

The ring was next heated till the resistance reached about 2·4, was allowed to cool somewhat, and a curve was determined (Curve XI.) at a resistance of 1·69 to 1·64. Temperature 378° C. to 354° C.

Magnetising force	0·075	0·15	0·3	0·6	1·2	2·2	4·0	7·6	13·1	51·7
Induction per sq. cm.	38 44	93 101	263	874	4,288	8,818	11,296	12,589	13,404	15,174

In addition to the variation of magnetisability depending on the temperature, these numbers show one or two interesting facts. Where two observations are given these are the results of successive reversals in opposite directions. After each experiment the ring was demagnetised by reversals of current; thus currents successively diminishing in amount were passed through the primary, each current being reversed ten times. The last currents gave magnetising forces 1·2, 0·6, 0·3, 0·15, 0·075, 0·05. The inequality of successive observations is due to the residual effect of the current last applied ; it is remarkable to observe how greatly this small force affects the result. In Curve XI. the first deflection was caused by a reversal of a current opposite to the last demagnetising current.

Comparing Curves X. and I. we see that the effect of working with the sample is to diminish its magnetisability for small forces, a fact which will be better brought out later.

Referring now to the temperature effects, we see that as the temperature rises the steepness of the initial part of the curve increases, but the maximum magnetisation diminishes. The coercive force, that is, the force required to completely demagnetise the material after it has been exposed to a great magnetising force, also, judging from the form of the ascending curves, diminishes greatly.

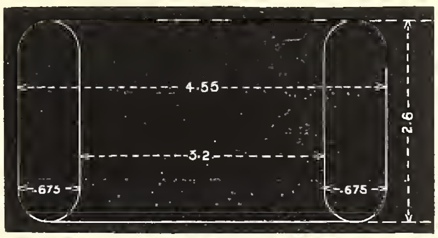
In Curves XII., XIII., and XIV. the abscissæ are temperatures, and the ordinates are induction-magnetising force, called by Sir WILLIAM THOMSON the permeability, and usually denoted by μ. These curves correspond to constant magnetising forces of 0·3, 4·0, 45·0. They best illustrate the facts which follow from these experiments. Looking at the curve for 0·3, we see that the permeability at the ordinary temperature is 367 ; that as the temperature rises the permeability rises slowly, but with an accelerated rate of increase ; above 681° C. it increases with very great rapidity, until it attains a maximum of 11,000 at a temperature of 775° C. Above this point it diminishes with extreme rapidity, and is practically unity at a temperature of 786° C.

Regarding the iron as made up of permanently magnetic molecules, the axes of which are more or less directed to parallelism by magnetising force, we may state the facts shown by the curve by saying that rise of temperature diminishes the magnetic moment of the molecules gradually at first, but more and more rapidly as the critical temperature at which the magnetism disappears is approached, but that the facility with which the molecules have their axes directed increases with rise of temperature at first slowly, but very rapidly indeed as the critical temperature is approached.

Whitworth's Mild Steel.—This sample was supplied to me by Sir JOSEPH WHITWORTH and Co., who also supplied me with the following analysis of its composition :—

	C	Mn	S	Si	P
Per cent. . .	·126	·244	·014	·038	·047

The dimensions of the ring were as shown in the accompanying sketch.



The area of section of the ring is 1·65 sq. cm. The area of the middle line of the secondary coil is estimated to be 2·32 sq. cms.

The secondary coil had 56, the primary 98, convolutions.

The resistance of the secondary and leads was 0·81 at 12° C.

The ring was at once raised to a temperature at which it ceased to be magnetic; with a magnetising force of 32·0, the total induction was observed to be 80·8, giving the value of the permeability 1·12.

The insulation resistance between the primary and the secondary was observed to be 12,000 ohms.

The ring was now allowed to cool very slowly; at resistance of 3·00, corresponding to a temperature of 723° C., the ring was non-magnetic; at 2·99, corresponding to 720° C., it was distinctly magnetic.

The following five series of observations were made at descending temperatures, the means of two observations being in each case given; the sample was demagnetised by reversals after each experiment:—

TABLES 9–13.

	Table 9, Curve XV.	Table 10, Curve XVI.	Table 11, Curve XVII.	Table 12, Curve XVIII.	Table 13, Curve XIX.
Resistance at beginning of experiment	2·99	2·71	2·31	1·80	0·812
Temperature at beginning of experiment	721° C.	630° C.	500° C.	333° C.	12° C.
Resistance at end of experiment	2·95	2·75	2·245	1·80	0·812
Temperature at end of experiment	708° C.	645° C.	478° C.	333° C.	12° C.

Table 9: Magnetising force.	Table 9: Induction per sq. cm.	Table 10: Magnetising force.	Table 10: Induction per sq. cm.	Table 11: Magnetising force.	Table 11: Induction per sq. cm.	Table 12: Magnetising force.	Table 12: Induction per sq. cm.	Table 13: Magnetising force.	Table 13: Induction per sq. cm.
0·075	607	0·075	140	0·075	77	0·075	73	0·075	19
0·15	1214	0·15	295	0·15	161	0·15	125	0·15	48
0·3	2031	0·3	1,098	0·30	396	0·30	293	0·3	119
0·6	2698	0·6	4,175	0·60	1,847	0·6	813	0·6	312
1·2	3181	1·2	6,163	1·2	5,217	1·2	4,552	0·9	884
2·2	3607	2·1	8,122	2·1	7,642	2·1	7,840	1·7	5,087
7·6	4118	7·5	10,900	7·7	11,586	7·4	12,232	3·3	9,535
36·9	4800	38·0	12,074	40·4	14,816	42·6	15,180	6·1	12,387
								10·7	13,991
								45·0	16,313

The following experiment is instructive, as showing a phenomenon which constantly recurs, namely, that after not quite perfect demagnetisation, as above described, the first kick of the galvanometer being in the same direction as the last magnetising force, the first kick is very materially greater than the reverse kick for small magnetising forces, is somewhat less for medium forces, and about the same for great forces. I have no explanation of this to offer.

The ring was heated until the resistance of the secondary coil was about 2·4, corresponding to a temperature of 529° C. Currents successively diminishing in amount were then passed through the primary, each current being reversed ten times. The last currents gave magnetising forces 1·2, 0·6, 0·3, 0·15, 0·075, and 0·05, the intention being to demagnetise the sample. The ring was allowed to cool till the resistance of secondary was 2·0, corresponding to a temperature of 398° C. The following series of observations was made: the first kick was in all cases produced by a reversal of current from the direction of the last demagnetising current; the second kick by a reversal in the opposite sense.

TABLE 14.

Magnetising force.	Galvanometer kick.	Resistance in circuit.
0·075	20·5 13·5	12·43
0·15	41·5 32·5	"
0·3	104·0 81·0	"
0·6	284·5 241·0	"
1·2	143·5 150·0	102·43
2·1	262·5 265·0	"
4·0	351·0 351·0	"
7·3	210·0 211·5	202·43
12·1	235·5 234·0	"
43·4	272·5 271·5	"

The resistance of the secondary coil at the end of the experiment was 2·05; temperature, 415° C.

The sample was again heated until it became non-magnetic, and then allowed to cool very slowly, and the following series of observations were made, the ring being demagnetised as before after each series. The actual kicks of the galvanometer are given, as they illustrate further the point last mentioned. In the first two series only one kick was taken, to save time.

TABLE 15.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.	Resistance of coil.	Temperature.
					° C.
				3·025	733
0·075	64·5	3·455	61		
0·15	287·0	3·454	273		
0·3	244·0	13·453	903		
0·6	199·0	23·452	1286		
1·2	241·0	23·451	1554		
2·0	290·0	23·450	1870		
				3·019	731

TABLE 16.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.	Resistance of coil.	Temperature.
					° C.
				3·018	730
0·075	133	13·448	492		
0·15	305	13·448	1128		
0·3	302	23·448	1948		
0·6	91	103·449	2584		
1·2	95	103·449	2698		
37·4	137	103·449	2891		
				3·019	731

TABLE 17.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.	Resistance of coil.	Temperature.
					° C.
				3·018	730
0·075	214	13·448	792		
0·075	149	13·447	551		
0·075	145	13·445	536		
0·6	102	103·444	2897		
38·4	150	103·442	4260		
				3·012	729

Table 18.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.	Resistance of coil.	Temperature.
					° C.
				3·01	728
0·075	229	13·44	847		
0·075	155	13·44	573		
0·3	89	103·44	2528		
0·075	154	13·43	570		
0·3	96	103·43	2726		
1·2	132	103·43	3749		
7·3	156	103·43	4430		
37·2	181	103·43	5155		
				3·0	725

The sample was again heated until it became non-magnetic. A magnetising force of 0·075 was applied by a current in the primary during heating, and was taken off entirely by breaking the primary circuit when the sample was non-magnetic. The sample was allowed to cool to the ordinary temperature of the room, 12° C., and the following series of observations was made, the first reversal being from the direction of the force of 0·075 which had been applied when the ring was heated.

Table 19.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0·075	120	1·244	41
"	87	"	30
0·15	249	"	85
"	210	"	72
0·3	62	11·244	193
"	58	"	179
0·6	178	"	550
"	154	"	476
1·2	59	101·244	1,590
"	55	"	
2·2	227	"	6,300
"	223	"	
4·0	357	"	10,080
"	363	"	
7·3	226	201·24	12,553
"	228	"	
12·1	252	"	13,991
"	254	"	
18·8	268	"	14,876
"	270	"	
25·9	275	"	15,318
"	278	"	
42·4	293	"	16,148
"	291	"	

In addition to the fact that the first kick is largest for small forces, this shows, I think, that heating a sample above the critical temperature does not destroy its remembrance of magnetic force applied before and during heating. It would seem that the molecules of iron lie as they were placed by the magnetising force even after their magnetisation has disappeared by heating, and that when they become again capable of magnetisation by cooling the effect of the position of their axes is again apparent.

The ring was now demagnetised by reversed currents, but these were successively reduced to a force of 0·0075, instead of 0·05 as heretofore, and the following series of observations was made :—

TABLE 20.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0·075	77·0	1·24	27
„	79·0		
0·15	180·0	„	62
„	183·0		
0·3	52·0	11·24	161
„	52·5		
0·6	126·0	„	389
„	125 0		
1·2	47·5	101·24	1,314
„	47·0		
2·1	222·0	„	6,172
„	223·0		
4·0	361·0	„	10,119
„	366·0		
7·5	228·0	201·24	12,636
„	228·0		
12·3	253·0	„	13,991
„	252·0		
18·8	270·0	„	14,903
„	269·0		
25·1	276·5	„	15,277
„	276·0		
42·2	291·0	„	16,037
„	289·5		

This series shows two things : first, when the demagnetising force is taken low enough there is no asymmetry in the galvanometer kicks ; second, the effect of demagnetising by reverse currents is to reduce the amount of induction for low forces.

The ring was now heated to a resistance of secondary of 3·18, temperature 783° C., the ring becoming non-magnetic at 3·03, temperature 734° C. or thereabouts, a magnetising force of about 12 C.G.S. units being constantly applied. The magnetising force was then taken off and the ring was allowed to cool, and the following series was made ; the first kick being in all cases produced by reversal from the direction of the current applied during heating.

TABLE 21.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0·15	28·5	11·26	87
"	28·0		
"	218·0	1·26	75
"	214·0		
0·3	66·5	11·26	205
"	66·0		
0·6	182·0	"	565
"	167·0	"	518
"	162·0	"	502
"	157·0	"	488
1·2	329·0	21·26	1,925
"	293·0	"	1,714
2·2	230·0	101·26	6,398
"	227·0		
4·0	181·0	201·26	10,000
"	179·0		
7·3	225·0	"	12,410
"	223·0		
11·6	250·0	"	13,850
"	249·0		
18·0	264·0	"	14,626
"	263·0		
28·3	274·0	"	15,180
"	274·0		
46·2	288·0	"	15,900
"	286·0		

From this table it will be observed that the induction for low forces has again increased ; that the ring still recollects its state previous to heating.

The ring was again demagnetised, with currents ranging down to 0·0075, and the following series of experiments was made :—

TABLE 22.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0·075	74·5	1·26	26
,,	76·5		
0·15	175·0	,,	62
,,	180·0		
0·3	51·5	11·26	161
,,	52·5		
0·6	125·0	,,	389
,,	125·0		
1·2	231·0	21·26	1,331
,,	224·0		
2·2	223·0	101·26	6,272
,,	224·0		
4·0	361·0	,,	10,192
,,	365·0		
7·7	224·0	201·26	12,576
,,	229·0		
13·1	252·0	,,	14,016
,,	254·0		
20·4	266·0	,,	14,847
,,	269·0		
28·8	277·0	,,	15,346
,,	276·0		
51·7	292·0	,,	16,455
,,	292·0		

It will be seen that this series agrees very closely with Table 20, evidence of the general accuracy of the results.

The ring was lastly demagnetised and heated to a resistance of secondary of 3·19, temperature 787° C., under a magnetising force ·075, which was removed when the ring was at its highest temperature ; the ring was cooled, and the following observations made. In this case, however, the first kick was due to a reversal from a current opposed to the current which was applied during heating.

TABLE 23.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0·075	84·0	1·43	33
,,	84·5		
0·15	192·0	1·43	75
,,	195·0		
0·3	60·0	11·43	192
,,	62·0		
0·6	153·0	,,	480
,,	154·0		
1·2	321·0	21·43	1,891
,,	? 302·5		
2·2	239·0	101·43	6,678
,,	238·0		
4·0	367·0	,,	10,262
,,	366·0		
7·3	227·0	201·43	12,576
,,	226·0		

This shows doubtfully the effects of magnetisation previous to heating, but, comparing it with Table 10, it completes the proof that the asymmetry was in that case due to the magnetising force, which had been stopped when the ring was non-magnetic.

I have dwelt at length on these experiments because they show two things: first, that heating until the ring becomes non-magnetic does not clear the material of the magnetism when it is afterwards cooled; second, that demagnetisation by reversal does not bring back the material to its virgin state, but leaves it in a state in which the induction is much less for small forces and greater for medium forces than a perfectly demagnetised ring would show.

To return to the effects of temperature, Curves XX., XXI., and XXII. show the relation of permeability to temperature for magnetising forces 0·3, 4, and 30.

It will be seen that they present the same general characteristics as the curves for wrought iron. The irregularities are due in part, no doubt, to the dependence of the observations on previous operations on the iron; in part, to uncertainty concerning the exact agreement of temperature of iron and temperature of secondary coil.

Whitworth's Hard Steel.—This sample was supplied to me with the following analysis of its composition:—

	C	Mn	S	Si	P
Per cent.	962	·212	·017	·164	·016

The dimensions of the ring were exactly the same as the mild steel.

The secondary coil had 56, the primary 101, convolutions.

The resistance of the secondary and leads was ·732 at 8° C.

Experiments were first made with the ring cold, partly to show the changes caused by annealing, and partly to examine the behaviour of the virgin steel.

The first series given in Table 24 was made on the virgin steel. The actual elongations on the galvanometer are given, as they afford a better idea of the probable errors of observation. These show that for very small forces the first and second elongations are practically equal, but that for forces between 1 C.G.S. unit and 14 C.G.S. units the first elongation is very materially greater than the later elongations.

The ring was now demagnetised, with magnetising forces ranging down to 0·0045, and the experiment was repeated, the results being shown in Table 25. Comparing them with Table 24, we see that the effect has been to reduce the inductions for low forces, as was the case with mild steel, and to render the kicks practically equal, whether they arise from the current first applied or subsequently applied.

The ring was not now demagnetised; the last current, giving a magnetising force 35·36, was removed, but not reversed, and a series of experiments made, the first reversal in each case being from the direction of the current of 35·36 last applied. The results are given in Table 26.

TABLES 24–26.

Table 24, Curves XXIII. and XXIV.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0·065	27·0, 28·0	1·164	9
0·13	57·5, 57·5	,,	18
0·26	116·0, 117·5	,,	37
0·52	234·0, 236·0	,,	75
1·04	56·5, 55·5	11·164	172
2·08	123·5, 117·5	,,	379, 361
	116·0, 116·5	,,	356
3·74	302·0	,,	927
	276·0	,,	847
	270·0	,,	829
	262·0	,,	804
	261·5, 261·5	,,	802
	258·5, 257·0	,,	792
6·66	93·5, 89·5	101·16	2,543
	87·0, 85·0	,,	2,391
	85·5, 83·5	,,	2,349
10·61	250·5, 247·0	,,	6,922
	234·5, 226·0	,,	6,394
	225·0, 230·0	,,	6,338
15·18	168·0, 171·0	201·16	9,346
	173·0, 169·0	,,	9,456
20·28	190·0, 197·0, 194·0, 193·0	,,	10,728
35·88	226·0, 228·0, 227·0, 227·0	,,	12,553

Table 25, Curve XXV.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0·065	26·5, 26·0	1·164	8
0·13	55·0, 53·5	,,	17
0·26	106·0, 106·0	,,	34
0·52	213·0, 213·0	,,	68
1·04	51·5, 51·5	11·164	158
2·08	108·0, 105·0	,,	328
3·74	241·0, 240·0	,,	740
6·66	80·0, 78·0	101·164	2,196
10·82	223·0, 226·0	,,	6,227
15·18	163·0, 164·0	201·164	9,069
21·0	193·0, 197·0	,,	10,783
35·36	226·0, 226·0	,,	12,498

Table 26.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0·065	25·0, 15·0	1·164	8
0·26	111·5	,,	36
	57·0	,,	18
	59·5	,,	19
	57·5	,,	18
3·95	311·5	11·164	956
	140·5	,,	431
	144·0, 145·0	,,	445
	136·0, 132·0	,,	411
	135·0, 134·0	,,	414
11·44	290·0	101·16	8,062
	257·0	,,	7,145
	254·0, 251·0	,,	7,033
	252·0, 250·0	,,	6,950
16·43	175·0, 173·0	201·16	9,622
	172·0, 172·0	,,	9,512

The ring was now thoroughly demagnetised and heated till it became non-magnetic. It was then cooled slowly, and the following observations were made :—

TABLES 27–33.

	Table 27.	Table 28, Curve XXVIII.	Table 29, Curve XXIX.	Table 30, Curve XXX.
Resistance at beginning of experiment	2·805	2·795	2·77	2·74
Temperature at beginning of experiment	687° C.	682° C.	674° C.	664° C.
Resistance at end of experiment	2·795	2·77	2·74	2·72
Temperature at end of experiment	682° C.	674° C.	664° C.	657° C.

Table 27. Magnetising force.	Table 27. Induction per sq. cm.	Table 28. Magnetising force.	Table 28. Induction per sq. cm.	Table 29. Magnetising force.	Table 29. Induction per sq. cm.	Table 30. Magnetising force.	Table 30. Induction per sq. cm.
0·065	9	0·065	24	0·065	45	0·065	43
0·13	21	0·13	53	0·26	197	0·26	184
0·26	61	0·26	123	1·04	873	1·04	..
		0·52	291	3·22	3578	..	1087
		1·04	821	8·32	4629	3·32	3621
		2·08	1595	18·2	5396	8·32	4771
		3·33	2215			18·51	5652
		5·51	2868				
		8·32	3301				

	Table 31, Curve XXXI.	Table 32, Curve XXXII.	Table 33, Curve XXXIII.
Resistance at beginning of experiment	2·72	2·43	2·35
Temperature at beginning of experiment	657° C.	561° C.	534° C.
Resistance at end of experiment	2·73	2·35	2·28
Temperature at end of experiment	661° C.	534° C.	511° C.

Table 31. Magnetising force.	Table 31. Induction per sq. cm.	Table 32. Magnetising force.	Table 32. Induction per sq. cm.	Table 33. Magnetising force.	Table 33. Induction per sq. cm.
0·065	42	0·065	27	0·065	24
0·26	171	0·26	112	0·26	103
1·04	1010	1·04	539	1·04	516
3·22	3706	3·22	..	2·08	1406
..	..	..	3396	3·43	3243
8·32	4885	8·53	5377	8·53	5414
19·8	5708	21·2	6707	21·53	6768

When cold, the resistance of the secondary coil and leads was 0·768 ; in calculating the temperatures, it is assumed that the cold resistance is 0·768. It is obvious that there is here considerable uncertainty concerning the actual temperatures, owing to the changes in the condition of the wire due to its oxidation.

The following series was next made, the mean results being given in

TABLE 34, Curve XXVI.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0·065	29	1·198	10
0·13	58	1·198	19
0·26	120	1·198	40
0·52	251	1·198	83
1·04	66	11·198	203
3·74	170	21·198	991
6·03	159	101·2	4,420
9·78	283	,,	7,867
13·94	176	201·2	9,733
15·81	187	,,	10,341
22·67	211	,,	11,668

The ring was now demagnetised, and another series of determinations was made, the mean results being given in

TABLE 35, Curve XXVII.

Magnetising force.	Galvanometer kick.	Resistance in circuit.	Induction per sq. cm.
0 065	26	1·198	9
0·13	54	,,	18
0·26	111	,,	37
0·52	236	,,	78
1·04	60	11·198	185
2·08	132	,,	407
3·74	327	,,	1,007
6·24	130	101·2	3,614
9·78	265	,,	7,367
13·10	168	201·2	9,290
15·7	187	,,	10,341
22·67	211	,,	11,668

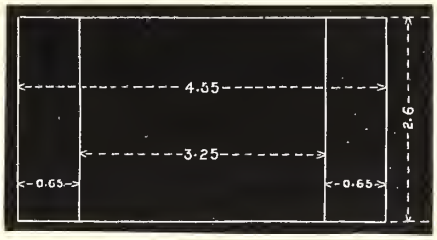
Comparing Curves XXV. and XXVII., we see the effect of annealing the iron to be to increase its permeability. Comparing Curves XXVI. and XXVII. we see the effect of demagnetising by reversed currents. Curve XXXIV. shows the relation of permeability to temperature for a force of 1·5.

Manganese Steel.—The sample of this steel was given to me by Mr. HADFIELD, who also supplied me with the following two analyses of the sample :—

	Per cent.	Per cent.
C	·74	·73
Si	·50	·55
S	·05	·06
P	·08	·09
Mn	11·15	12·06

It is well known that this steel at ordinary temperatures, and for both great and small magnetising forces, is but very slightly magnetic. The object of these experiments was to ascertain whether it became magnetic at any higher temperature.

The dimensions of the ring were as shown in the accompanying section :—



Thus the mean area of section is 1·7 sq. cm., and the mean length of lines of magnetic force 12·3 cms. The ring was wound with 52 convolutions for the secondary and 76 convolutions for the primary. It was not possible to accurately estimate the mean area of the secondary; it is, however, assumed to exceed the mean area of the steel by as much as the secondary of the sample of wrought iron is estimated to exceed the area of that sample; this gives an area of 2·38 sq. cms.

A preliminary experiment at the ordinary temperature gave induction 67·7; magnetising force 26·9.

The induction in the airspace between the wire and steel will be $26·9 \times 0·68 = 18·3$; deducting this from 67·7, we obtain the induction in the steel equal to 49·4, or 29·0 per sq. cm.; dividing this by 26·9, we obtain 1·08 as the permeability from this experiment.

After the ring had been heated to a high temperature, about 800° C., and had been allowed to cool, a second experiment gave total induction 76, magnetising force 22·8, permeability 1·5.

The ring was again heated and allowed to cool, observations being made both during rise and fall of temperature, with the following results :—

TABLE 36.

Resistance of secondary and leads.	Temperature.	Total induction.	Permeability.
	° C.		
0·77	9·0 (room)	67·7	1·08
2·20	476·0	93·1	1·95
3·00	757·0	101·7	2·19
3·23	816·0	71·7	1·45
3·30	841·0	72·0	1·42
3·14	787·0	72·0	1·38
2·80	674·0	92·3	1·99
0·79	8·8 (room)	94·5	1·99

As the changes in the temperature were in this case made somewhat rapidly, the temperature of the ring lags behind the temperature of the copper.

These show: first, that at no temperature does this steel become at all strongly magnetic; second, that at a temperature of a little over 750° C. there is a substantial reduction of permeability; third, that above this temperature the substance remains slightly magnetic; fourth, that annealing somewhat increases the permeability of the material.

Resistance of Iron at High Temperatures.

These experiments were made in a perfectly simple way. Coils of very soft iron wire, pianoforte wire, manganese steel wire, and copper wire were insulated with asbestos, were bound together with copper wire so placed as to tend by its conductivity for heat to bring them to the same temperature, and were placed in an iron cylindrical box for heating in a furnace. They were heated with a slowly rising temperature, and the resistance of the wires was successively observed, and the time of each observation noted. By interpolation the resistance of any sample at any time intermediate between the actual observations could be very approximately determined. The points shown in Curves XXXV., XXXVI., XXXVII., were thus determined. In these curves the abscissæ represent the temperatures, and the ordinates the resistance of a wire having unit resistance at 0° C. Curve XXXVII. is manganese steel, which exhibits a fairly constant temperature coefficient of 0·00119; Dr. FLEMING gives 0·0012 as the temperature coefficient of this material. Curve XXXV. is soft iron; at 0° C. the coefficient is 0·0056; the coefficient gradually increases with rise of temperature to 0·019, a little below 855° C.; at 855° C. the coefficient suddenly, or at all events very rapidly, changes to 0·007. Curve XXXVI. is pianoforte wire; at 0° C. the coefficient is 0·0035; the coefficient increases with rise of temperature to 0·016, a little below 812° C.; at 812° C. the coefficient suddenly changes to 0·005. The actual values of the coefficients above the points of change must be regarded as somewhat uncertain, because the range of temperature is small, and because the accuracy of the results may be affected by the possible oxidation of the copper. The temperatures of change of coefficient, 855° C. and 812° C., are higher than any critical temperature I had observed. It was necessary to determine the critical temperatures for magnetisation for the particular samples. A ring was formed of the respective wires, and was wound with a primary and secondary coil, and the critical temperature was determined as in the preceding magnetic experiments: it was found to be for the soft iron 880° C., for the hard pianoforte wire 838° C. These temperatures agree with the temperatures of sudden change of resistance coefficient within the limits of errors of observation.*

Some interesting observations were made on the permanent change in the resistance at ordinary temperatures caused in the wires by heating to a high temperature. In the following table are given the actual resistances of wires at the temperature of the room:—

	Before heating.	After first heating.	Second heating.	Third heating.
Soft iron	0·629	0·624	0·72	0·735
Pianoforte wire	0·851	0·794	0·79	0·74
Manganese steel . . .	1·744	1·656	1·61	1·61

In a second experiment the resistances before heating were: soft iron 0·614, pianoforte wire 0·826; after heating, soft wire 0·643, pianoforte wire 0·72.

The effects are opposite in the cases of soft iron and pianoforte wire.

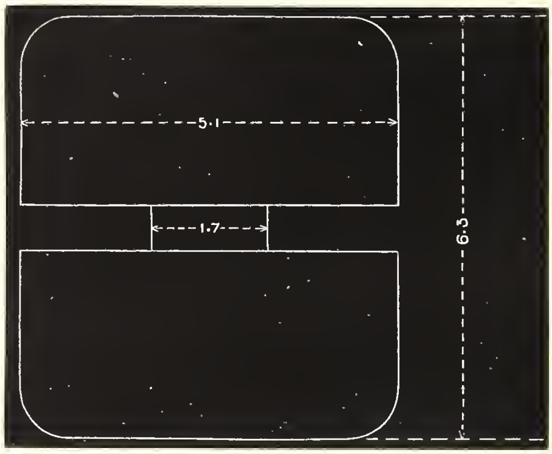
Recalescence of Iron.

Professor BARRETT has observed that, if an iron wire be heated to a bright redness and then allowed to cool, this cooling does not go on continuously, but after the wire has sunk to a very dull red it suddenly becomes brighter and then continues to cool down. He surmised that the temperature at which this occurs is the temperature at which the iron ceases to be magnetisable. In repeating Professor BARRETT's experiments, I found no difficulty in obtaining the phenomenon with hard steel wire, but I failed to observe it in the case of soft iron wire, or in the case of manganese steel wire. Although other explanations of the phenomenon have been offered, there can never, I think, have been much doubt that it was due to the liberation of heat owing

* [*Note added July* 2, 1889.—Sir JOSEPH WHITWORTH and Co. have kindly analysed these two wires for me, with the following results:—

	C	Mn	S	Si	P
Soft iron wire	·006	·289	·015	·034	·141 per cent.
Pianoforte wire . . .	·724	·157	·010	·132	·030 ,,]

to some change in the material, and not due to any change in the conductivity or emissive power. This has indeed been satisfactorily proved by Mr. NEWALL.* My method of experiment was exceedingly simple. I took a cylinder of hard steel 6·3 cms. long and 5·1 cms. in diameter, cut a groove in it, and wrapped in the groove a copper wire insulated with asbestos.



The cylinder was wrapped in a large number of coverings of asbestos paper to retard its cooling; the whole was then heated to a bright redness in a gas furnace; was taken from the furnace and allowed to cool in the open air, the resistance of the copper wire being, from time to time, observed. The result is plotted in Curve XXXVIII., in which the ordinates are the logarithms of the increments of resistance above the resistance at the temperature of the room, and the abscissæ are the times. If the specific heat of the material were constant, and the rate of loss of heat were proportional to the excess of temperature, the curve would be a straight line. It will be observed that below a certain point this is very nearly the case, but that there is a remarkable wave in the curve. The temperature was observed to be falling rapidly, then to be suddenly retarded, next to increase, then again to fall. The temperature reached in the first descent was 680° C. The temperature to which the iron subsequently ascends is 712° C. The temperature at which another sample of hard steel ceased to be magnetic, determined in the same way by the resistance of a copper coil, was found to be 690° C. This shows that, within the limits of errors of observation, the temperature of recalescence is that at which the material ceases to be magnetic. This curve gives the material for determining the quantity of heat liberated. The dotted lines in the curve show the continuation of the first and second parts of the curve; the horizontal distance between these approximately represents the time during which the material was giving out heat without fall of temperature. After the bend in the curve, the temperature is falling at the rate of 0·21° C. per second. The

* 'Phil. Mag.,' June, 1888.

distance between the two straight parts of the curve is 810 seconds. It follows that the heat liberated in recalescence of this sample is 173 times the heat liberated when the iron falls in temperature 1° C. With the same sample, I have also observed an ascending curve of temperature. There is, in this case, no reduction of temperature at the point of recalescence, but there is a very substantial reduction in the rate at which the temperature rises.*

A similar experiment was made with a sample of wrought iron substantially the same as the wrought iron ring first experimented upon. The result is shown in Curve XXXIX. It will be seen that there is a great pause in the descent of this curve at a temperature of 820° C., but that the curve does not sensibly rise. This shows why soft iron apparently does not recalesce. Determining the heat liberated in the same way as before, we find the temperature falling after the bend in the curve at the rate of 0°·217 C. per second. The distance between the two straight parts is 960 seconds. Hence, heat liberated in recalescence is 208 times the heat liberated when the iron falls 1° C. in temperature. The temperature at which a sample ordered at the same time and place ceased to be magnetic was 780° C. Comparing this result with that for hard steel, we see that the quantity of heat liberated is substantially the same, but that in the case of the soft iron there is no material rise of temperature.†

[* *Note added 2nd July*, 1889.—Some remarks of Mr. TOMLINSON's suggested that it might be possible that there would be no recalescence if the iron were heated but little above the critical point. To test this, I repeated the experiment, heating the sample to 765° C., very little above the critical point. Curve XXXVIIIA. shows the result. From this it will be seen that the phenomenon is substantially the same whether the sample is heated to 988° C. or to 765° C.]

[† *Note added 2nd July*, 1889.—In order to complete the proof of the connexion of recalescence and the disappearance of magnetism, a block of manganese steel was tried in exactly the same way as the blocks of hard steel and of iron. The result is shown in Curve XL., from which it will be seen there is no more bend in the curve than would be accounted for by the presence of a small quantity of magnetic iron, such a quantity as one would expect from the magnetic results, supposing the true alloy of manganese and iron to be absolutely non-magnetic.]

XV. *The Diurnal Variation of Terrestrial Magnetism.*

By Arthur Schuster, *F.R.S., Professor of Physics in Owens College. With an Appendix by* H. Lamb, *F.R.S., Professor of Mathematics in Owens College.*

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I. *Introduction.*

In the year 1839 Gauss published his celebrated Memoir on Terrestrial Magnetism, in which the potential on the Earth's surface was calculated to 26 terms of a series of surface harmonics. It was proved in this Memoir that, if the horizontal components of magnetic force were known all over the Earth, the surface potential could be derived without the help of the vertical forces, and it is well known now how these latter can be used to separate the terms of the potential which depend on internal from those which depend on external sources. Nevertheless Gauss made use of the vertical forces in his calculations of the surface potential in order to ensure a greater degree of accuracy. He assumed for this purpose that magnetic matter was distributed through the interior of the Earth, and mentions the fair agreement between calculated and observed facts as a justification of his assumption. In the latter part of the Memoir it was suggested that the same method should be employed in the investigation of the regular and secular variations.

The use of harmonic analysis to separate internal from external causes has never been put to a practical test, but it seems to me to be especially well adapted to enquiries on the causes of the periodic oscillations of the magnetic needle.

If the magnetic effects can be fairly represented by a single term in the series of harmonics as far as the horizontal forces are concerned, there should be no doubt as to the location of the disturbing cause, for the vertical force should be in the opposite direction if the origin is outside from what it should be if the origin is inside the Earth. As the expression for the potential contains in one case the distance from the Earth's centre in the numerator, in the other case in the denominator, and as the vertical force depends on the differential coefficient with regard to distance from the Earth's centre, each single term in the series is of opposite sign according to the location of the cause; but what is true for each single term need not be true for the sum of the series. By a curious combination of terms the vertical forces might possibly be of the same sign, on whichever of the two hypotheses it is calculated. In any case, however, the differences between the two results will be of the same order of magnitude as the vertical force itself. If it is then a question simply of deciding whether the cause is outside or inside, without taking into account a possible combination of both causes, the result should not be doubtful, even if we have only an approximate knowledge of the vertical forces.

Two years ago I showed that the leading features of the horizontal components for diurnal variation could be approximately represented by the surface harmonic of the second degree and first type, and that the vertical variation agreed in direction and phase with the calculation on the assumption that the seat of the force is outside the Earth. The agreement seemed to me to be sufficiently good to justify the conclusion that the greater part of the variation is due to causes outside the Earth's surface. Nevertheless, it seemed advisable to enter more fully into the matter, as in the first approximate treatment of the subject a number of important questions had to be left untouched. I now publish the results of an investigation which has been carried as far as the observations at my disposal have allowed me to do. My original conclusions have been fully confirmed, and some further information has been obtained which I believe to be of importance. The results of the calculation point not only to an external source, but to an additional internal source, standing in fixed relationship to the external cause. This we might have expected. A varying potential due to external causes must be accompanied by currents induced in the Earth's body, which, in turn, must affect the magnetic needle. The phase of these currents and their magnitude lead us to form definite conclusions on the average conducting power of the Earth, and it will be seen that there is strong evidence that the average conductivity is very small near the surface, but must be greater further down. In this part of the investigation I had much assistance from my colleague, Professor LAMB.

I hope that the results obtained in this paper may induce the heads of magnetic observatories to consider the suggestions which I have made at the end of it, as their adoption would very materially assist further investigations.

I had, in the first place, to fix on a year for which we possess as complete magnetic records as possible. The phase of the variation of horizontal force changes sign in a latitude not far removed from that of Lisbon, and it seemed to me, therefore, essential that the excellent observations there made by Sen. JOS. CAPELLO should be made use of. The observations are published as far as 1872, and I had to take a year therefore anterior to this. It seemed also desirable to make use of the St. Petersburg observations, as it is the most northerly station for which we have records extending over a period of years, and as Mr. H. WILD's well-known skill gives special value to the observations made under his direction. The obseravtions (continued since 1878 in Paulowsk) were interrupted in 1871 and 1872, and we have to go back, therefore, as far as 1870 if we want to utilise the St. Petersburg and Lisbon observations simultaneously. As far as the horizontal components are concerned, we also possess good records of 1870 at Greenwich and Bombay. Four stations are sufficient to find to the necessary accuracy the potential on the surface of the Earth, but it would be of advantage if in future similar investigations a greater number of stations could be utilised.

The observations are published in very different form by the various observatories. The units of force at Bombay and St. Petersburg are the Gaussian unit millimeter-milligram-second. At Lisbon it is the foot-grain-second. At Greenwich the variations are given in terms of the whole vertical and horizontal force. At Bombay, moreover, the observations are given not for a certain hour, but for a time which varies with different instruments between 12 and 19 minutes past each hour. For this there was some reason originally, but at present it would be far better if Bombay would adopt the practice of other observatories. The daily variations had, in the first place, to be all reduced to C.G.S. units, and, further, instead of variations in declination and horizontal force, we had to find the components of the periodic force towards the geographical North and West. I am much indebted to Mr. Wm. Ellis, of the Greenwich Observatory, for the help he has given me in the reduction of the observations to a form which I could use in my calculations. A good many of the computations were done under his direct superintendence, and much time and trouble was saved me in consequence.

The daily variation of declination and horizontal force was expressed in the form

$$a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t + a_3 \cos 3t + b_3 \sin 3t + a_4 \cos 4t + b_4 \cos 4t,$$

where t represents astronomical time. The summer months, April to September, were treated separately from the winter months, October to March. The unit of force, for convenience's sake, was taken as 10^{-6} C.G.S.

Tables I. and II. give the coefficients which were calculated according to a well-known method from the original observations.

Tables III. and IV. give the same coefficients reduced to forces directed to the geographical North and West, instead of to the magnetic North and West.

Before showing how, with the help of these coefficients, the surface potential can be calculated, I must deduce a few formulæ which will be used hereafter.

Table I.—Force to Magnetic West.

Coefficients in the expansion

$$a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t + a_3 \cos 3t + b_3 \sin 3t + a_4 \cos 4t + b_4 \sin 4t.$$

The unit of force is 1 C.G.S. $\times$ 10^{-6}; t being astronomical time.

	Bombay.		Lisbon.		Greenwich.		St. Petersburg.	
	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.
a_1	+ 64·3	+ 6·3	+ 125·5	+ 121·5	+ 168·0	+ 154·5	+ 109·0	+ 95·7
b_1	+ 116·5	+ 33·2	+ 213·7	+ 132·6	+ 192·3	+ 113·5	+ 219·4	+ 96·7
a_2	+ 134·3	+ 6·5	+ 126·1	+ 53·6	+ 129·1	+ 32·7	+ 104·2	− 12·8
b_2	+ 59·5	+ 23·2	+ 152·2	+ 111·1	+ 114·6	+ 84·0	+ 113·2	+ 69·6
a_3	+ 103·5	+ 40·4	+ 75·1	+ 51·0	+ 65·8	+ 38·4	+ 37·9	+ 21·8
b_3	+ 7·1	+ 7·6	+ 56·2	+ 51·0	+ 42·5	+ 30·6	+ 59·2	+ 28·9
a_4	+ 15·5	+ 33·1	+ 13·1	+ 30·7	+ 9·3	+ 21·3	+ 2·8	+ 6·2
b_4	+ 18·7	− 9·6	− 5·9	+ 20·3	+ 2·6	+ 17·6	+ 10·9	+ 13·7

TABLE II.—Force to Magnetic North.

Coefficients in the expansion

$$a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t + a_3 \cos 3t + b_3 \sin 3t + a_4 \cos 4t + b_4 \sin 4t.$$

The unit of force is 1 C.G.S. $\times 10^{-6}$; t being astronomical time.

	Bombay.		Lisbon.		Greenwich.		St. Petersburg.	
	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.
a_1	+ 386·9	+ 332·5	− 124·2	− 78·6	− 213·7	− 117·4	− 257·9	− 111·8
b_1	+ 5·7	+ 38·2	+ 71·5	− 15·0	+ 154·0	+ 22·8	+ 150·2	− 5·8
a_2	+ 152·7	+ 125·0	− 28·3	− 54·8	− 112·6	− 87·5	− 149·1	− 69·0
b_2	− 11·8	+ 7·3	+ 43·0	− 0·9	+ 78·9	+ 24·6	+ 45·7	− 12·8
a_3	+ 41·1	+ 39·6	+ 23·1	− 2·3	− 2·3	− 16·4	− 15·8	− 18·4
b_3	− 41·8	− 31·4	+ 19·0	+ 19·0	+ 27·1	+ 27·1	+ 39·7	+ 22·9
a_4	− 4·3	+ 15·3	+ 12·2	+ 6·9	+ 12·1	+ 2·0	− 9·0	− 3·8
b_4	− 13·7	− 21·1	− 12·3	+ 18·8	+ 8·4	+ 16·6	+ 3·3	+ 6·4

TABLE III.—Force to Geographical West.

Coefficients in the expansion

$$a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t + a_3 \cos 3t + b_3 \sin 3t + a_4 \cos 4t + b_4 \sin 4t.$$

The unit of force is 1 C.G.S. $\times 10^{-6}$; t being astronomical time.

	Bombay.		Lisbon.		Greenwich.		St. Petersburg.	
	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.
a_1	+ 57·5	+ 0·4	+ 74·3	+ 86·5	+ 85·4	+ 105·4	+ 99·6	+ 91·6
b_1	+ 116·4	+ 32·5	+ 225·3	+ 119·1	+ 233·3	+ 114·6	+ 224·6	+ 96·4
a_2	+ 131·6	+ 4·3	+ 108·3	+ 31·1	+ 83·1	+ 1·0	+ 98·7	− 15·3
b_2	+ 59·7	+ 23·1	+ 157·7	+ 103·8	+ 134·6	+ 87·4	+ 114·8	+ 69·1
a_3	+ 102·8	+ 39·7	+ 78·5	+ 47·0	+ 61·1	+ 30·5	+ 37·3	+ 21·2
b_3	+ 7·8	+ 8·1	+ 59·3	+ 54·4	+ 49·2	+ 38·0	+ 60·6	+ 29·7
a_4	+ 15·6	+ 32·8	+ 16·6	+ 31·2	+ 12·9	+ 20·7	+ 2·5	+ 6·1
b_4	− 18·5	− 9·2	− 9·8	+ 25·6	+ 5·3	+ 22·2	+ 11·0	+ 13·9

TABLE IV.—Force to Geographical North.

Coefficients in the expansion

$a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t + a_3 \cos 3t + b_3 \sin 3t + a_4 \cos 4t + b_4 \sin 4t.$

The unit of force is 1 C.G.S. $\times 10^{-6}$; t being astronomical time.

	Bombay.		Lisbon.		Greenwich.		St. Petersburg.	
	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.
a_1	+ 388·0	+ 332·4	− 160·2	− 116·1	− 258·1	− 162·9	− 261·6	− 115·2
b_1	+ 7·8	+ 38·8	− 7·6	− 60·4	+ 79·5	− 17·2	+ 142·2	− 9·3
a_2	+ 155·1	+ 125·1	− 70·5	− 70·1	− 149·8	− 93·4	− 152·8	− 68·5
b_2	− 10·8	+ 7·7	− 12·8	− 39·6	+ 35·2	− 5·5	+ 41·6	− 15·3
a_3	+ 42·9	+ 40·3	− 4·5	− 20·0	− 24·6	− 28·5	− 17·2	− 19·2
b_3	− 41·7	− 31·3	− 1·8	0·0	+ 11·0	+ 15·1	+ 37·6	+ 21·9
a_4	− 4·0	+ 15·9	+ 6·8	− 4·2	+ 8·2	− 5·3	− 9·1	− 4·0
b_4	− 14·0	− 21·3	− 9·5	+ 10·5	+ 7·0	+ 9·6	+ 2·9	+ 5·9

II. *Some Formulæ useful in the Analysis by Spherical Harmonics.*

In the expansion of mathematical expressions into spherical harmonics, it will often occur that we have to express powers of the cosine of an angle, or a cosine of some multiple of an angle, in terms of the differential coefficients of zonal harmonics with respect to the cosine of the argument. The necessary equations for the powers of cosines can be easily obtained by differentiation of the well-known formulæ, giving the powers of a cosine in terms of zonal harmonics. But I think it will be useful here to give the general equations which I have had to use in expressing $\cos mu$ in terms of $d^p P_i / d\mu^p$, where p is any given number, $\mu = \cos u$ and P_i the zonal harmonic of degree i.

We may start from the expression

$$\cos mu = A_m P_m + A_{m-2} P_{m-2} + \ldots A_i P_i + \ldots \quad (1),$$

where

$$A_i = -(2i+1) \frac{\{m-(i-2)\}\{m-(i-4)\}\ldots(m-2)\,m^2\,(m+2)\ldots\{m+(i-2)\}}{\{m-(i+1)\}\{m-(i-1)\}\ldots(m-1)(m+1)\ldots\{m+(i+1)\}},$$

if m and i be even, and

$$A_i = -(2i+1) \frac{\{m-(i-2)\}\{m-(i-4)\}\ldots(m-1)(m+1)\ldots\{m+(i-2)\}}{\{m-(i+1)\}\{m-(i-1)\}\ldots(m-2)(m+2)\ldots\{m+(i+1)\}},$$

if m and i be odd.

Both expressions are included in the general one

$$A_i = -(2i+1)\,m\frac{\{m-(i-2)\}\{m-(i-4)\}\ldots\{m+(i-2)\}}{\{m-(i+1)\}\{m-(i-1)\}\ldots\{m+(i+1)\}},$$

where successive factors of both numerator and denominator increase by 2.*

If in formula (1) we substitute for any term P_i

$$\frac{dP_{i+1}}{d\mu} - \frac{dP_{i-1}}{d\mu} = (2i+1)\,P_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

we obtain an expression for $\cos m\theta$ in terms of the first differential coefficients of the zonal harmonics; and, if in the formula so obtained we successively apply the transformation

$$\frac{d^pP_{i+1}}{d\mu^p} - \frac{d^pP_{i-1}}{d\mu^p} = (2i+1)\frac{d^{p-1}P_i}{d\mu^{p-1}},$$

we finally obtain the required expression in terms of any required differential coefficient.

I have obtained in this way the equation

$$(-1)^{p+1}\frac{\cos mu}{1.3.5\ldots(2p+1)} = A_{m+p}\,d^pP_{m+p} + A_{m+p-2}\,d^pP_{m+p-2} + \ldots A_i\,d^pP_i + \ldots \quad (3),$$

where d^pP_i is written shortly for $d^pP_i/d\mu^p$, and

$$A_i = (2i+1)\,m\frac{\{m-(i-p-2)\}\{m-(i-p-4)\}\ldots\{m+(i-p-2)\}}{\{m-(i+p+1)\}\{m-(i+p-1)\}\ldots\{m+(i+p+1)\}} \quad . \quad (4),$$

except for the last term of the series, which will be given presently (5). If $p=0$, this expression agrees with the one previously given, and, as I shall proceed to show, if it is true for any value p, it will also be true for the value $p+1$. Assume, then, the equation (3) to hold.

The relation

$$d^{p+1}P_{(i+1)} - d^{p+1}P_{(i-1)} = (2i+1)\,d^pP_i$$

shows that the factor multiplying $d^{p+1}P_{i+1}$ in the expression for $\cos m\theta$ will depend only on A_i and on $A_{(i+2)}$ in (3). If $B_{(i+1)}$ is this factor, we have

$$B_{(i+1)} = \frac{A_i}{2i+1} - \frac{A_{i+2}}{2i+5},$$

* In the very useful book on 'Spherical Harmonics,' by FERRERS, the factor m does not occur in the general expression for A_i (page 33); but, from the deduction of the formula, it is clear that the factor m must be taken twice when it occurs in the numerator, and not at all when it occurs in the denominator. In using the equation I was at first led into error by the ambiguity, and hence I believe the expression given above to be clearer.

or, by substituting A_i from (4) and A_{i-2} from the corresponding equation,

$$B_{i+1} = m \frac{\{m-(i-p-2)\}\{m-(i-p-4)\}\ldots\{m+(i-p-2)\}}{\{m-(i+p+1)\}\{m-(i-p-1)\}\ldots\{m+(i+p+1)\}} \left[1 - \frac{\{m-(i-p)\}\{m+(i-p)\}}{\{m-(i+p+3)\}\{m+(i+p+3)\}}\right].$$

The square bracket reduces to

$$\frac{(i-p)^2-(i+p+3)^2}{\{m-(i+p+3)\}\{m+(i+p+3)\}} = -\frac{(2i+3)(2p+3)}{\{m-(i+p+3)\}\{m+(i+p+3)\}},$$

so that

$$-(-1)^{p+1}\frac{\cos m\theta}{1.3.5\ldots(2p+3)} = A_{m+p+1}\, d^{p+1}P_{m+p+1} + \ldots A_{i+1}\, d^{p+1}P_{i+1}\ldots,$$

where

$$A_{i+1} = m(2i+3)\frac{\{m-(i-p-2)\}\{m-(i-p-4)\}\ldots\{m+(i-p-2)\}}{\{m-(i+p+3)\}\{m-(i+p+1)\}\ldots\{m+(i+p+3)\}},$$

which agrees with (4) if $p+1$ is written for p, and $i+1$ for i.

The end terms require special consideration.

As only the even or only the odd zonal harmonics enter into any one series, the difference between $m+p$ and i in the equation (3) must be even; hence, the numerators in the fractional expression of (4) consist always of even numbers, whatever values m, i, or p may have.

If $(m+p)$ be odd, the zonal harmonics will all be odd, and the last term of the series will depend on d^pP_{p+1} if p be even, and d^pP_p if p be odd, for the differential coefficients of the zonal harmonics of lower degree will vanish. The expression (4) in neither case gives the correct factor, and we must substitute for it in both cases

$$(2i+1)\frac{1}{\{m-(i+p+1)\}\{m-(i+p-1)\}\ldots\{m+(i+p+1)\}} \quad \ldots \quad (5).$$

If in (4) i is put equal to $(p+2)$, the first and last term will be equal to m; in that case the m is only taken once.

The expression (5) is easily proved, and shown to hold also if $(m+p)$ be even.

The first term of the series is included in the general expression (4).

I have deduced with the help of the equation (3) the following relations, which will be used in this paper:—

$$\cos u = \tfrac{1}{3}\frac{dP_2}{d\mu} \quad \ldots \quad (A),$$

$$\cos 2u = \tfrac{4}{15}\frac{dP_3}{d\mu} - \tfrac{9}{15}\frac{dP_1}{d\mu} \quad \ldots \quad (B),$$

$$\cos 3u = \tfrac{8}{35}\frac{dP_4}{d\mu} - \tfrac{3}{7}\frac{dP_2}{d\mu} \quad \ldots \quad (C),$$

$$\cos 4u = \tfrac{64}{315}\frac{dP_5}{d\mu} - \tfrac{16}{45}\frac{dP_3}{d\mu} + \tfrac{3}{35}\frac{dP_1}{d\mu} \quad \ldots \quad (D),$$

$$\cos 5u = \tfrac{128}{693}\frac{dP_6}{d\mu} - \tfrac{24}{77}\frac{dP_4}{d\mu} + \tfrac{5}{63}\frac{dP_2}{d\mu} \quad \ldots \quad (E),$$

$$\cos 6u = \tfrac{512}{3003}\frac{dP_7}{d\mu} - \tfrac{128}{455}\frac{dP_5}{d\mu} + \tfrac{4}{55}\frac{dP_3}{d\mu} + \tfrac{1}{105}\frac{dP_1}{d\mu} \quad \ldots \quad (F);$$

$$\cos u = \tfrac{1}{105}\frac{d^3P_4}{d\mu^3} \quad \ldots \quad (G),$$

$$\cos 2u = \tfrac{4}{945}\frac{d^3P_5}{d\mu^3} - \tfrac{7}{135}\frac{d^3P_3}{d\mu^3} \quad \ldots \quad (H),$$

$$\cos 3u = \tfrac{8}{3465}\frac{d^3P_6}{d\mu^3} - \tfrac{1}{55}\frac{d^3P_4}{d\mu^3} \quad \ldots \quad (I),$$

$$\cos 4u = \tfrac{64}{45045}\frac{d^3P_7}{d\mu^3} - \tfrac{16}{1755}\frac{d^3P_5}{d\mu^3} + \tfrac{7}{297}\frac{d^3P_3}{d\mu^3} \quad \ldots \quad (K),$$

$$\cos 5u = \tfrac{128}{135135}\frac{d^3P_8}{d\mu^3} - \tfrac{8}{1485}\frac{d^3P_6}{d\mu^3} + \tfrac{5}{429}\frac{d^3P_4}{d\mu^3} \quad \ldots \quad (L),$$

$$\cos 6u = \tfrac{512}{765765}\frac{d^3P_9}{d\mu^3} - \tfrac{128}{36465}\frac{d^3P_7}{d\mu^3} + \tfrac{4}{585}\frac{d^3P_5}{d\mu^3} - \tfrac{7}{1287}\frac{d^3P_3}{d\mu^3} \quad \ldots \quad (M).$$

There is another formula useful in similar investigations, which may find a place here, although I have not used it in the final reductions. It is the expression of a zonal harmonic in terms of the n^{th} differential coefficient of other zonal harmonics.

$$\begin{aligned} P_i = & \frac{(2i+2n+1)}{(2i+2n+1)(2i+2n-1)\ldots(2i+1)}\, d^nP_{i+n} \\ & - \frac{n}{1}\,\frac{(2i+2n-3)}{(2i+2n-1)(2i+2n-3)\ldots(2i-1)}\, d^nP_{i+n-2} \\ & + \frac{n\,.\,n-1}{1\,.\,2}\cdot\frac{(2i+2n-7)}{(2i+2n-3)\ldots(2i-3)}\, d^nP_{i+n-4} \ldots \\ & \pm \frac{2i-(2n-1)}{(2i+1)(2i-1)\ldots(2i-(2n-1))}\, d^nP_{i-n}, \end{aligned}$$

the general p^{th} term being

$$(-1)^{p-1}\frac{n\,.\,n-1\ldots n-(p-2)}{1\,.\,2\ldots p-1}\;\frac{2i+2n+5-4p}{(2i+2n+3-2p)(2i+2n+1-2p)\ldots(2i+3-2p)}\, d^nP_{i+n-2p+4}.$$

The proof is conducted exactly in the same way as for equation (3). If i is equal to or greater than $2n$, the series has its full number of terms, viz., $n+1$, otherwise the series breaks off owing to the differential coefficients vanishing.

III. *Expansion of Potential in terms of a Series of Surface Harmonics.*

We know from observation that, excepting the Arctic regions, the daily variation of the West force is nearly the same along the same circle of latitude. It is this fact which renders the present investigation possible, as comparatively few places of observation will be necessary to give us a very fair idea of the nature of the oscillation over a considerable area of the Earth's surface. If at any place we find that the daily oscillation of any one element can be expressed in the form

$$a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t + \ldots,$$

when t is reckoned by local time, we may get the variation at any point of the same latitude circle by writing $t + \lambda$ for t, where λ is the longitude towards the East from some standard meridian and t now is the time of the standard meridian. At the time $t = 0$ we have then the variation of the force to geographical West in different longitudes expressed by

$$\mathrm{Y} = a_1 \cos \lambda + b_1 \sin \lambda + a_2 \cos 2\lambda + b_2 \sin 2\lambda.$$

The coefficients will be functions of the latitude, and by expressing these functions in a series of proper form we may at once obtain an expression for the potential.

If X is the force to geographical North, Y the force to geographical West, and Z the vertical force, reckoned positive upwards, we have, putting u for the colatitude, and λ for the longitude towards the East,

$$\mathrm{X} = \frac{d\mathrm{V}}{a\,du}, \qquad \mathrm{Y} \sin u = \frac{d\mathrm{V}}{a\,d\lambda}, \qquad \mathrm{Z} = -\frac{d\mathrm{V}}{dr},$$

a being the Earth's radius.

If we can expand Y sin u in terms of surface harmonics our task is accomplished, and for this purpose we need only express $a_1 \sin u$, $b_1 \sin u$, &c., in a series of tesseral harmonics.

The expansion of a function of an angle in terms of the trigonometrical functions of its multiples is so easily carried out by the method of least squares, if the function is given for a regular series of submultiples of 2π, that it seemed to me to be the easiest method of proceeding to obtain first by interpolation from the observed coefficients of Y its values for equidistant circles of latitude.

To obtain a curve for each of the values a_1, b_1, a_2, b_2, as depending on the latitude, we have the following data. The values are directly observed for four points in the Northern hemisphere. Taking the potential (as it is observed to be) symmetrical on the Northern and Southern hemispheres, and fixing the period to which we apply the calculation to be the one for which the mean value of the observed summer variations in the Northern hemisphere holds good, we must put, for the West force at the same period, in the Southern half of the globe, the observed *winter* values of the corresponding Northern latitudes, reversing, however, the sign to make our expression agree with observation. We have then eight values for the different terms of Y through which we might at once proceed to draw a curve ; but, making use of the observed variations of Northern force, we can also calculate the direction of the tangent of the required curve for the same eight points. This has been done as follows. Let Y be expressed as

$$\Sigma(a_n \cos n\lambda + b_n \sin n\lambda),$$

and X as

$$\Sigma(\alpha_n \cos n\lambda + \beta_n \sin n\lambda).$$

The existence of a potential implies the relation

$$\frac{d\mathrm{Y} \sin u}{du} = \frac{d\mathrm{X}}{d\lambda}$$

or

$$\frac{d\mathrm{Y}}{du} = \frac{d\mathrm{X}}{d\lambda} \operatorname{cosec} u - \mathrm{Y} \cot u,$$

from which, by substitution,

$$\Sigma\left(\frac{da_n}{du} \cos n\lambda + \frac{db_n}{du} \sin n\lambda\right)$$
$$= \Sigma[(n\beta_n \operatorname{cosec} u - a_n \cot u) \cos n\lambda - (n\alpha_n \operatorname{cosec} u + b_n \cot u) \sin n\lambda] ;$$

and, therefore,

$$\frac{da_n}{du} = n\beta_n \operatorname{cosec} u - a_n \cot u,$$

$$\frac{db_n}{du} = - n\alpha_n \operatorname{cosec} u + b_n \cot u.$$

These equations give the rate of change per radian ; to get the rate of change per degree of colatitude we have to multiply the differential coefficients with the circular measure of one degree. The following Table gives the rate of change of the West force per degree of colatitude, calculated as explained from the North force.

TABLE V.

The unit of force in this Table is 10^{-6} C.G.S., and the unit of latitude is 1 degree.

	Bombay.		Lisbon.		Greenwich.		St. Petersburg.	
	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.	Summer.	Winter.
da_1/du . . .	− 0·201	+ 0·714	− 1·210	− 2·561	+ 0·356	− 2·793	+ 1·951	− 3·086
db_1/du . . .	− 7·853	− 6·326	+ 0·432	+ 0·931	+ 2·118	+ 2·053	+ 2·342	+ 1·107
da_2/du . . .	− 1·184	+ 0·258	− 2·087	− 2·207	+ 0·150	− 0·330	− 0·079	− 0·605
db_2/du . . .	− 6·079	− 4·754	+ 0·948	+ 1·684	+ 5·445	+ 3·318	+ 7·185	+ 2·689
da_3/du . . .	− 2·921	− 1·968	− 1·218	− 0·658	− 0·416	+ 0·600	+ 2·805	+ 1·650
db_3/du . . .	− 2·420	− 2·277	− 0·527	+ 0·580	+ 0·988	+ 1·564	− 0·030	+ 1·112
da_4/du . . .	− 1·125	− 1·768	− 1·084	+ 0·503	+ 0·501	+ 0·622	+ 0·329	+ 0 640
db_4/du . . .	+ 0·407	− 1·117	− 0·471	+ 0·018	− 1·036	+ 0·109	+ 0·936	+ 0·137

Curves were now carefully drawn for each of the eight coefficients, making them fit in as well as possible with the ordinates and the direction of their tangents, as given in Tables III. and V.

The values of Y were then read off for each 7°·5 of colatitude, and a fresh table was formed (Table VI.).

From this point onwards we have to carry on the calculations separately for each type of the variation.

TABLE VI.

Coefficients in the series $Y = a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t +$ &c. for different degrees of colatitude, the unit of force being C.G.S. $\times 10^{-6}$.

Colatitude.	a_1.	b_1.	a_2.	b_2.	a_3.	b_3.	a_4.	b_4.
°								
0	+ 10	0	0	0	0	(+ 48)	(0)	(0)
7·5	+ 32	+ 97	+ 42	+ 9	(+ 12)	(+ 51)	(+ 1)	(+ 3)
15·0	+ 52	+ 158	+ 76	+ 22	(+ 24)	(+ 53)	(+ 1)	(+ 6)
22·5	+ 71	+ 196	+ 91	+ 48	+ 39	+ 54	+ 1	+ 7
30·0	+ 85	+ 215	+ 90	+ 101	+ 56	+ 54	+ 2	+ 11
37·5	+ 96	+ 231	+ 89	+ 143	+ 73	+ 55	+ 11	+ 18
45·0	+ 92	+ 243	+ 96	+ 160	+ 99	+ 61	+ 16	− 3
52·5	+ 83	+ 247	+ 90	+ 168	+ 98	+ 58	+ 22	− 21
60·0	+ 74	+ 205	+ 83	+ 165	+ 91	+ 54	+ 20	− 24
67·5	+ 65	+ 144	+ 131	+ 83	+ 89	+ 28	+ 11	− 20
75·0	+ 63	+ 87	+ 127	+ 37	+ 69	+ 10	+ 2	− 15
82·5	+ 58	+ 58	+ 117	+ 15	+ 46	− 3	− 6	− 11
90·0	+ 37	+ 42	+ 82	+ 7	+ 25	− 2	− 14	+ 6
97·5	+ 10	+ 21	+ 15	+ 1	+ 3	+ 1	− 20	+ 18
105·0	− 3	− 7	− 4	− 17	− 16	− 6	− 27	+ 13
112·5	+ 3	− 56	− 3	− 48	− 36	− 23	− 40	0
120·0	0	− 105	− 4	− 102	− 47	− 45	− 43	− 23
127·5	− 50	− 120	− 15	− 118	− 53	− 50	− 35	− 26
135·0	− 78	− 113	− 16	− 106	− 54	− 46	− 31	− 25
142·5	− 97	− 100	+ 10	− 85	− 47	− 37	− 24	− 24
150·0	− 121	− 85	+ 6	− 65	− 38	− 30	− 12	− 14
157·5	− 140	− 75	+ 2	− 46	− 22	− 21	− 5	− 13
165·0	− 154	− 61	+ 1	− 30	(− 9)	(− 14)	(− 3)	(− 9)
172·5	− 162	− 36	0	− 14	(− 2)	(− 6)	(− 2)	(− 4)
180·0	− 165	0	0	0	(0)	(0)	(0)	(0)

The quantities a_1, b_1 were expressed in the usual way in terms of the multiples of the cosines of the colatitude, and two equations obtained which can be shown to represent with sufficient accuracy the force of the first type towards the geographical West.

$$a_1 = -5 + 106{\cdot}7 \cos u - 50{\cdot}6 \cos 2u - 1{\cdot}7 \cos 3u - 14{\cdot}6 \cos 4u - 8{\cdot}9 \cos 5u - 2{\cdot}0 \cos 6u,$$

$$b_1 = 49{\cdot}4 + 154{\cdot}3 \cos u - 0{\cdot}4 \cos 2u - 76{\cdot}0 \cos 3u - 18{\cdot}1 \cos 4u - 31{\cdot}3 \cos 5u - 9{\cdot}2 \cos 6u.$$

If, now, $\cos u$, $\cos 2u$, $\cos 3u$, &c., be expressed in terms of $dP_1/d\mu$, $dP_2/d\mu_1$, $dP_3/d\mu$,

&c., we have an equation for that part of Y which depends on $\cos\lambda$ and $\sin\lambda$. After multiplication with $\sin u$, one side of the equation contains the colatitude in form of tesseral harmonics only, and hence we obtain at once the required expansion of $V^{(1)}$. The equations are

$$Y^{(1)} = \cos\lambda\left[24\cdot09\frac{dP_1}{d\mu} + 35\cdot59\frac{dP_2}{d\mu} - 8\cdot45\frac{dP_3}{d\mu} + 2\cdot39\frac{dP_4}{d\mu} - 2\cdot41\frac{dP_5}{d\mu} - 1\cdot64\frac{dP_6}{d\mu}\right]$$
$$+ \sin\lambda\left[48\cdot00\frac{dP_1}{d\mu} + 81\cdot52\frac{dP_2}{d\mu} + 5\cdot66\frac{dP_3}{d\mu} - 7\cdot62\frac{dP_4}{d\mu} - 1\cdot09\frac{dP_5}{d\mu}\right.$$
$$\left. - 5\cdot78\frac{dP_6}{d\mu} - 1\cdot57\frac{dP_7}{d\mu} - 0\cdot34\frac{dP_7}{d\mu}\right].$$

With the help of

$$\frac{dV}{d\lambda} = Ya\sin u,$$

we have, finally,

$$- V^{(1)}/a = \cos\lambda[48\cdot00T_1^1 + 81\cdot52T_2^1 + 5\cdot66T_3^1 - 7\cdot62T_4^1$$
$$- 1\cdot09T_5^1 - 5\cdot78T_6^1 - 1\cdot57T_7^1]$$
$$+ \sin\lambda[- 24\cdot09T_1^1 + 35\cdot59T_2^1 + 8\cdot45T_3^1 - 2\cdot39T_4^1$$
$$+ 2\cdot41T_5^1 + 1\cdot64T_6^1 + 0\cdot34T_7^1] \quad . \quad . \quad [A].$$

To obtain, similarly, an expression for $V^{(2)}$, the series of coefficients a_2, b_2 were expressed in terms of $\sin u$, $\sin 2u$, &c., giving the equations

$$a_2 = 67\cdot0\sin u + 61\cdot3\sin 2u + 7\cdot4\sin 3u - 11\cdot6\sin 4u + 15\cdot9\sin 5u + 18\cdot6\sin 6u,$$
$$b_2 = 21\cdot5\sin u + 113\cdot2\sin 2u + 7\cdot7\sin 3u - 12\cdot6\sin 4u - 10\cdot8\sin 5u - 28\cdot2\sin 6u.$$

From the known expansion of $\cos pu$, in terms of the first differential coefficient of the zonal harmonics, we can obtain by differentiation an equation which will at once give us $V^{(2)}$ in the required form.

Thus, for instance, we have by Equation D, page 474,

$$\cos 4u = \tfrac{64}{315}\frac{dP_5}{d\mu} - \tfrac{16}{45}\frac{dP_3}{d\mu} + \tfrac{3}{35}\frac{dP_1}{d\mu},$$

and differentiating with respect to μ,

$$4\sin 4u \,/\, \sin u = \tfrac{64}{315}\frac{d^2P_5}{d\mu^2} - \tfrac{16}{45}\frac{d^2P_3}{d\mu^2} + \tfrac{3}{35}\frac{d^2P_1}{d\mu^2}.$$

By substituting this and other similar expressions, we find

$$Y^{(2)}/\sin u = \cos 2\lambda \left[64{\cdot}59 + 9{\cdot}42 \frac{d^2P_3}{d\mu^2} - 0{\cdot}42 \frac{d^2P_4}{d\mu^2} - 1{\cdot}47 \frac{d^2P_5}{d\mu^2} + 0{\cdot}59 \frac{d^2P_6}{d\mu^2} + 0{\cdot}51 \frac{d^2P_7}{d\mu^2} \right]$$
$$+ \sin 2\lambda \left[17{\cdot}69 + 15{\cdot}87 \frac{d^2P_3}{d\mu^2} + 1{\cdot}27 \frac{d^2P_4}{d\mu^2} + 0{\cdot}68 \frac{d^2P_5}{d\mu^2} - 0{\cdot}40 \frac{d^2P_6}{d\mu^2} - 0{\cdot}80 \frac{d^2P_7}{d\mu^2} \right],$$

and from this, by multiplication with $\sin^2 u$ and integration with respect to λ,

$$- V^{(2)}/a = \cos 2\lambda \left[2{\cdot}95\, T_2^2 + 7{\cdot}94\, T_3^2 + 0{\cdot}63\, T_4^2 + 0{\cdot}34\, T_5^2 - 0{\cdot}20\, T_6^2 - 0{\cdot}41\, T_7^2\right]$$
$$- \sin 2\lambda \left[10{\cdot}76\, T_2^2 + 4{\cdot}72\, T_3^2 - 0{\cdot}21\, T_4^2 - 0{\cdot}73\, T_5^2 + 0{\cdot}29\, T_6^2 + 0{\cdot}26\, T_7^2\right] . \text{ [B]}.$$

To find $V^{(3)}$ from the coefficients a_3, b_3, a_4, b_4, the values of these coefficients were all divided by $\sin^2 u$, for a reason which will appear. It was then found, however, that to represent the numbers so obtained satisfactorily, a greater number of terms was necessary in the expansion than the observations seemed to justify. I was thus led to give up calculating as far as these types are concerned the terms on which the difference between summer and winter depends, so that, instead of using the coefficients from Table VI., the mean of corresponding values on both sides of the equator were taken, changing, of course, the sign of the coefficient given for the Southern hemisphere. In the next place, it was found that, owing to the smallness of $\sin u$ at high latitude, the three numbers corresponding to the colatitudes 0°, 7°·5, 15° were very large, while their weight is very small, as they have only been obtained by graphical extrapolation; I have, therefore, discarded these numbers altogether, putting them into brackets in Table VI. BESSEL has shown how the method of least squares may be applied to obtain a trigonometrical series for a succession of values some of which are missing.

The equations thus calculated are as follows :—

$$a_3/\sin^2 u = 74 \cos u - 118 \cos 3u - 101 \cos 5u,$$
$$b_3/\sin^2 u = 23 \cos u - 111 \cos 3u - 127 \cos 5u,$$
$$a_4/\sin^2 u = 39 \sin 2u - 6 \sin 4u - 9 \sin 6u,$$
$$b_4/\sin^2 u = 31 \sin 2u + 38 \sin 4u + 4 \sin 6u.$$

The agreement with the numbers from which the series are obtained is not as good as would be desirable, especially for a_3 and b_3, but the types higher than the second will probably depend much on local circumstances, and the result would not, in my opinion, repay the trouble of taking account of further terms in the above series; $\cos u$, $\cos 3u$, and $\cos 5u$ have already been given in terms of $d^3P_i/d\mu^3$ (Equations G, I, L, page 474), and, by differentiation with respect to u, we can also get $\sin 2u$, sin $4u$, sin $6u$ in terms of $\sin u\, d^4P_i / d\mu^4$. This gives, by substitution and integration with respect to λ,

$$
\begin{aligned}
-V^{(3)}/a = & (-0\cdot0401\, T_8{}^3 + 0\cdot1426\, T_6{}^3 + 0\cdot2523\, T_4{}^3) \cos 3\lambda \\
& + (0\cdot0319\, T_8{}^3 - 0\cdot0936\, T_6{}^3 - 0\cdot5577\, T_4{}^3) \sin 3\lambda \\
& + (0\cdot00033\, T_9{}^4 + 0\cdot00279 T_7{}^4 - 0\cdot00411\, T_5{}^4) \cos 4\lambda \\
& + (-0\cdot00075\, T_9{}^4 + 0\cdot00078\, T_7{}^4 - 0\cdot01978\, T_5{}^4) \sin 4\lambda.
\end{aligned}
$$

We have obtained thus finally, 38 coefficients in the expansion of V/a, which, for the sake of reference, I collect into Table VII.

In this Table $C_n{}^m$ is the coefficient of $T_n{}^m \cos m\lambda$.

$S_n{}^m$,, ,, $T_n{}^m \sin m\lambda$.

TABLE VII.

The unit of force is C.G.S. 10^{-6}.

$C_1{}^1$	− 48·00	$C_2{}^2$	− 2·948	$C_4{}^3$	− 0·252
$C_2{}^1$	− 81·52	$C_3{}^2$	− 7·936	$C_6{}^3$	− 0·143
$C_3{}^1$	− 5·66	$C_4{}^2$	− 0·630	$C_8{}^3$	+ 0·040
$C_4{}^1$	+ 7·62	$C_5{}^2$	− 0·341		
$C_5{}^1$	+ 1·09	$C_6{}^2$	+ 0·200	$C_5{}^4$	+ 0·00411
$C_6{}^1$	+ 5·78	$C_7{}^2$	+ 0·407	$C_7{}^4$	− 0·00279
$C_7{}^1$	+ 1·57			$C_9{}^4$	− 0·00033
$S_1{}^1$	+ 24·09	$S_2{}^2$	+ 10·764	$S_4{}^3$	+ 0·578
$S_2{}^1$	+ 35·59	$S_3{}^2$	+ 4·715	$S_6{}^3$	+ 0·094
$S_3{}^1$	− 8·45	$S_4{}^2$	− 0·214	$S_8{}^3$	− 0·032
$S_4{}^1$	+ 2·39	$S_5{}^2$	− 0·731		
$S_5{}^1$	− 2·41	$S_6{}^2$	+ 0·293	$S_5{}^4$	+ 0·01978
$S_6{}^1$	− 1·64	$S_7{}^2$	+ 0·264	$S_7{}^4$	− 0·00078
$S_7{}^1$	− 0·34			$S_9{}^4$	+ 0·00075

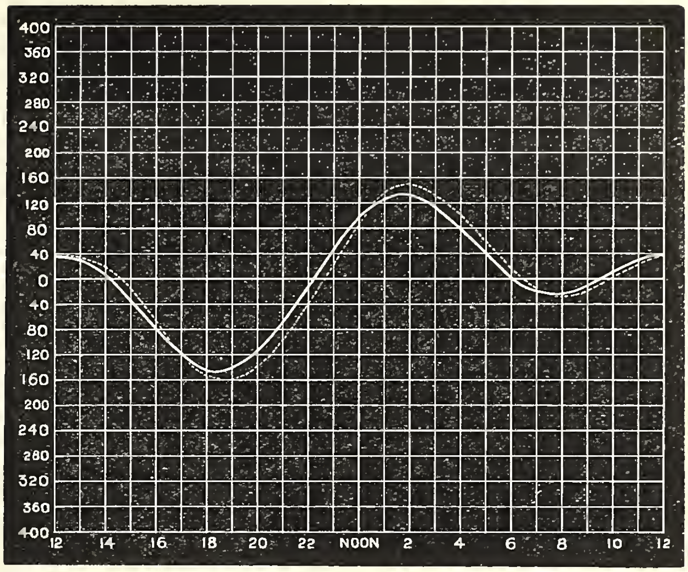
In order to show how far these numbers correctly represent the forces which have been made use of in their computation I have calculated backwards from the potential the force to geographical West. Table VIII. exhibits the results, showing by comparison the coefficients calculated from the formulæ for the potential with those obtained directly by observation. It will be seen that the agreement is satisfactory, except for the coefficients a_3 and b_3.

In figs. 1, 2, 3, and 4 the curves for the mean of the year are plotted. In these curves only the first four terms $a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t$ have been used. The numbers from which the curves are plotted are given in Table IX.

The curves are seen to be almost identical, and having, therefore, obtained an expression for the potential which correctly represents the observed West force, we may turn to the main object of this inquiry and calculate the vertical forces.

The calculation of the various components from the potential involves the knowledge of the differential coefficients of zonal harmonics and of the tesseral harmonics for the

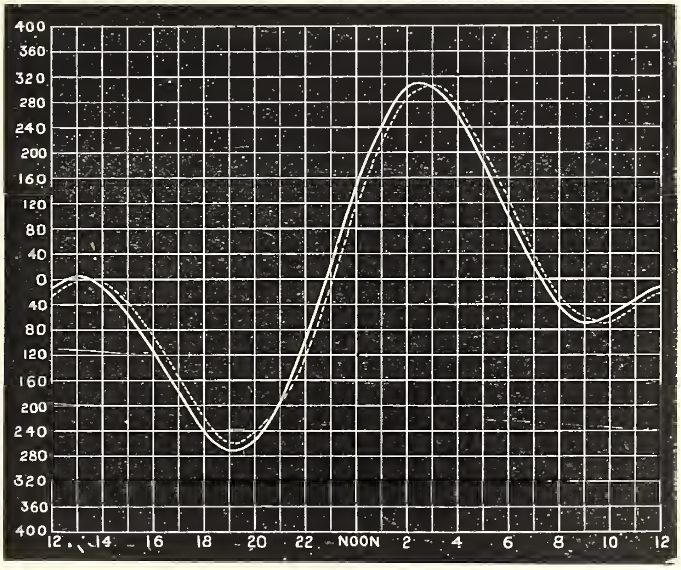
Fig. 1.



BOMBAY.

Comparison between calculated and observed curve of West force. The abscissæ denote astronomical time, the ordinates the force to geographical West, the unit of force being C.G.S. 10^{-6}.
Observed curve, white line. Calculated curve, dotted line.

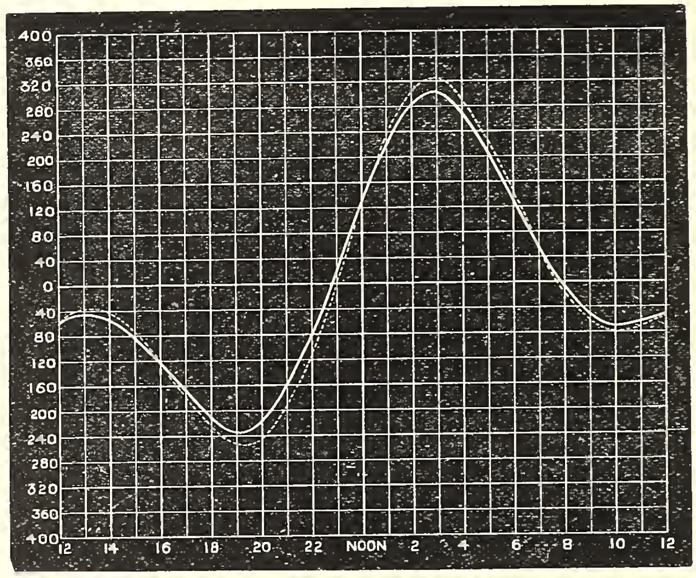
Fig. 2.



LISBON.

Comparison between calculated and observed curve of West force. The abscissæ denote astronomical time, the ordinates the force to geographical West, the unit of force being C.G.S. 10^{-6}.
Observed curve, white line. Calculated curve, dotted line.

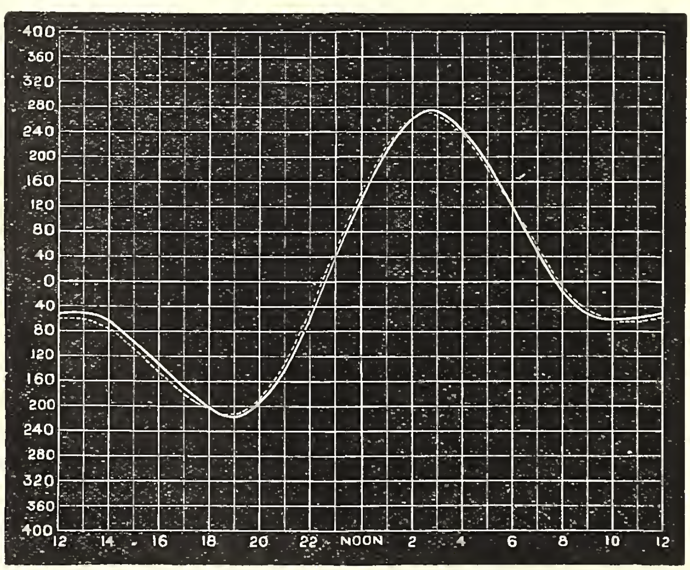
Fig. 3.



GREENWICH.

Comparison between calculated and observed curve of West force. The abscissæ denote astronomical time, the ordinates the force to geographical West, the unit of force being C.G.S. 10^{-6}. Observed curve, white line. Calculated curve, dotted line.

Fig. 4.



ST. PETERSBURG.

Comparison between calculated and observed curve of West force. The abscissæ denote astronomica time, the ordinates the force to geographical West, the unit of force being C.G.S. 10^{-6}. Observed curve, white line. Calculated curve, dotted line.

four observing stations. The numbers which I have computed for this purpose are given in Table X.

In that table T_p^9 stands for

$$\frac{d^9 P_p}{d\mu^9} \sin^9 u,$$

where $\mu = \cos u$ and P_p is the zonal harmonic of degree p.

TABLE VIII.—Comparison between observed and calculated coefficients of West force.

		a_1.		b_1.		a_2.		b_2.	
		Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
Bombay	Summer	+ 58	+ 58	+ 116	+ 124	+ 132	+ 124	+ 60	+ 73
	Winter	0	− 4	+ 32	+ 41	+ 4	+ 8	+ 23	+ 39
	Mean	+ 29	+ 27	+ 74	+ 82	+ 68	+ 66	+ 41	+ 56
Lisbon	Summer	+ 74	+ 88	+ 225	+ 232	+ 108	+ 90	+ 158	+ 172
	Winter	+ 87	+ 52	+ 119	+ 112	+ 31	+ 10	+ 104	+ 105
	Mean	+ 80	+ 70	+ 172	+ 172	+ 70	+ 50	+ 131	+ 138
Greenwich	Summer	+ 85	+ 91	+ 233	+ 255	+ 83	+ 85	+ 135	+ 149
	Winter	+ 105	+ 95	+ 115	+ 113	+ 1	− 5	+ 87	+ 104
	Mean	+ 95	+ 93	+ 174	+ 184	+ 42	+ 40	+ 111	+ 127
St. Petersburg	Summer	+ 100	+ 79	+ 225	+ 229	+ 99	+ 92	+ 115	+ 100
	Winter	+ 92	+ 121	+ 96	+ 93	− 15	− 6	+ 69	+ 72
	Mean	+ 96	+ 100	+ 161	+ 161	+ 42	+ 43	+ 92	+ 87

		a_3.		b_3.		a_4.		b_4.	
		Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
Bombay	Mean of year	+ 71	+ 19	+ 8	− 24	+ 24	+ 20	− 14	− 12
Lisbon	,,	+ 63	+ 108	+ 57	+ 88	+ 24	+ 21	+ 8	+ 5
Greenwich	,,	+ 41	+ 82	+ 44	+ 74	+ 17	+ 31	+ 14	+ 21
St. Petersburg	,,	− 12	− 7	− 8	+ 16	− 18	− 20	− 1	+ 27

TABLE IX.—Comparison between calculated and observed variation of force to geographical West. The unit of force is C.G.S. 10^{-6}. The first four terms only of the series $a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t$ have been taken into account.

Astronomical time.	Bombay.		Lisbon.		Greenwich		St. Petersburg.	
	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
a_1	+ 29	+ 27	+ 80	+ 70	+ 95	+ 93	+ 96	+ 100
b_1	+ 74	+ 82	+ 172	+ 172	+ 174	+ 184	+ 161	+ 161
a_2	+ 68	+ 66	+ 70	+ 50	+ 42	+ 40	+ 42	+ 43
b_2	+ 41	+ 56	+ 131	+ 138	+ 111	+ 127	+ 92	+ 87
Hour.								
0	+ 97·0	+ 93·0	+ 150·0	+ 120·0	+ 137·0	+ 133·0	+ 138·0	+ 143·0
1	+ 126·6	+ 132·5	+ 247·9	+ 224·4	+ 228·8	+ 235·6	+ 216·8	+ 219·0
2	+ 131·6	+ 145·9	+ 303·7	+ 291·1	+ 286·4	+ 302·5	+ 264·3	+ 263·9
3	+ 113·8	+ 133·1	+ 309·2	+ 309·1	+ 301·2	+ 322·9	+ 273·7	+ 271·5
4	+ 80·1	+ 100·0	+ 267·4	+ 278·5	+ 273·3	+ 295·8	+ 246·1	+ 243·2
5	+ 40·6	+ 57·0	+ 191·8	+ 210·0	+ 211·8	+ 230·7	+ 190·0	+ 187·7
6	+ 6·0	+ 16·0	+ 102·0	+ 122·0	+ 132·0	+ 144·0	+ 119·0	+ 118·0
7	− 15·4	− 13·0	+ 19·4	+ 35·8	+ 51·6	+ 55·5	+ 48·2	+ 48·9
8	− 19·9	− 24·0	− 39·4	− 30·5	− 13·9	− 17·2	− 9·3	− 7·4
9	− 9·2	− 17·1	− 66·0	− 65·9	− 55·2	− 62·7	− 46·1	− 43·9
10	+ 10·4	+ 2·1	− 61·7	− 69·1	− 70·4	− 78·5	− 61·3	− 59·9
11	+ 29·6	+ 24·3	− 37·7	− 48·8	− 65·8	− 71·0	− 60·6	− 61·2
12	+ 39·0	+ 39·0	− 10·0	− 20·0	− 53·0	− 53·0	− 54·0	− 57·0
13	+ 32·2	+ 37·9	+ 4·3	+ 0·2	− 45·0	− 39·4	− 52·0	− 57·6
14	+ 7·4	+ 17·1	− 6·9	− 2·1	− 52·2	− 42·5	− 62·9	− 70·3
15	− 31·8	− 21·1	− 47·2	− 33·1	− 79·2	− 68·9	− 89·7	− 97·5
16	− 77·1	− 69·0	− 110·6	− 89·5	− 123·1	− 115·8	− 128·7	− 135·6
17	− 117·4	− 115·4	− 182·0	− 158·6	− 173·6	− 172·9	− 170·8	− 175·1
18	− 142·0	− 148·0	− 242·0	− 222·0	− 216·0	− 224·0	− 203·0	− 204·0
19	− 143·4	− 157·4	− 271·6	− 260·4	− 235·4	− 251·7	− 213·0	− 210·3
20	− 119·1	− 139·0	− 257·4	− 258·5	− 220·3	− 242·8	− 192·1	− 186·2
21	− 72·8	− 94·9	− 196·0	− 210·1	− 166·8	− 191·3	− 137·9	− 130·1
22	− 13·4	− 33·1	− 95·1	− 119·9	− 79·8	− 101·5	− 56·1	− 47·7
23	+ 47·2	+ 34·1	+ 27·9	− 2·6	+ 27·6	+ 13·2	+ 41·4	+ 48·6

Table X.—Numerical values for the zonal harmonics, their differential coefficients, and the functions T_p^q.

	Bombay.	Lisbon.	Greenwich.	St. Petersburg.
P_1	+ 0·3239	+ 0·6255	+ 0·7824	+ 0·8654
P_2	− 0·3426	+ 0·0868	+ 0·4183	+ 0·6235
P_3	− 0·4009	− 0·3265	+ 0·0238	+ 0·3224
P_4	+ 0·0297	− 0·4225	− 0·2811	+ 0·0206
P_5	+ 0·3380	− 0·2144	− 0·4149	− 0·2258
P_6	+ 0·1760	+ 0·1061	− 0·3620	− 0·3754
$dP_2/d\mu$ . . .	+ 0·9718	+ 1·8764	+ 2·3473	+ 2·5963
$dP_3/d\mu$ . . .	− 0·7131	+ 1·4341	+ 3·0914	+ 4·1174
$dP_4/d\mu$ . . .	− 1·8346	− 0·4089	+ 2·5142	+ 4·8529
$dP_5/d\mu$ . . .	− 0·4457	− 2·3681	+ 0·5618	+ 4·3029
$dP_6/d\mu$ . . .	+ 1·8839	− 2·7679	− 2·0498	+ 2·3692
$dP_7/d\mu$ . . .	+ 1·8421	− 0·9882	− 4·1305	− 0·5776
$d^2P_2/d\mu^2$ . .	+ 3·0000	+ 3·0000	+ 3·0000	+ 3·0000
$d^2P_3/d\mu^2$ . .	+ 4·8588	+ 9·3821	+ 11·7364	+ 12·9816
$d^2P_4/d\mu^2$ . .	− 1·9916	+ 13·0386	+ 24·6401	+ 31·8221
$d^2P_5/d\mu^2$ . .	− 11·6529	+ 5·7017	+ 34·3644	+ 56·6573
$d^2P_6/d\mu^2$ . .	− 6·8945	− 13·0104	+ 30·8207	+ 79·1540
$d^2P_7/d\mu^2$ . .	+ 12·8380	− 30·2804	+ 7·7166	+ 87·4567
$d^2P_8/d\mu^2$ . .	+ 20·7369	− 27·8334	− 31·1374	+ 70·4907
$d^3P_4/d\mu^3$ . .	+ 34·011	+ 65·674	+ 82·155	+ 90·872
$d^3P_6/d\mu^3$ . .	− 94·170	+ 128·393	+ 460·164	+ 714·102
$d^3P_8/d\mu^3$ . .	+ 98·399	− 325·813	+ 575·912	+ 2025·953
$d^4P_5/d\mu^3$ . .	+306·10	+ 591·07	+ 739·39	+ 817·84
$d^4P_7/d\mu^3$ . .	−918·11	+2260·17	+ 6721·52	+10101·17
$d^4P_9/d\mu^3$ . .	+754·68	−3278·66	+16512·03	+44542·37
T_1^1	+ 0·9194	+ 1·4640	+ 1·4620	+ 1·3008
T_2^1	− 0·6747	+ 1·1190	+ 1·9253	+ 2·0629
T_3^1	− 1·7358	− 0·3190	+ 1·5658	+ 2·4313
T_4^1	− 0·4217	− 1·8477	+ 0·3498	+ 2·1558
T_5^1	+ 1·7825	− 2·1596	− 1·2766	+ 1·1870
T_6^1	+ 1·7429	− 0·7710	− 2·5725	− 0·2894
T_7^1				
T_2^2	+ 2·685	+ 1·826	+ 1·163	+ 0·753
T_3^2	+ 4·349	+ 5·712	+ 4·551	+ 3·258
T_4^2	− 1·783	+ 7·938	+ 9·556	+ 7·987
T_5^2	− 10·430	+ 3·471	+ 13·327	+ 14·221
T_6^2	− 6·171	− 7·921	+ 11·953	+ 19·868
T_7^2	+ 11·491	− 18·435	+ 2·993	+ 21·952
T_8^2	+ 18·561	− 16·945	− 12·075	+ 17·693
T_4^3	+ 28·801	+ 31·195	+ 19·841	+ 11·427
T_6^3	− 79·745	+ 60·985	+ 111·130	+ 89·801
T_8^3	+ 83·326	− 154·760	+ 139·090	+ 254·770
T_5^4	+245·23	+ 219·06	+ 111·20	+ 51·525
T_7^4	−735·55	+ 837·66	+ 1010·90	+ 636·390
T_9^4	+604·62	−1215·10	+ 2483·40	+ 2806·300

IV. *Comparison of Observed and Calculated Vertical Forces.*

The complete potential can be written down from the value at its surface in the usual manner, either on the supposition that the potential is zero at an infinite distance, or that it is zero at the centre of the Earth, the first supposition corresponding to the hypothesis that the seat of the magnetic variation is outside the Earth. If Y_n is a surface harmonic of degree n occurring in the expansion of V/a, the solid harmonic will either be $Y_n(r/a)^n$ or $Y_n(r/a)^{-(n+1)}$. The vertical force is given by $\partial V/\partial r$, as the force is considered positive when it acts downwards. At the surface, therefore, we have a term for the vertical force which is either nY_n or $-(n+1)Y_n$.

Before we proceed to discuss the comparison between the observed and calculated values of the vertical force, a few words are necessary regarding the available observations.

The only station for which we have complete records for 1870 is Lisbon. It is therefore impossible to obtain a satisfactory series for the vertical force which would give us, if our information was more complete, directly the two terms, one due to outside, the other due to inside, effects. But I shall show that even from the existing data we can draw important conclusions.

At Bombay no vertical force determinations are published, as far as I know, before 1873, when the magnetograph came into operation; but we have complete records between 1873 and 1878. During these years the general type of the vertical force remained practically the same, only the range varying. Figs. 5 and 6, for instance,

Fig. 5.

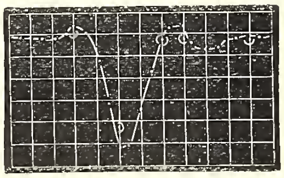
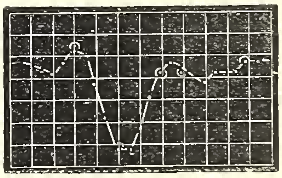


Fig. 6.



are tracings of the curves given by Mr. CHAMBERS in the years 1873 and 1877, the two years which differ most in range. The other years show curves varying between these two extremes. As far as the general form of the curve is concerned, we cannot go far wrong, therefore, if we make use of the 1873 observations, especially as the horizontal components show no marked difference (except as regards range) in 1873 and 1870.

Similar remarks apply to Greenwich. Although published records exist for 1870, there is an uncertainty about the temperature correction, which makes the vertical force observations previous to 1882 useless for our present purpose.

Table XI. will give an idea of the changes which the vertical force variation is subject to in various years. The numbers are copied from the published records of the Greenwich Observatory.

Table XI.

Coefficients in the series

$$V_t = c_1 \sin (t + \alpha) + c_2 \sin (2t + \beta) + c_3 \sin (3t + \gamma) + c_4 \sin (4t + \gamma)$$

where changes in V_t represent changes in the vertical force, the unit being ·00001 of the whole vertical force; t being the time from midnight.

	c_1.	α.	c_2.	β.	c_3.	γ.	c_4.	δ.
		°		°		°		°
1883	14·3	148·13	13·1	266·58	5·3	89·60	1·3	293·20
1884	14·8	139·33	11·7	272·00	5·5	95·52	2·1	289·49
1885	13·0	137·50	11·7	265·35	5·1	83·28	1·5	281·04
1886	12·5	160·58	11·9	268·38	4·0	94·22	1·2	297·50

The principal discrepancy here occurs in the angle α, which would give a difference of phase between 1885 and 1886 of about 23°, or about an hour and a half; the angle α differs much at Greenwich during different months; the phases of the other terms show practically no difference. In comparing the observed and calculated values, I have taken the year 1884, as during that year the range of the declination needle was greatest, and corresponded most nearly to that of 1870.

At St. Petersburg, also, the results for 1870 are not corrected for temperature, and there is reason to believe that this has affected the observed type considerably. I have therefore compared the results of calculation with observation, also, for the year 1878, in which year the temperature corrections have been taken into account.

We must remember, then, in the comparison between the observed and calculated values, that the greatest weight must attach to the Lisbon observations, and that comparatively little value is to be given to St. Petersburg, as I have no records at my disposal which would enable me to judge how far the type of vertical force there varies from year to year.

Tables XII. and XIII. give the values of the coefficients a_1, b_1, a_2, b_2, &c., as calculated on the hypothesis that the disturbing force comes from the inside of the Earth. Tables XIV. and XV. give their values calculated on the hypothesis that the disturbing force is outside. In both cases the observed numbers are given for comparison. I have calculated the values for Lisbon separately for the winter and for the summer months; in the other cases the values for the mean of the year only have been taken. The years attached to the observing stations refer to the date of the observations; the calculated values all belong to 1870.

TABLE XII.—Comparison between the observed and calculated coefficients for the vertical force, a_1, b_1, a_2, b_2 on the hypothesis that the disturbing force is inside the Earth.

	a_1.		b_1.		a_2.		b_2.	
	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.
Bombay	+ 219	− 42	− 57	+ 9	+ 79	− 21	− 152	+ 28
Lisbon, summer . . .	+ 580	− 196	− 206	+ 66	+ 381	− 201	− 87	+ 26
„ winter	+ 336	− 135	− 149	+ 55	+ 277	− 103	− 19	+ 1
Greenwich, 1884 . . .	+ 350	− 42	− 190	+ 49	+ 139	− 51	− 34	+ 2
St. Petersburg, 1870. .	+ 177	+ 114	− 154	+ 125	+ 62	− 59	− 45	− 38
„ 1878. .		− 8	..	+ 29	..	− 24	..	− 2

TABLE XIII.—Comparison between the observed and calculated coefficients a_3, b_3, a_4, b_4 for the vertical force on the hypothesis that the disturbing force is inside the Earth.

	a_3.		b_3.		a_4.		b_4.	
	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.
Bombay	− 73·4	− 10	− 42·0	+ 35	− 20·4	+ 3	− 29·0	+ 16
Lisbon	+156·1	− 81	−171·4	+ 12	+ 9·2	− 21	− 22·1	+ 11
Greenwich	+ 85·5	− 24	− 88·0	+ 3	+ 28·1	− 8	− 38·2	+ 3
St. Petersburg	+ 12·0	− 12	− 17·4	− 8	+ 22·3	− 18	− 31·3	− 1

TABLE XIV.—Comparison between the observed and calculated coefficients a_1, b_1, a_2, b_2 on the assumption that the disturbing force is outside the Earth.

	a_1.		b_1.		a_2.		b_2.	
	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed	Calculated.	Observed.
Bombay	− 141	− 42	+ 31	+ 9	− 53	− 21	+ 121	+ 28
Lisbon, summer . . .	− 398	− 196	+ 137	+ 66	− 315	− 201	+ 52	+ 26
„ winter	− 248	− 135	+ 108	+ 55	− 235	− 103	+ 15	+ 1
Greenwich, 1884 . . .	− 235	− 42	+ 132	+ 49	− 110	− 51	+ 21	+ 2
St. Petersburg, 1870 . .	− 97	+ 114	+ 104	+ 125	− 40	− 59	+ 35	− 38
„ 1878. .	..	− 8	..	+ 29	..	− 24	..	− 2

TABLE XV.—Comparison between the observed and calculated coefficients a_3, b_3, a_4, b_4 on the assumption that the disturbing force is outside the Earth.

	a_3.		b_3.		a_4.		b_4.	
	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.
Bombay	+ 66	− 10	+ 22	+ 35	+ 18	+ 3	+ 24	+ 16
Lisbon	− 134	− 81	+ 125	+ 12	− 8	− 21	+ 18	+ 11
Greenwich	− 70	− 24	+ 38	+ 3	− 25	− 8	+ 33	+ 3
St. Petersburg	− 7	− 12	+ 16	− 8	− 20	− 18	+ 27	− 1

Confining ourselves in the first place to the first four coefficients, we find that out of twenty coefficients eighteen have the wrong sign on the hypothesis of an internal cause, while only two have the wrong sign on the hypothesis of an external cause, and those two belong to St. Petersburg, to which station, as was pointed out, we cannot attach much value. If instead of the numbers given for 1870 we take the numbers given at the same station for 1878, the agreement becomes better, even for St. Petersburg. The coefficients a_3, b_3, a_4, b_4 are of course more uncertain; but even here the evidence is strongly in favour of the external cause. Out of twenty coefficients seventeen agree in sign with that hypothesis.

A better comparison can perhaps be obtained in a different way: the two terms

$$a_n \cos nt + b_n \sin nt$$

can be written

$$r_n \cos n (t - t_n),$$

where r_n is the amplitude of the oscillation, and t_n the time at which the maximum elongations take place.

Tables XVI. and XVII. contain the results for t_n, and from these tables I think it will clearly appear that the phase of the vertical force completely agrees with the assumption of an *external* cause and completely disagrees with the assumption of an *internal* cause.

For Lisbon, our principal station of comparison, the phase in Table XVI. agrees for both the diurnal and semi-diurnal variation within four minutes of time. For Bombay the diurnal variation agrees within three minutes, and the semi-diurnal variation within 36 minutes. For Greenwich, the semi-diurnal variation, which we have seen differs little from year to year, agrees closely, while the diurnal variation shows a greater difference. In all these cases the phases, as calculated in Table XV., are in as great a disagreement as possible. St. Petersburg gives less decisive results, but they still go in the same direction, especially if we take the observations of 1878 to represent the type of vertical variation.

The amplitudes of the variation are given in Table XVIII. where the second column gives the calculated values on the hypothesis which we must reject, while the third column gives the same numbers calculated on the assumption of an external cause.

The calculated numbers are much larger than the observed ones, which is a fact requiring explanation. But, the range of vertical force differing from year to year, we must confine ourselves in our reasoning principally to the Lisbon observations.

The changes in the range of different years, as far as we have any observations, are not so considerable, however, as to account for the discrepancies at Bombay or Greenwich, and we must conclude, I believe, that also for those stations a considerably smaller range is obtained in the observed than in the calculated forces.

TABLE XVI.—Observed and calculated values of the coefficients t_1 and t_2 of vertical force, when expressed in the form $r_1 \cos (t - t_1) + r_2 \cos 2 (t - t_2)$ on the supposition that the disturbing force is inside the Earth.

	t_1.			t_2.		
	Calculated.	Observed.	Difference.	Calculated.	Observed.	Difference.
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
Bombay	23 02	11 13	+ 11 49	9 55	4 23	+ 5 32
Lisbon	22 35	10 40	+ 11 58	11 42	5 50	+ 5 52
Greenwich	22 06	8 42	− 11 57	11 32	5 56	+ 5 36
St. Petersburg, 1870 . .	21 16	3 10	− 5 54	10 48	7 05	+ 3 43
,, 1878 . .	..	7 05	− 9 49	..	6 12	+ 4 36

TABLE XVII.—Observed and calculated values of the coefficients t_1 and t_2 when expressed in the form $r_1 \cos (t - t_1) + r_2 \cos 2 (t - t_2)$ on the supposition that the disturbing force is outside the Earth.

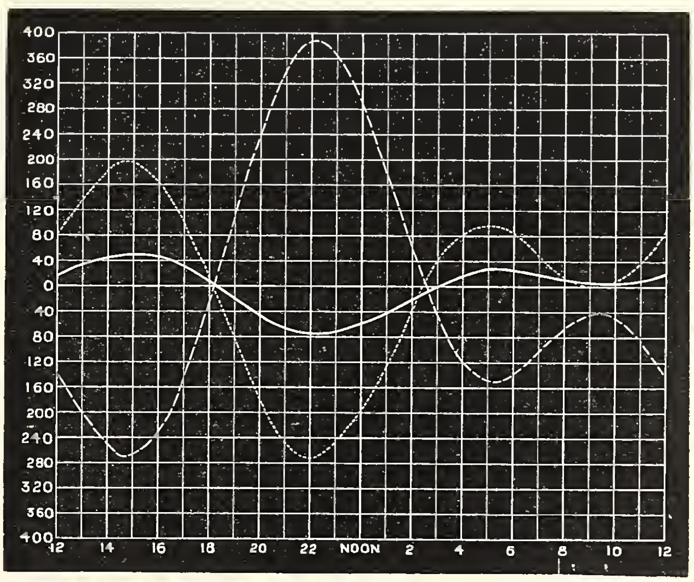
	t_1.			t_2.		
	Calculated.	Observed.	Difference.	Calculated.	Observed.	Difference.
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
Bombay	11 10	11 13	− 0 03	3 47	4 23	− 0 36
Lisbon	10 37	10 40	− 0 03	5 46	5 50	− 0 04
Greenwich	10 03	8 42	+ 1 21	5 38	5 56	− 0 18
St. Petersburg, 1870 . .	8 52	3 10	+ 5 42	4 38	7 05	− 2 27
,, 1878 . .	..	7 05	− 1 47	..	6 12	− 1 34

TABLE XVIII.—Observed and calculated values of r_1 and r_2 in the expression $r_1 \cos(t - t_1) + r_2 \cos 2(t - t_2)$ for vertical force.

	r_1.			r_2.		
	Calculated from inside.	Calculated from outside.	Observed.	Calculated from inside.	Calculated from outside.	Observed.
Bombay	226	144	43	171	132	35
Lisbon	491	346	176	333	277	153
Greenwich	398	269	65	143	112	51
St. Petersburg, 1870 . .	235	142	169	77	53	71
,, 1878 . .	..	..	30	..	..	24

The agreement between the calculated and observed curves for vertical force is best seen from the graphical representation given in figs. 7, 8, 9, 10. In the curves, the diurnal and semidiurnal variation only have been taken into account. It is seen how at Bombay, Lisbon, and Greenwich the observed curves are almost identical in shape with the curves calculated on the hypothesis of an external cause, if the range is reduced in a proper ratio. The disagreement with the curves calculated on the other alternative is complete, a maximum occurring where a minimum should occur, and *vice versâ*. At St. Petersburg, although the agreement is much worse, the 1878 curve, which has been corrected for temperature, follows pretty closely the shape of the calculated curve. The numbers from which the curves have been drawn are given in Table XIX.

Fig. 7.

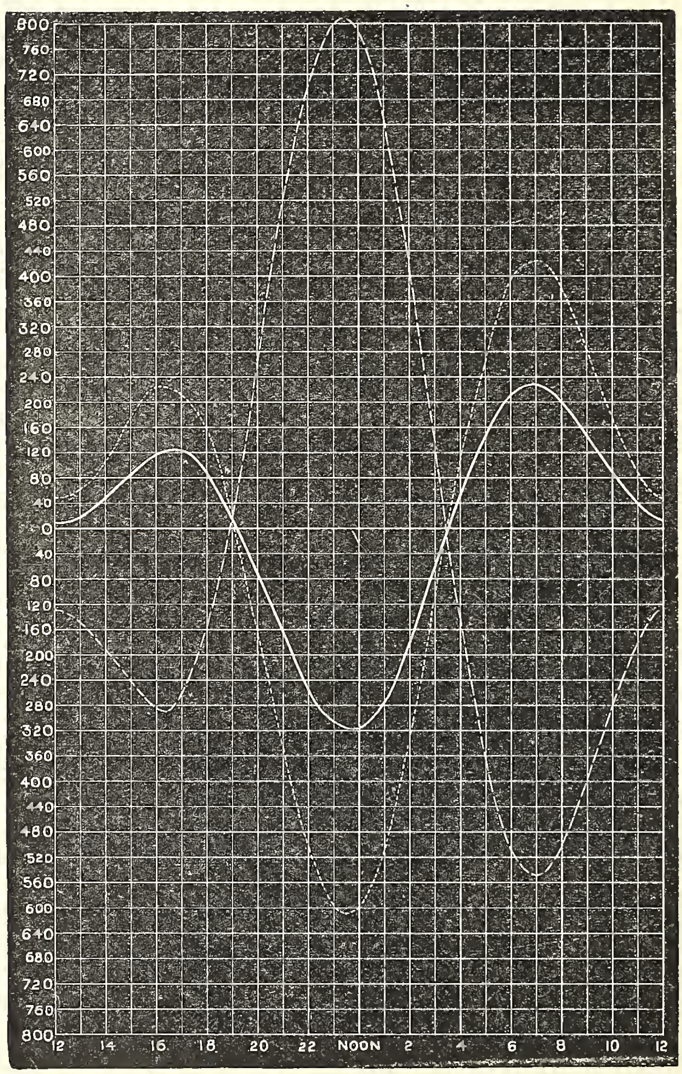


BOMBAY.

Comparison between calculated and observed curve of vertical force. The abscissæ denote astronomical time, the ordinates vertical force, the unit of force being C.G.S. 10^{-6}.

Observed curve, white line. Curve calculated on hypothesis of outside force, dotted line. Curve calculated on hypothesis of inside force, white and black line.

Fig. 8.

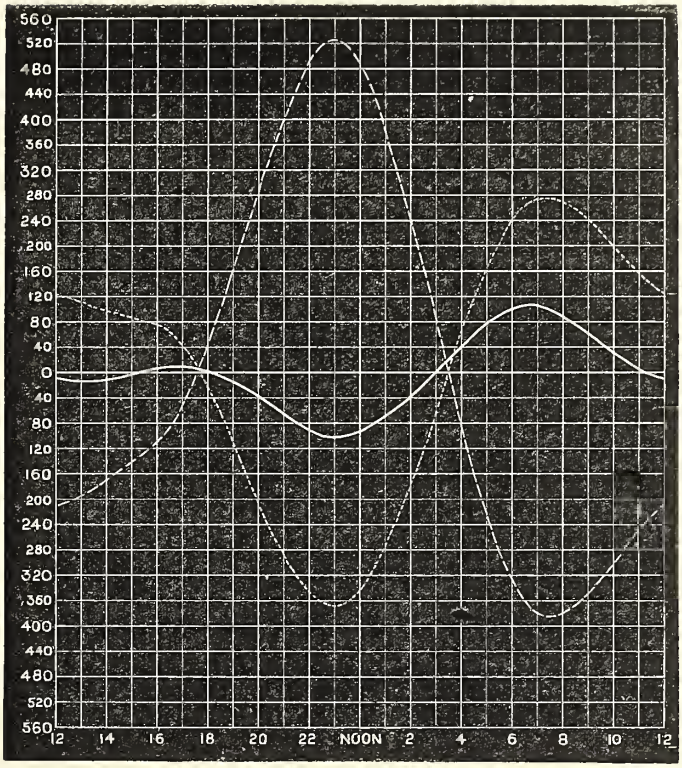


Lisbon.

Comparison between calculated and observed curve of vertical force. The abscissæ denote astronomical time, the ordinates vertical force, the unit of force being C.G.S. 10^{-6}.

Observed curve, white line. Curve calculated on hypothesis of outside force, dotted line. Curve calculated on hypothesis of inside force, white and black line.

Fig. 9.

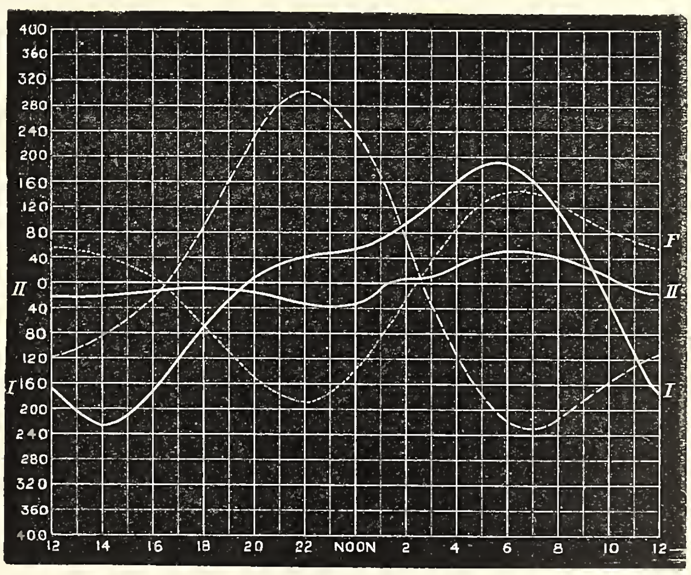


Greenwich.

Comparison between calculated and observed curve of vertical force. The abscissæ denote astronomical time, the ordinates vertical force, the unit of force being C.G.S. 10^{-6}.

Observed curve, white line. Curve calculated on hypothesis of outside force, dotted line. Curve calculated on hypothesis of inside force, white and black line.

Fig. 10.



St. Petersburg.

Comparison between calculated and observed curve of vertical force. The abscissæ denote astronomical time, the ordinates vertical force, the unit of force being C.G.S. 10^{-6}.

Observed curves, white line—I. for 1870, II. for 1878. Curve calculated on hypothesis of outside force, dotted line. Curve calculated on hypothesis of inside force, white and black line.

TABLE XIX.—Comparison between calculated and observed variation of vertical force. The unit of force is C.G.S. 10^{-6}. The first four terms of the series $a_1 \cos t + b_1 \sin t + a_2 \cos 2t + b_2 \sin 2t$ have been taken into account.

Astronomical time.	Bombay.			Lisbon.			Greenwich.			St. Petersburg.			
	Observed.	Calculated from outside.	Calculated from inside.	Observed.	Calculated from outside.	Calculated from inside.	Observed.	Calculated from outside.	Calculated from inside.	Observed 1870.	Calculated from outside.	Calculated from inside.	Observed 1878.
	a_1 −42	−141	+219	a_1 −165	−323	+458	a_1 −42	−235	+350	+114	− 97	+177	− 8
	b_1 + 9	+ 31	− 57	b_1 + 60	+122	−177	b_1 +49	+132	−190	+125	+104	−154	+29
	a_2 −21	− 53	+ 79	a_2 −152	−275	+329	a_2 −51	−110	+139	− 59	− 40	+ 62	−24
	b_2 +28	+121	−152	b_2 + 13	+ 34	− 53	b_2 + 2	+ 21	− 34	− 38	+ 35	− 45	− 2
Hour.													
0	−63·0	−194·0	+298·0	−317·0	−598·0	+787·0	− 93·0	−345·0	+489·0	+ 55·0	−137·0	+239·0	−32·0
1	−42·5	−113·6	+189·2	−269·0	−501·6	+655·0	− 71·1	−277·6	+392·3	+ 72·4	− 83·9	+162·3	−22·0
2	−18·2	− 28·3	+ 69·1	−177·6	−326·8	+426·7	− 35·7	−174·3	+248·2	+ 98·8	− 21·7	+ 68·3	− 6·1
3	+ 4·7	+ 43·2	− 37·5	− 61·3	−108·1	+145·7	+ 6·9	− 51·8	+ 79·2	+131·0	+ 39·9	− 28·8	+12·8
4	+21·5	+ 87·6	−111·0	+ 56·8	+111·1	−134·7	+ 48·6	+ 70·0	− 88·4	+161·9	+ 91·9	−114·9	+31·4
5	+30·0	+ 99·8	−142·8	+153·4	+289·4	−363·8	+ 81·6	+172·4	−230·2	+182·4	+127·5	−179·2	+45·7
6	+30·0	+ 84·0	−136·0	+212·0	+397·0	−506·0	+100·0	+242·0	−329·0	+184·0	+144·0	−216·0	+53·0
7	+23·8	+ 51·8	−104·2	+225·8	+422·8	−548·0	+101·4	+273·2	−377·6	+161·4	+142·7	−225·8	+51·9
8	+15·1	+ 19·0	− 66·8	+199·2	+375·3	−500·9	+ 87·2	+268·6	−379·6	+113·7	+128·3	−213·9	+42·8
9	+ 8·1	+ 0·6	− 43·1	+146·1	+280·7	−395·9	+ 62·3	+238·4	−347·8	+ 45·8	+107·1	−189·0	+28·2
10	+ 6·2	+ 6·3	− 47·1	+ 85·6	+173·8	−274·7	+ 33·7	+196·3	−299·2	− 32·8	+ 85·7	−160·3	+11·1
11	+10·7	+ 37·8	− 82·0	+ 36·8	+ 88·4	−176·8	+ 8·1	+155·4	−249·9	−109·8	+ 68·5	−134·7	− 4·6
12	+21·0	+ 88·0	−140·0	+ 13·0	+ 48·0	−129·0	− 9·0	+125·0	−211·0	−173·0	+ 57·0	−115·0	−16·0
13	+34·1	+142·8	−204·4	+ 18·8	+ 59·2	−138·2	− 15·3	+108·0	−185·5	−212·6	+ 49·7	− 99·9	−21·6
14	+45·6	+184·9	−253·3	+ 48·2	+110·6	−189·5	− 11·9	+100·7	−168·0	−223·6	+ 42·3	− 84·3	−21·3
15	+51·3	+198·8	−266·5	+ 87·3	+176·1	−251·7	− 2·9	+ 93·8	−147·2	−207·0	+ 30·1	− 61·2	−16·8
16	+47·9	+175·0	−231·2	+117·8	+222·7	−286·1	+ 5·8	+ 76·4	−109·4	−168·7	+ 8·7	− 25·1	−10·8
17	+34·4	+113·0	−146·0	+122·8	+221·0	−259·0	+ 8·8	+ 39·2	− 44·6	−118·2	− 23·3	+ 26·8	− 6·1
18	+12·0	+ 22·0	− 22·0	+ 92·0	+153·0	−152·0	+ 2·0	− 22·0	+ 51·0	− 66·0	− 64·0	+ 92·0	− 5·0
19	−15·4	− 81·0	+119·4	+ 24·4	+ 19·6	+ 31·2	− 15·0	−103·6	+170·8	− 21·2	−108·5	+163·4	− 8·3
20	−42·5	−175·6	+251·0	− 69·8	−159·1	+263·7	− 39·6	−195·0	+299·4	+ 11·1	−148·9	+229·9	−15·4
21	−64·1	−242·6	+347·1	−172·1	−348·7	+501·9	− 66·3	−280·4	+415·8	+ 30·2	−177·1	+279·0	−24·2
22	−75·6	−268·9	+389·3	−260·2	−507·6	+695·5	− 88·1	−342·7	+497·0	+ 39·6	−186·3	+300·3	−31·7
23	−75·1	−250·6	+370·8	−313·0	−598·8	+799·6	− 98·5	−367·0	+524·7	+ 45·6	−172·7	+287·1	−35·0

If we, then, take it as proved that the primary cause of the variation comes to us from outside the Earth's surface, we are led to consider that a varying magnetic potential must cause induced currents within the Earth, if that body is a sufficiently good conductor. These induced currents might be the cause of the apparent reduction in amplitude. As my colleague, Professor LAMB, had given considerable attention to the problem of currents inside a conducting sphere, I consulted him, and he gave me the formulæ by means of which the induced currents can be calculated. His investigation is added as an Appendix to this paper.

V. *Discussion of effects due to Currents Induced in the Inside of the Earth.*

I shall assume, then, for the present, that there is a periodic magnetic disturbance having its cause outside the Earth, and being probably due to electric currents in our atmosphere. Currents will be induced within the Earth, and we must now discuss what the effect of these currents will be, and whether they will account for the reduction in amplitude of the vertical forces which the observations show.

The varying potential can be expressed as a sum of terms of the form

$$\Omega_n \cos(pt + \lambda),$$

where Ω is a solid harmonic of degree n. Professor LAMB's formulæ allow us to calculate for each value of n, and for each value of p, the magnetic effect due to the induced currents, on the supposition that the specific resistance ρ of the Earth is uniform. The forces due to these currents will have a different amplitude and a different phase from the original forces, and it is the resultant effect which we observe in the diurnal variations. The general effect will be to increase the horizontal components and to diminish the vertical component. The difference of phase will be the same for all components, provided we give a different sign to the amplitude of the vertical components of the inside and outside currents respectively. Otherwise the difference of phase of the vertical component will be greater by two right angles than the difference of phase of the horizontal components.

If one of the horizontal forces and the vertical force due to the solid harmonic of positive degree n are written

$$a \cos(pt + \lambda) \quad \text{and} \quad b \cos(pt + \lambda + \epsilon),$$

the corresponding components due to the induced potential of negative degree will be of the form

$$c'a \cos(pt + \lambda + \alpha) \quad \text{and} \quad cb \cos(pt + \lambda + \epsilon + \alpha).$$

Table XX. gives the coefficients c', c, and α for given values of the specific resistance ρ, if $n = 2$. The value of δ has the same meaning as in Professor LAMB's paper, and is connected with ρ by the equation

$$\rho\delta = 4\pi p R^2$$

where p is equal to $2\pi m/\mathrm{T}$, and m is equal to 1 for the diurnal variation, and equal to 2 for the semidiurnal variation.

Table XXI. gives the same quantities for $n = 4$.

An example may render the use of the Table more intelligible. Let the Earth, for instance, have a uniform specific resistance, which in C.G.S. units is $1\cdot23 \times 10^{13}$, and consider that term of the magnetic potential which, on the surface of the Earth, has the form

$$\mathrm{A} \sin u \cos u \cos \frac{2\pi t}{\mathrm{T}},$$

where T is the time of the revolution of the Earth. This term alone represents fairly well the characteristic features of the diurnal variation. Here, as $n = 2$, we may use Table XX. The value of δ corresponding to the assumed resistance is 30, and we find $c = -\cdot5$ approximately, which means that the vertical force due to the induced currents has half the amplitude of the vertical force due to the primary currents; also, the difference in phase is 41°; as the sign of the amplitude is changed, this means that the *minimum* of the vertical force due to induced currents takes place not quite three hours after the *maximum* of the corresponding primary force. Similarly the horizontal force due to the inside current is only one-third of the corresponding horizontal force due to the outside potential, and here the *maximum* due to the secondary currents takes place nearly three hours after the *maximum* due to the secondary currents. To get similar numbers for the semidiurnal variation we should have to put $m = 2$, and find c, c', and α for $\delta = 60$, because $\rho = 1\cdot23 \times 10^{13} = 2 \times 6\cdot15 \times 10^{12}$, and looking up $6\cdot15 \times 10^{12}$ in the last column we should find the corresponding number in the first column to be 60.

TABLE XX.—Comparison of the magnetic forces due to a system of varying electric currents outside the Earth, with the forces due to the currents induced inside the Earth. $n = 2$.

δ.	c. Ratio of normal forces due to secondary and primary variation.	c'. Ratio of tangential forces due to secondary and primary variation.	α. Difference of phase.	Corresponding value of ρ.
			° ′	
1	$-\cdot02854$	$+\cdot01903$	87 27½	$3\cdot70 \times 10^{14} \times m$.
2	·05688	·03792	84 55½	$1\cdot85 \times 10^{14}$
3	·08484	·05656	82 25	$1\cdot23 \times 10^{14}$
4	·11226	·07484	79 57	$9\cdot25 \times 10^{13}$
5	·13895	·09263	77 32½	$7\cdot40 \times 10^{13}$
6	·16479	·10986	75 10½	$6\cdot17 \times 10^{13}$
7	·18968	·12645	72 53	$5\cdot29 \times 10^{13}$
8	·21354	·14236	70 41	$4\cdot63 \times 10^{13}$
9	·23633	·15755	68 32	$4\cdot11 \times 10^{13}$
10	·25799	·17199	66 29	$3\cdot70 \times 10^{13}$
20	·41771	·27847	50 32	$1\cdot85 \times 10^{13}$
30	·50520	·33680	41 03	$1\cdot23 \times 10^{13}$
40	·56158	·37439	35 11	$9\cdot25 \times 10^{12}$
50	·59864	·39909	31 11	$7\cdot40 \times 10^{12}$
100	·69949	·46632	21 33	$3\cdot70 \times 10^{12}$

TABLE XXI.—Comparison of the magnetic force due to a varying potential represented by a solid harmonic of degree 4 with the corresponding forces due to currents induced inside the Earth.

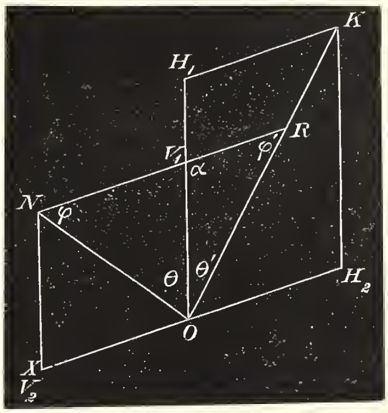
δ.	c. Ratio of normal forces due to secondary and primary variation.	c'. Ratio of tangential forces due to secondary and primary variation.	α. Difference of phase.	Corresponding value of ρ.
			° ′	
1	− ·0101	+ ·0081	89 01	$3{\cdot}70 \times 10^{14} \times m$.
10	·0992	·0794	80 21	$3{\cdot}70 \times 10^{13}$
100	·5149	·4119	37 50	$3{\cdot}70 \times 10^{12}$

Tables XX. and XXI. cannot, however, be used to compare our calculated and observed results, but form only an intermediate step.

We observe on the Earth the resultant of the outside and inside effect, and we have calculated the vertical force on the assumption that the whole horizontal force is due to outside effect.

In fig. 11, let OH_1 represent that part of the horizontal force which is due to the

Fig. 11.



outside effect, and OH_2 the corresponding force in phase and amplitude which is due to the induced effect. The observed horizontal force will be the resultant OK. Let OV_1 be the magnitude of the vertical force due to the outside currents, and OV_2 the vertical force in phase and amplitude due to the induced effect. The observed vertical force will be ON. If we calculate the vertical force on the assumption that the resultant OK is due entirely to the outside, we should obtain a force OR, such that $OR : OK :: OV_1 : OH_1$. Our calculated value of vertical force will be OR, and our observed value ON. We require the ratio of lengths of these lines and the angle between them.

In the triangle ONR we know, from Table XX., the ratios

$$V_1R : OV_1 = H_1K : OH_1 = OH_2 : OH_1 = c',$$
$$V_1N : OV_1 = OV_2 : OV_1 = c,$$

also the angle

$$NV_1O = V_1OH_2 = \alpha.$$

Write

$$NOV_1 = \theta;\ ROV_1 = \theta';\ ONV_1 = \phi;\ ORV_1 = \phi'.$$

The triangles OV_1N and OV_1R give us the equations

$$\tan \tfrac{1}{2}(\phi - \theta) = \frac{1-c}{1+c}\cot\frac{\alpha}{2},$$

$$\tan \tfrac{1}{2}(\phi' - \theta') = \frac{1-c'}{1+c'}\tan\frac{\alpha}{2},$$

$$(\phi + \theta) = \pi - \alpha,$$

$$(\phi' + \theta') = \alpha.$$

These equations determine θ and θ', and hence the required angle $\gamma = \theta + \theta'$; also OR : ON = $\sin\phi : \sin\phi'$.

In Table XXII. the angle γ and the ratio ON : OR : $= r$ have been calculated for $n = 2$. Table XXIII. gives the corresponding quantities if the inducing solid harmonic is of degree 4.

TABLE XXII.—Comparison between resultant vertical force as regards magnitude and phase when induced currents are taken into account and vertical force calculated on the assumption that the whole is due to an outside effect. The inducing potential is a solid harmonic of degree 2.

c.	Reduction in amplitude. r.	Change of phase. γ.	ρ.
1	·9981	2° 43′	$3{\cdot}70 \times 10^{14}$
5	·9565	13 02	$7{\cdot}40 \times 10^{13}$
10	·8589	23 10	$3{\cdot}70 \times 10^{13}$
20	·6705	34 03	$1{\cdot}85 \times 10^{13}$
30	·5516	38 12	$1{\cdot}23 \times 10^{13}$
40	·4762	40 16	$9{\cdot}25 \times 10^{12}$
50	·4261	41 10	$7{\cdot}40 \times 10^{12}$
100	·3004	43 08	$3{\cdot}70 \times 10^{12}$

TABLE XXIII.—Comparison between resultant vertical force, as regards magnitude and direction when induced currents are taken into account, and vertical force calculated on the assumption that the whole is due to an outside effect. The inducing potential is a solid harmonic of degree 4.

δ.	Reduction in amplitude.	Change of phase.	ρ.
		° ′	
1	·9997	1 06	$3{\cdot}70 \times 10^{14}$
10	·9723	10 06	$3{\cdot}70 \times 10^{13}$
100	·4982	38 49	$3{\cdot}70 \times 10^{12}$

The observed amplitude of the vertical force at Lisbon is about one half of its calculated value. If the conductivity of the Earth was such as to produce this reduction in amplitude, it is seen from Tables XXII. and XXIII. that the phase would be altered about 40°, while in reality there is a remarkable agreement in phase. If the conductivity is so small as to leave the resultant phase practically unaltered, as observation tends to show, the amplitude also should not be sensibly altered. There is, therefore, no uniform conductivity of the Earth which can make the observations agree with the calculation. Such an agreement, however, can be easily brought about, as Professor LAMB has suggested to me, if the conductivity of the inside of the Earth is larger than the conductivity of the upper layers. It is extremely probable that this is really the case. The bulk of the outside layer of the Earth, except in so far as it is water, is made up of material which in its ordinary condition is non-conducting; but we know that some of the silicates begin to conduct at temperatures above 200° C., and, generally speaking, insulators lose their insulating powers at high temperatures. Without regard even to the quantities of metallic matter that may be stored inside the Earth, there is nothing improbable in the supposition that its conductivity increases towards the inside. If the bulk of the observed induced effect is due to currents in a fairly conducting inner sphere, the calculated phase would be that due to good conducting matter, and would not differ from the observed value, while the reduction in amplitude might yet be sufficient to account for the observed facts. In order to give a better idea of the kind of conductivity which is required to produce a certain change of phase, it may be stated that for the purest distilled water obtained by KOHLRAUSCH ρ would be about $1{\cdot}4 \times 10^{15}$. Such water is, as is well known, a very bad conductor, and, according to our Tables, if the whole Earth was made up of matter which conducts as badly, there would be no currents in the Earth induced by the diurnal variation of sufficient intensity to affect our magnetic needle sensibly. Ordinary rain water, however, has a specific resistance of about 6×10^{13}. A conducting sphere of the same resistance would already produce a retardation in phase of about an hour for the diurnal variation if the solid harmonic is of degree 2. For salt water the resistance may get as low as 4×10^9. A whole sphere made up of such water would very considerably reduce the amplitude of the observed vertical force, and alter the resultant phase by 45° nearly. The average conductivity of the Earth, as seen from these examples, must be small, although it may be considerable over limited areas. Such limited areas would principally affect the harmonic terms of higher degrees, and we should not consequently expect for them such a good agreement between theory and observation. Table XXII. shows that as the resistance of the sphere diminishes the retardation of the resultant phase seems to approach a constant value of 45°. This can be proved to be quite generally the case. It follows directly from a formula given by Professor LAMB in the Appendix, and can also be seen as follows:—When the conductivity is good the angle between OH_2 and OH_1 in fig. 11 will steadily diminish, and ultimately vanish. OK will ultimately coincide with OH_1, and OR with OV_1; but the angle between OV_2 and OV_1 will increase towards 180°, and the sides will tend towards equality. Two very nearly equal and very nearly opposite forces may have a resultant which is inclined by a finite angle to the forces. To find the angle in the limit between OV_1 and OV_2 we deduce, in the first place, an expression for the ratio of the vertical force due to the outside effect to the vertical force due to induced currents for good conductivities.

This ratio depends, as shown by Professor LAMB, on the function

$$\chi_n(\kappa R) \quad \text{where} \quad \kappa^2 = \frac{4\pi p i}{\rho},$$

and

$$\chi_n(\zeta) = 3\cdot 5 \ldots (2n+1)\left(\frac{d}{\zeta d\zeta}\right)^n \frac{\sinh\zeta}{\zeta}.$$

If n is odd,

$$\left(\frac{d}{\zeta d\zeta}\right)^n \sinh\zeta = \frac{\cosh\zeta}{\zeta^{n+1}} - \frac{n.n+1}{2}\frac{\sinh\zeta}{\zeta^{n+2}} + \frac{n-1.n.n+1.n+2}{2.4}\frac{\cosh\zeta}{\zeta^{n+3}} - \frac{(n-2)\ldots(n+3)}{2.4.6}\frac{\sinh\zeta}{\zeta^{n+4}}.$$

If n is even, we must interchange $\sinh\zeta$ and $\cosh\zeta$.

If ζ is larger, so that $e^{-\zeta}$ can be neglected, compared to e^{ζ}, $\cosh\zeta = \sinh\zeta = e^{\zeta}$, and

$$\frac{\kappa^2 R^2}{2n+1.(2n+3)}\frac{\chi_{n+1}(\kappa R)}{\chi_{(n-1)}(\kappa R)}$$

which is the ratio of the vertical forces due to the induced and inducing potential will become

$$\frac{1 - \frac{n+1.n+2}{2}\zeta^{-1} + \frac{n.n+1.n+2.n+3}{2.4}\zeta^{-2} - \ldots}{1 - \frac{n-1.n}{2}\zeta^{-1} + \frac{n-2.n-1.n.n+1}{2.4}\zeta^{-2} - \ldots},$$

where

$$\zeta^2 = \frac{4\pi p}{\rho} R^2 i = i\delta \,,$$

$$\zeta^{-2} = -\frac{i}{\delta}, \text{ and } \zeta^{-1} = \pm \frac{\delta^{-\frac{1}{2}}}{\sqrt{2}} (1 - i) = \delta^{-\frac{1}{2}} \left(\cos\frac{\pi}{4} - i \sin\frac{\pi}{4}\right).$$

Generally in the above expansion we may put therefore

$$\zeta^{-q} = \delta^{-q/2} \left(\cos\frac{\pi q}{4} - i \sin\frac{\pi q}{4}\right);$$

so that the ratio of the vertical forces will be

$$\frac{X + iY}{X' + iY'},$$

where

$$X = 1 - \frac{n+1.n+2}{2} \cos\frac{\pi}{4} \quad \delta^{-\frac{1}{2}} + \frac{n.n+1.n+2.n+3}{2.4} \cos\frac{2\pi}{4} \quad \delta^{-1}$$
$$- \frac{(n-1)\ldots(n+4)}{2.4.6} \cos\frac{3\pi}{4} \quad \delta^{-\frac{3}{2}},$$

$$Y = \frac{n+1.n+2}{2} \sin\frac{\pi}{4} \quad \delta^{-\frac{1}{2}} - \frac{n.n+1.n+2.n+3}{2.4} \sin\frac{2\pi}{4} \quad \delta^{-1}$$
$$+ \frac{(n-1)\ldots(n+4)}{2.4.6} \sin\frac{3\pi}{4} \quad \delta^{-\frac{3}{2}},$$

$$X' = 1 - \frac{n-1.n}{2} \cos\frac{\pi}{4} \quad \delta^{-\frac{1}{2}} + \frac{n-2.n-1.n.n+1}{2.4} \cos\frac{2\pi}{4} \quad \delta^{-1} - \ldots,$$

$$Y' = \frac{n-1.n}{2} \sin\frac{\pi}{4} \quad \delta^{-\frac{1}{2}} - \frac{n-2.n-1.n.n+1}{2.4} \sin\frac{2\pi}{4} \quad \delta^{-1} + \ldots$$

The ratio of the amplitudes, if δ is large, becomes

$$\frac{\sqrt{(X^2 + Y^2)}}{\sqrt{(X'^2 + Y'^2)}} = 1 - (2n+1)(2\delta)^{-\frac{1}{2}},$$

and the angle between the two vertical forces

$$\tan^{-1}\frac{Y}{X} - \tan^{-1}\frac{Y'}{X'} = (2n+1)(2\delta)^{-\frac{1}{2}}.$$

The resultant of these two forces, resolved in a direction parallel to either of them, is therefore equal to the component which is at right angles, and the resultant will consequently be at an inclination of 45°.

The result will not hold, of course, if conduction in the Earth's crust takes place chiefly at some distance below the surface, as in that case the vertical force due to the induced currents will not tend to become equal to the vertical force due to the primary variation. For a bad conductivity we shall always have a resultant vertical force sensibly equal to the primary force. If the conductivity increases, the resultant will have a different phase from the primary variation, tending towards a difference of 45°, if the conductivity is uniform. If the conductivity is not uniform, a maximum difference of phase will be reached, which, if the conductivity is still further increased, diminishes indefinitely.

VI. *The Magnetic Potential on the Surface of the Earth.*

As it seemed interesting to trace the equipotential lines on the surface of the Earth as far as they depend on the diurnal variation, I have calculated the potential from the equations [A] and [B].

It is necessary, for this purpose, to compute the tesseral harmonics for definite points on the Earth's surface. It would seem most natural to choose these points, so that they lie on equidistant circles of latitudes, but as tables* exist for the zonal harmonics in terms of the *cosine* of the colatitude, I have selected values of these cosines so that the corresponding angles should differ as nearly as possible by 10°.

The values of u and $\mu = \cos u$, for which the potential is computed on the Northern hemisphere, are given in Table XXIV., u being the colatitude.

TABLE XXIV.

$\mu = \cos u =$	·98	·94	·87	·77	·64	·50	·34	·17	·00
$u =$	11° 29′	19° 57′	29° 32′	39° 39′	50° 12′	60° 00′	70° 07′	80° 13′	90° 00′

Symmetrical circles of latitude were taken on the Southern hemisphere.

Taking from Mr. GLAISHER's Table the values of P_i corresponding to each of the above values of μ, we obtain the differential coefficients of zonal harmonics by a successive application of the formula

$$\frac{dP_i}{d\mu} - \frac{dP_{i-2}}{d\mu} = (2i - 1)\, P_{i-1}.$$

The first and second differential coefficients thus calculated are given in Tables XXV. and XXVI.

* 'Report of the British Association' (Sheffield, 1879).

TABLE XXV.—Values of $\frac{\partial P_i}{\partial \mu}$ where P_i is the zonal harmonic of degree i and μ the cosine of the colatitude.

$\mu =$	1·00.	0·98.	0·94.	0·87.	0·77.	0·64.	0·50.	0·34.	0·17.	0·00.
$\frac{dP_1}{d\mu}$	+ 1·000	+ 1·000	+ 1·000	+ 1·000	+ 1·000	+ 1·000	+ 1·000	+ 1·000	+ 1·000	+ 1·000
$\frac{dP_2}{d\mu}$	+ 3·000	+ 2·940	+ 2·820	+ 2·610	+ 2 310	+ 1·920	+ 1·500	+ 1·020	+ 0·510	0·000
$\frac{dP_3}{d\mu}$	+ 6·000	+ 5·705	+ 5·125	+ 4·175	+ 2·945	+ 1·570	+ 0·375	+ 0·635	− 1·285	− 1·500
$\frac{dP_4}{d\mu}$	+ 10·000	+ 9·121	+ 7·482	+ 4·997	+ 2·212	− 0·215	− 1·559	− 1·864	− 1·191	0·000
$\frac{dP_5}{d\mu}$	+ 15·000	+ 12·986	+ 9·418	+ 4·562	+ 0·155	− 2·273	− 2·226	− 0·635	+ 1·145	+ 1·875
$\frac{dP_6}{d\mu}$	+ 21·000	+ 17·041	+ 10·496	+ 2·731	− 2·397	− 2·943	− 0·569	+ 1·755	+ 1·856	0·000
$\frac{dP_7}{d\mu}$	+ 28·000	+ 21·046	+ 10·393	− 0·170	− 4·174	− 1·428	+ 1·973	+ 2·030	− 0·662	− 2·181

TABLE XXVI.—Values of $\frac{\partial^2 P_i}{\partial \mu^2}$ where P_i is the zonal harmonic of degree i and μ the colatitude.

$\mu =$	1·00.	0·98	0·94.	0·87.	0·77.	0·64.	0·50.	9·34.	0·17.	0·00.
$\frac{d_2 P_2}{d\mu_2}$	+ 3·000	+ 3·000	+ 3·000	+ 3·000	+ 3·000	+ 3·000	+ 3·000	+ 3·000	+ 3·000	+ 3·000
$\frac{d_2 P_3}{d\mu_2}$	+ 15·000	+ 14·700	+ 14·100	+ 13·050	+ 11·550	+ 9·600	+ 7·500	+ 5·100	+ 2·550	0·000
$\frac{d_2 P_4}{d\mu_2}$	+ 45·000	+ 42·935	+ 38·875	+ 32·225	+ 23·615	+ 13·990	+ 5·525	− 1·445	− 5·995	− 7·500
$\frac{d_2 P_5}{d\mu_2}$	+ 105·000	+ 96·789	+ 81·438	+ 58·023	+ 31·458	+ 7·665	− 6·531	− 11·676	− 8·269	0·000
$\frac{d_2 P_6}{d\mu_2}$	+ 210·000	+ 185·781	+ 142·473	+ 82·407	+ 25·320	− 11·013	− 18·861	− 8·430	+ 6·600	+ 13·125
$\frac{d_2 P_7}{d\mu_2}$	+ 378·000	+ 318·322	+ 217·886	+ 93·526	+ 0·297	− 30·594	− 13·928	+ 11·139	+ 15·859	0·000
$\frac{d_2 P_8}{d\mu_2}$	+ 630·000	+ 501·471	+ 298·368	+ 79·857	− 37·290	− 32·433	+ 10·734	+ 22·020	− 3·330	− 19·590

Introducing these quantities in the equations for the potential, and taking proper account of the change of sign of μ in the Southern hemisphere, I have obtained Table XXVII., in which the potentials are given for 24 equidistant meridian circles. In order to reproduce the daily variations, we must imagine the whole system of equipotential lines to revolve round the Earth from East to West; the time for which the potential is given is mean noon for the zero meridian. It will be remembered that the equations for the potential have been derived from the mean summer values in the Northern, and mean winter values in the Southern hemisphere. If we want to get a symmetrical potential in both hemispheres, we must take the average variation for the whole year, or, what comes to the same thing, we may in Table XXVII. write down the mean values for two corresponding circles of latitude, one in each hemisphere. This has been done in Table XXVIII., where the values are only given for the Northern hemisphere. The mean equipotential lines for the year are drawn in fig. 12. If we imagine the variable part of the magnetic force to be produced by a system of surface currents in a conducting sphere concentric with the Earth, and surrounding it, we may, if the potential is known, calculate the distribution of the lines of flow.

If the magnetic surface potential is of the form Ω_n, when Ω_n is a harmonic of degree n, the current function ϕ_n is given by

$$4\pi\phi_n = -\frac{2n+1}{n+1}\Omega_n,^*$$

so that the lines of flow are the same as the equipotential lines. This is no longer true when the magnetic potential is made up of a number of terms corresponding to harmonics of different degrees, for the factor $(2n+1)/(n+1)$ will vary for different terms, and the resultant current function will therefore no longer be proportional to the resultant magnetic potential.

In our own case, taking the mean values for the whole year, the series begins with Ω_2, and the factor $(2n+1)/(n+1)$ will vary, therefore, only between 5/3 and 2. We may then, as an approximation, still take the equipotential lines to give us the general form of the lines of flow. We conclude that we may imagine the daily variation of the Earth's magnetic force to be produced by a system of electric currents in a sphere surrounding the Earth, in which the lines of flow are roughly represented in fig. 12, the direction being such that at longitude 60° East the flow is away from the equator.

* MAXWELL, 'Electricity and Magnetism,' vol. 2, p. 281.

TABLE XXVII.—Values of the variable part of the magnetic potential on the cosine of the colatitude) and 24 equidistant meridian circles, reckoned from is that of the mean summer months. The time is Greenwich noon.

$\mu = \cos u$.	$\lambda = 0$.	15.	30.	45.	60.	75.	90.	105.	120.	135.	150.
+ ·98	− 21·7	− 15·8	− 9·5	− 3·3	+ 1·6	+ 5·4	+ 7·9	+ 9·8	+ 11·6	+ 13·5	+ 15·9
+ ·94	− 61·8	− 46·6	− 28·6	−10·6	+ 5·1	+17·1	+ 25·4	+ 30 7	+ 34·1	+ 37·4	+ 41·0
+ ·87	−135·2	−106·9	− 69·5	−28·8	+ 9·1	+40·3	+ 62·3	+ 75·3	+ 81·3	+ 83·2	+ 84·1
+ ·77	−212·4	−171·6	−113·3	−47·0	+17·0	+70·3	+107·6	+127·7	+133·4	+129·6	+122·5
+ ·64	−247·3	−197·5	−126·4	−45·9	+30·7	+93·0	+134·2	+153·2	+153·7	+143·1	+129·6
+ ·50	−221·2	−167·3	− 96·3	−21·2	+46·1	+96·3	+125·3	+133·7	+127·3	+114·6	+103·3
+ ·34	−158·1	−105·7	− 45·3	+11·8	+56·3	+83·4	+ 92·5	+ 88·0	+ 76·9	+ 67·0	+ 64·1
+ ·17	− 93·5	− 49·5	− 5·1	+31·2	+54·2	+62·4	+ 58·2	+ 47·2	+ 36·0	+ 30·8	+ 34·7
00	− 42·4	− 13·4	+ 13·1	+32·2	+41·6	+41·5	+ 34·8	+ 24·9	+ 17·0	+ 14·4	+ 18·5
− ·17	+ 6·4	+ 17·1	+ 24·1	+26·8	+24·7	+19·6	+ 13·0	+ 6·8	+ 2·9	+ 1·4	+ 2·5
− ·34	+ 62·3	+ 56·2	+ 43·3	+26·3	+ 8·4	− 7·1	− 18·5	− 24·5	− 26·0	− 24·5	− 22·3
− ·50	+111·2	+ 94·3	+ 65·6	+30·4	− 5·2	−35·6	− 56·7	− 67·1	− 67·2	− 61·0	− 52·2
− ·64	+129·2	+107·8	+ 72·3	+28·1	−17·3	−56·7	− 84·9	− 98·7	− 98·9	− 88·7	− 73·1
− ·77	+106·8	+ 85·4	+ 52·2	+11·7	−29·7	−65·9	− 92·1	−105·1	−104·9	− 93·9	− 76·2
− ·87	+ 63·0	+ 44·2	+ 19·1	− 9·2	−37·2	−61·1	− 78·0	− 85·9	− 84·8	− 76·5	− 61·7
− ·94	+ 26·3	+ 12·1	− 3·8	−20·1	−35·1	−47·1	− 55·0	− 58·1	− 56·3	− 50·1	− 40·4
− ·98	+ 8·6	− 0·2	− 9·2	−17·5	−24·6	−29·9	− 33·1	− 33·7	− 32·0	− 28·1	− 22·2

TABLE XXVIII.—Values of the variable part of the magnetic potential on the cosine of the colatitude) and 24 equidistant meridian circles, reckoned from is that corresponding to the mean values for the year. The time is Green-

$\mu = \cos u$.	$\lambda = 0$.	15.	30.	45.	60.	75.	90.	105.	120.	135.	150.
·98	− 15·1	− 7·8	− 0·1	+ 7·1	+13·1	+17·6	+ 20·5	+ 21·7	+ 21·8	+ 20·8	+ 19·0
·94	− 44·0	− 29·3	− 12·4	+ 4·7	+20·1	+32·1	+ 40·2	+ 44·4	+ 45·2	+ 43·7	+ 40·7
·87	− 99·1	− 75·5	− 44·3	− 9·8	+23·1	+50·7	+ 70·1	+ 80·6	+ 83·0	+ 79·8	+ 72·9
·77	−159·6	−128·5	− 82·7	−29·3	+23·3	+68·1	+ 99·8	+116·4	+119·1	+111·7	+ 99·3
·64	−188·2	−152·6	− 99·3	−37·0	+24·0	+74·8	+109·5	+125·9	+126·3	+115·9	+101·3
·50	−166·2	−130·8	− 80·9	−25·8	+25·6	+65·9	+ 91·0	+100·4	+ 97·2	+ 87·8	+ 77·7
·34	−110·2	− 80·9	− 44·3	− 7·2	+23·9	+45·2	+ 55·5	+ 56·2	+ 51·4	+ 45·7	+ 43·2
·17	− 49·9	− 33·3	− 14·6	+ 2·2	+14·7	+21·4	+ 22·6	+ 20·2	+ 16·5	+ 14·7	+ 16·1
·00	00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0

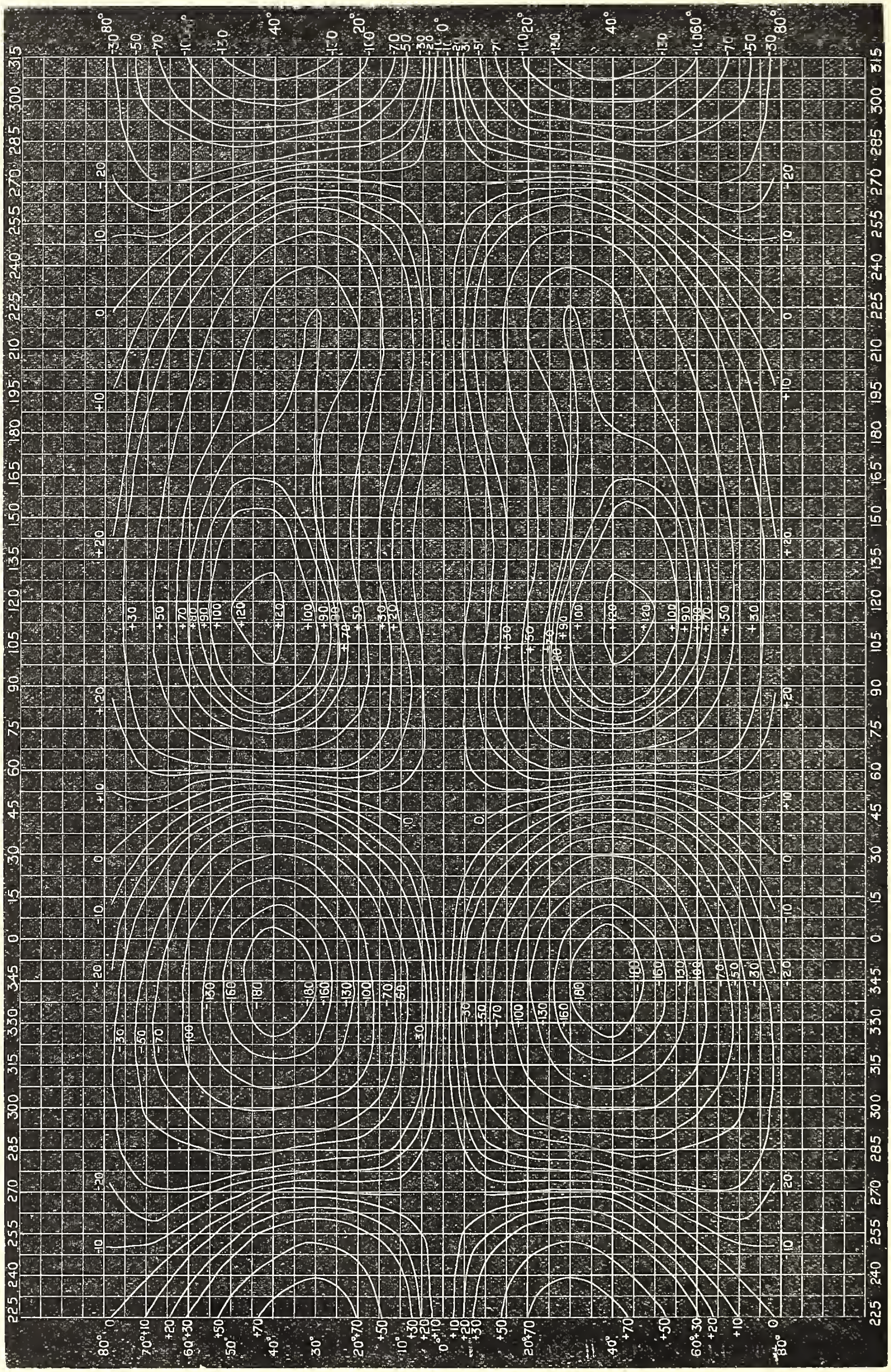
Earth's surface for 17 latitude circles corresponding to different values of μ (the Greenwich towards the East. The period of the year to which the Table refers

165.	180.	195.	210.	225.	240.	255.	270.	285.	300.	315.	330.	345.
+ 18·4	+ 20·3	+ 21·0	+ 19·7	+ 16·1	+10·2	+ 2·2	− 6·5	−15·0	− 21·8	− 26·3	− 27·7	− 26·0
+ 45·2	+ 48·8	+ 50·4	+ 48·4	+ 40·8	+27·5	+ 9·1	−12·4	−34·5	− 53·9	− 67·6	− 73·6	− 71·4
+ 85·7	+ 87·6	+ 98·3	+ 85·4	+ 74·2	+54·1	+23·0	−14·7	−56·7	− 96·9	−128·6	−147·3	−149·5
+116·6	+114·0	+113·4	+110·9	+101·0	+79·0	+41·9	− 9·2	−69·5	−131·0	−183·6	−218·5	−228·8
+119·7	+116·3	+118·2	+119·8	+113·9	+93·5	+54·4	− 3·2	−73·8	−147·1	−211·1	−253·8	−267·1
+ 98·9	+102·6	+111·1	+118·0	+114·2	+93·7	+53·7	− 6·7	−77·5	−148·7	−207·6	−243·2	−247·9
+ 70·7	+ 84·5	+ 99·9	+109·1	+104·4	+81·1	+38·4	−18·9	−82·2	−140·7	−183·2	−201·5	−192·5
+ 47·5	+ 64·9	+ 80·9	+ 88·3	+ 81·2	+57·4	+18·6	−29·6	−78·6	−119·2	−143·2	−146·3	−128·5
+ 27·8	+ 39·4	+ 48·6	+ 50·9	+ 43·4	+25·2	− 1·1	−31·5	−60·1	− 81·0	− 90·0	− 85·3	− 68·2
+ 5·1	+ 7·4	+ 7·9	+ 5·1	− 0·8	− 9·1	−18·6	−26·8	−31·8	− 32·1	− 27·4	− 18·1	− 6·1
− 21·4	− 22·7	− 26·6	− 31·5	− 35·5	−36·2	−31·7	−21·1	− 5·1	+ 14·2	+ 33·7	+ 50·1	+ 60·2
− 44·7	− 41·0	− 41·5	− 44·6	− 46·6	−43·8	−33·2	−13·5	+14·2	+ 46·2	+ 77·2	+101·2	+113·5
− 57·2	− 45·0	− 38·2	− 35·7	− 34·5	−30·1	−19·3	+ 0·7	+29·1	+ 62·3	+ 95·1	+120·5	+133·2
− 56·6	− 38·8	− 25·4	− 16·8	− 9·1	− 1·9	+ 8·5	+24·1	+44·9	+ 68·5	+ 91·3	+107·8	+114·0
− 44·8	− 27·6	− 12·2	+ 0·9	+ 12·0	+22·0	+31·9	+42·6	+53·9	+ 64·8	+ 73·2	+ 76·8	+ 74·0
− 28·7	− 15·7	− 2·7	+ 9·4	+ 20·5	+29·9	+38·1	+44·4	+48·7	+ 50·7	+ 49·7	+ 45·6	+ 37·7
− 15·0	− 6·8	+ 1·4	+ 9·6	+ 17·1	+23·4	+28·3	+31·3	+32·5	+ 31·6	+ 28·5	+ 23·4	+ 16·6

Earth's surface for 17 latitude circles corresponding to different values of μ (the Greenwich towards the East. The period of the year to which the Table refers wich noon.

165.	180.	195.	210.	225.	240.	255.	270.	285.	300.	315.	330.	345.
+16·7	+13·5	+ 9·8	+ 5·0	− 0·5	− 6·6	−13·0	−18·9	−23·7	− 26·7	− 27·4	− 25·5	− 21·3
+36·9	+32·2	+26·5	+19·5	+10·1	− 1·2	−14·5	−28·4	−41·6	− 52·3	− 58·6	− 59·6	− 54·5
+65·2	+57·6	+55·2	+42·2	+31·1	+16·0	− 4·4	−28·6	−55·3	− 80·8	−100·9	−112·0	−111·7
+86·6	+76·4	+69·4	+63·8	+55·0	+40·4	+16·7	−16·6	−57·2	− 99·7	−137·4	−163·1	−171·4
+88·4	+80·6	+78·2	+77·7	+74·2	+61·8	+36·8	− 1·9	−51·4	−104·7	−153·1	−187·1	−200·1
+71·8	+71·8	+76·3	+81·3	+80·4	+68·7	+43·4	+ 3·4	−45·8	− 97·4	−142·4	−172·2	−180·7
+46·0	+53·6	+63·2	+70·3	+69·9	+58·6	+35·0	+ 1·1	−38·5	− 77·4	−108·4	−125·8	−126·3
+21·2	+28·7	+36·5	+41·6	+41·0	+33·2	+18·6	− 1·4	−23·4	− 43·5	− 57·9	− 64·1	− 61·2
00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0	00·0

Fig. 12.


315 300 285 270 255 240 225 210 195 180 165 150 135 120 105 90 75 60 45 30 15 0 345 330 315 300 285 270 255 240 225
80° 70° 60° 50° 40° 30° 20° 10° 0° 10° 20° 30° 40° 50° 60° 70° 80°

VII. *Concluding Remarks.*

FARADAY, in the year 1850, discussed the diurnal variation of the magnetic needle. He showed that the changes which took place during daytime could be accounted for by supposing two magnetic poles—namely, a North pole in the Southern hemisphere, and a South pole in the Northern hemisphere—to be carried round with the Sun in our atmosphere. A glance at fig. 13 will show that our result entirely agrees with FARADAY'S. The proof that the principal part of the Earth's magnetism is due to causes outside its surface would have been almost as complete in the year 1850 as it is now, if FARADAY had added the remark that, if all three components of the variation can be completely accounted for by hypothetical changes taking place outside the Earth's surface, they *cannot* be accounted for by changes taking place in the inside.

I cannot agree, however, with FARADAY in the explanation which he gives of the variation. He imagines that the solar radiation, heating up the air, produces a sufficient change in its magnetic permeability to account for the observed deflection of the lines of magnetic force.

The magnetic susceptibility of oxygen at the atmospheric pressure and temperature is about $\cdot 5, 10^{-7}$, and for air it is smaller still. This would give the magnetic permeability as 1·0000006. If the air was entirely removed the change of magnetic force would be so small that we could not detect it. I have tried in various ways to find how a partial removal of the atmosphere as a magnetic medium could affect the needle in any appreciable way, but have failed to do so. FARADAY suggests that the oxygen in the higher regions of the atmosphere might, owing to the greater cold, be much more magnetic than what we observe it to be. But, on the other hand, owing the smaller density, the permeability would be diminished; so that I do not think we are at present justified in ascribing any material part of the daily variation to a change of the magnetic permeability of air due to the heating effect of the Sun. The effect of the Moon suggests a tidal action as the cause, and we may inquire whether such a tidal action could produce the observed effects. The late Professor BALFOUR STEWART has suggested that the Earth's magnetic force might induce electric currents in the convection currents which flow in the upper regions of the atmosphere. One difficulty of this hypothesis was removed by an experimental investigation, by means of which I have proved that the air can be thrown into a sensitive state in which small electromotive forces will produce sensible electric currents. To bring the air into that sensitive state it is only necessary to send an electric current through it from some independent source of high potential. It is very likely that the air in the upper regions of our atmosphere is in such a sensitive state, and it is quite possible, therefore, that the induced electric currents suggested by Professor BALFOUR STEWART really exist.

The symmetry of the diurnal variation in both hemispheres shows that, if it is due to the assumed cause, the vertical component of the magnetic force is the important factor, as that component changes sign on crossing the magnetic equator. In order that electric currents should be induced which could account for the observed movement of the magnetic needle, it is only necessary to imagine convection currents in the upper regions from East to West during certain parts of the day, and from West to East at other times. Judging from the analogy of the theory of waves in shallow water, a horizontal motion of considerable velocity might be produced by a tidal action due to solar and lunar attraction. It is true that no periodic change of the barometer has been traced with certainty to a tidal action; but I suppose that a tidal wave must nevertheless exist, and that its horizontal flow might be considerable, while the changes of pressure might escape our attention. As regards the effect of the Sun we have, indeed, a daily period of the barometer which is probably due to thermal effects. It is curious and suggestive that the horizontal motion which must accompany the change in pressure is just such as would account for the daily variation of the magnetic needle. In the tropics the principal minimum of the barometer takes place about 3.40 o'clock in the afternoon, and the principal maximum about 9 o'clock in the morning. According to the theory of waves, there would be a horizontal movement from West to East in the afternoon, and from East to West in the morning. The direction of the induced electric currents would be away from the equator in both hemispheres in the afternoon, and towards the equator in the morning. This is exactly the system of currents we have been led to, starting from the observed magnetic variation. The only difficulty I feel in suggesting that the cause of the diurnal variation of the magnetic needle is the diurnal variation of the barometer lies in the fact that it would oblige us to place the electric currents into the lower regions of the atmosphere, as these only will be much affected by the thermal radiation of the Sun. The phase of the barometric oscillation has been found to be reversed on the top of mountains, and it would be interesting to see whether the magnetic variations show any peculiarities at great heights.*

The region of the atmosphere which other considerations lead me to consider as the most sensitive to electromotive forces is that of the cirrus clouds, and I should be inclined to look to that region for a solution of the question. The lunar action seems, according to the researches of Mr. CHAMBERS, to be a modification of the solar action rather than an independent effect. This might be accounted for if we suppose that the conductivity of the air depends on the position of the Sun, while the electromotive forces depend on the combined positions of the Sun and Moon.

* [Note added October 11, 1889.—Since writing the above I have become acquainted with HANN's recent work on the diurnal oscillation of the barometer ('Wien, Denkschriften,' vol. 55, 1889.) It appears from the regularity of the semidiurnal period in different altitudes and latitudes that its cause must lie in atmospheric movements in higher regions of the atmosphere. The reversal of phase mentioned in the text is due to local effects and has nothing to do with the regular oscillation. It seems to be exceedingly probable in the light of these researches that the daily variation of the magnetic needle is connected with the daily oscillation of the barometer in the way described in the text.]

It will be interesting to follow out in future researches the field which this investigation has opened, especially in order to trace the effect of the Sunspot variation; but for this purpose it is absolutely necessary that different observatories should follow a more uniform plan in reducing their observations. It has been found by experience that if the hourly readings of the magnetic needle are collected together, and their mean taken, that mean is different according as the disturbed days are taken into account or rejected. In other words, the disturbances are not irregularly distributed, but have a daily period which is mixed up with the regular daily variation. If we want to separate the investigation concerning the regular variation and the disturbance variation, we must adopt some plan of obtaining the one without the other. I need not here describe SABINE's well-known method of doing this. Grave objections have been urged against it, but it is still adopted in many observatories. A discussion of the various methods of reduction which have been proposed will be found in recent Reports of the British Association, and amongst them that adopted by Mr. WILD at St. Petersburg seems to me to be the only one which can be justified on strict scientific principles. It consists in selecting the curves for the quiet days, of which there are always a sufficient number in each month, and not to take account, as SABINE's method does, of any reading at all during the disturbed days. We get in this way something perfectly definite, namely, the mean variation of the magnetic needle during certain specified days. It seems to me that if the heads of different observatories could adopt some system of intercommunication, by which they could select those days which are most quiet all over the world, and if the elements are reduced for those days solely at the different stations, we should obtain a series of values for different points of the Earth which are strictly comparable with each other. The labour of reduction, as far as I can judge, would thereby be seriously diminished. The method hitherto adopted at Greenwich is very similar to that of WILD, and will not, probably, lead to results which are sensibly different.

The reduction of the observations by spherical harmonic analysis would be a very simple matter according to the method which I have followed, if the results of different stations were published in a manner which would lend itself easily to the work. The method of publication adopted at Greenwich is very convenient, and might serve as a model to other observatories. Much labour is, however, involved in reducing variations in horizontal force and declination to variations in force towards the geographical West and North respectively. If all observatories could publish the coefficients of the harmonic series of the elements as at Greenwich, but reduced to the geographical instead of the magnetic co-ordinates, the progress of magnetic science would be much assisted, as every scientific investigation must take the geographical components for its starting point.

I have tried to form an idea as to the degree of accuracy reached in the determination of such quantities as the daily variation of declination; the result is not altogether satisfactory. Mr. WHIPPLE, in the 'British Association Report' (Birmingham, 1886, page 71), says :—

"Contrasting the Kew results with those of Greenwich, we may fairly consider the difference to be due in some measure to instrumental causes, the construction of the magnetographs being dissimilar at the two observatories. The slight difference in position of the two observatories may likewise have some influence."

The difference amounts to about 15 per cent., and it seems as if the question whether such a difference can be due to instrumental causes deserves a careful examination. Mr. CHAMBERS, at Bombay, has found similarly that the results of the magnetographs differ from those obtained by the old magnetometers; and he seems to ascribe the difference to an "influence of height above or below the ground level." The height of the magnetometer was 6 feet above ground, and that of the magnetograph $7\frac{1}{2}$ feet below; the former gives ranges greater by 7 per cent. for the declination variation, and the difference is greater still for the horizontal force component. That there should be a real difference of that magnitude in the two positions seems excessively unlikely, and we must conclude that at present the results given by magnetographs are doubtful to the extent of about 10 per cent.

In conclusion, we may sum up the principal results obtained in this paper as follows :—

1. The principal part of the diurnal variation is due to causes outside the Earth's surface, and probably to electric currents in our atmosphere.

2. Currents are induced in the Earth by the diurnal variation which produce a sensible effect chiefly in reducing the amplitude of the vertical component and increasing the amplitude of the horizontal components.

3. As regards the currents induced by the diurnal variation, the Earth does not behave as a uniformly-conducting sphere, but the upper layers must conduct less than the inner layers.

4. The horizontal movements in the atmosphere which must accompany a tidal action of the Sun or Moon or any periodic variation of the barometer such as is actually observed would produce electric currents in the atmosphere having magnetic effects similar in character to the observed daily variation.

5. If the variation is actually produced by the suggested cause, the atmosphere must be in that sensitive state in which, according to the author's experiments, there is no lower limit to the electromotive force producing a current.

In conclusion, the author begs to return his thanks to Mr. WILLIAM ELLIS for help given in some of the calculations, and also to his assistant, Mr. ARTHUR STANTON, for much labour bestowed on making and checking numerical calculations.

APPENDIX.

On the Currents Induced in a Spherical Conductor by Variation of an External Magnetic Potential.

By HORACE LAMB, *M.A., F.R.S.*

THE general formulæ for the currents induced in a sphere of uniform conductivity by any electric or magnetic disturbances outside it have been given in the 'Phil. Trans.,' 1883, pp. 526 *et seq.* I here reproduce (with some further developments) so much of the investigation as is required for the discussion in Part V. of the foregoing paper.

Suppose that, the origin being taken at the centre of the Earth, we have an external disturbing force, whose magnetic potential near the Earth's surface is

$$\Omega_n e^{i(pt+\epsilon)},$$

when Ω_n is a solid harmonic of positive degree n, and $i = \sqrt{-1}$.

The corresponding values of the components of electric momentum are

$$\mathrm{F} = \frac{1}{n+1}\left(y\frac{d}{dz} - z\frac{d}{dy}\right)\Omega_n,$$

$$\mathrm{G} = \frac{1}{n+1}\left(z\frac{d}{dx} - x\frac{d}{dz}\right)\Omega_n,$$

$$\mathrm{H} = \frac{1}{n+1}\left(x\frac{d}{dy} - y\frac{d}{dx}\right)\Omega_n,$$

the time-factor being omitted here and elsewhere for shortness. For these values make

$$\frac{d\mathrm{F}}{dx} + \frac{d\mathrm{G}}{dy} + \frac{d\mathrm{H}}{dz} = 0,$$

and also give, for the components of magnetic force, the values

$$\alpha = \frac{d\mathrm{H}}{dy} - \frac{d\mathrm{G}}{dz} = \frac{1}{n+1}\left\{x\nabla^2\Omega_n - \left(x\frac{d}{dx} + y\frac{d}{dy} + z\frac{d}{dz} + 2\right)\frac{d\Omega_n}{dx}\right\}$$

$$= -\frac{d\Omega_n}{dx},$$

and, similarly,

$$\beta = -\frac{d\Omega_n}{dy},$$

$$\gamma = -\frac{d\Omega_n}{dz}.$$

If u, v, w be the components of electric current, the equations of induced currents are

$$\rho u = -\dot{\mathrm{F}}, \qquad \rho v = -\dot{\mathrm{G}}, \qquad \rho w = -\dot{\mathrm{H}},$$

where ρ denotes the specific resistance. Eliminating u, v, w by means of the relations

$$\nabla^2\mathrm{F} = -4\pi u, \qquad \nabla^2\mathrm{G} = -4\pi v, \qquad \nabla^2\mathrm{H} = -4\pi w,$$

we find that at all points within the sphere F, G, H must satisfy the equations

$$(\nabla^2 - k^2)\,\mathrm{F} = 0, \qquad (\nabla^2 - k^2)\,\mathrm{G} = 0, \qquad (\nabla^2 - k^2)\,\mathrm{H} = 0,$$

$$\frac{d\mathrm{F}}{dx} + \frac{d\mathrm{G}}{dy} + \frac{d\mathrm{H}}{dz} = 0,$$

where

$$k^2 = \frac{4\pi p}{\rho}\cdot i.$$

The appropriate solution of these is

$$\left.\begin{aligned} \mathrm{F} &= \chi_n(kr).\left(y\frac{d}{dz} - z\frac{d}{dy}\right)\omega_n, \\ \mathrm{G} &= \chi_n(kr).\left(z\frac{d}{dx} - x\frac{d}{dz}\right)\omega_n, \\ \mathrm{H} &= \chi_n(kr).\left(x\frac{d}{dy} - y\frac{d}{dx}\right)\omega_n, \end{aligned}\right\}$$

where $r = (x^2 + y^2 + z^2)^{\frac{1}{2}}$, ω_n denotes a solid harmonic of degree n, and

$$\begin{aligned} \chi_n(\zeta) &= 1 + \frac{\zeta^2}{2.2n+3} + \frac{\zeta^4}{2.4.2n+3.2n+5} + \dots \\ &= 3.5\dots 2n+1.\left(\frac{d}{\zeta d\zeta}\right)^n \frac{\sinh\zeta}{\zeta}. \end{aligned}$$

The total magnetic potential outside the sphere will be

$$\Omega_n + \Omega_{-n-1},$$

where Ω_{-n-1} is the part due to the induced currents. The values of F, G, H at external points will then be

$$\begin{aligned} \mathrm{F} &= \left(y\frac{d}{dz} - z\frac{d}{dy}\right)\left(\frac{1}{n+1}\,\Omega - \frac{1}{n}\,\Omega_{-n-1}\right), \\ \mathrm{G} &= \left(z\frac{d}{dx} - x\frac{d}{dz}\right)\left(\frac{1}{n+1}\,\Omega_n - \frac{1}{n}\,\Omega_{-n-1}\right), \\ \mathrm{H} &= \left(x\frac{d}{dy} - y\frac{d}{dx}\right)\left(\frac{1}{n+1}\,\Omega_n - \frac{1}{n}\,\Omega_{-n-1}\right). \end{aligned}$$

It remains to introduce the conditions to be satisfied at the surface of the sphere ($r = \mathrm{R}$). The continuity of electromotive force, *i.e.*, of $\dot{\mathrm{F}}$, $\dot{\mathrm{G}}$, $\dot{\mathrm{H}}$, requires

$$\chi_n(k\mathrm{R}) \,.\, \omega_n = \frac{1}{n+1}\Omega_n - \frac{1}{n}\Omega_{-n-1}. \quad [r = \mathrm{R}]$$

The continuity of the magnetic force involves the continuity of the space-derivatives of F, G, H, and, therefore, of $d\mathrm{F}/dr$, $d\mathrm{G}/dr$, $d\mathrm{H}/dr$. Hence

$$\{k\mathrm{R}\chi'_n(k\mathrm{R}) + n\chi_n(k\mathrm{R})\}\,\omega_n = \frac{n}{n+1}\Omega_n + \frac{n+1}{n}\Omega_{-n-1}. \quad [r = \mathrm{R}]$$

We thence find

$$\left.\begin{aligned} k\mathrm{R}\,.\,\chi'_n(k\mathrm{R})\,.\,\omega_n &= \frac{2n+1}{n}\Omega_{-n-1}, \\ \{k\mathrm{R}\chi'_n(k\mathrm{R}) + (2n+1)\chi_n(k\mathrm{R})\}\,\omega_n &= \frac{2n+1}{n+1}\Omega_n, \end{aligned}\right\} \quad [r = \mathrm{R}]$$

which are equivalent to

$$\left.\begin{aligned} \frac{k^2\mathrm{R}^2}{2n+1\,.\,2n+3}\chi_{n+1}(k\mathrm{R})\,.\,\omega_n &= \frac{1}{n}\Omega_{-n-1}, \\ \chi_{n-1}(k\mathrm{R})\,.\,\omega_n &= \frac{1}{n+1}\Omega_n. \end{aligned}\right\} \quad [r = \mathrm{R}]$$

Hence

$$\frac{\Omega_{-n-1}}{\Omega_n} = \frac{n}{n+1}\cdot\frac{k^2\mathrm{R}^2}{2n+1\,.\,2n+3}\cdot\frac{\chi_{n+1}(k\mathrm{R})}{\chi_{n-1}(k\mathrm{R})}.$$

This gives the ratio of surface potentials, and, therefore, of horizontal forces, due to internal and external influences respectively. Since this ratio is "complex," there will be a difference of phase, as well as of amplitude. The corresponding ratio of vertical forces is

$$\left[\frac{-\dfrac{d\Omega_{-n-1}}{dr}}{-\dfrac{d\Omega_n}{dr}}\right]_{r=\mathrm{R}} = -\frac{n+1}{n}\cdot\frac{\Omega_{-n-1}}{\Omega_n} = -\frac{k^2\mathrm{R}^2}{2n+1\,.\,2n+3}\cdot\frac{\chi_{n+1}(k\mathrm{R})}{\chi_{n-1}(k\mathrm{R})}.$$

To interpret these results it is necessary to calculate the function

$$\frac{\zeta^2}{2n+1\,.\,2n+3}\cdot\frac{\chi_{n+1}(\zeta)}{\chi_{n-1}(\zeta)},$$

where

$$\zeta^2 = k^2\mathrm{R}^2 = \frac{4\pi p\mathrm{R}^2}{\rho}\cdot i = i\delta, \text{ say.}$$

For moderate values of δ we may use the form

$$\frac{i\delta}{2n+1.2n+3}\cdot\frac{A_{n+1}+iB_{n+1}}{A_{n-1}+iB_{n-1}},$$

where

$$A_n = 1 - \frac{\delta^2}{2.4.2n+3.2n+5} + \frac{\delta^4}{2.4.6.8.2n+3.2n+5.2n+7.2n+9} - \ldots,$$

$$B_n = \frac{\delta}{2.2n+3} - \frac{\delta^3}{2.4.6.2n+3.2n+5.2n+7} = \ldots.$$

The following Table gives the values of A_1, B_1, A_3, B_3, A_5, B_5, for various values of δ. It may possibly be of service in other investigations.

TABLE XXIX.

δ.	A_1.	B_1.	A_3.	B_3.	A_5.	B_5.
1	+ 0·996429	+ 0·099934	+ 0·998738	+ 0·055539	+ 0·999359	+ ·038456
2	+ 0·985726	+ 0·199471	+ 0·994952	+ 0·110982		
3	+ 0·967918	+ 0·298216	+ 0·988647	+ 0·116230		
4	+ 0·943050	+ 0·395773	+ 0·979832	+ 0·221187		
5	+ 0·911184	+ 0·491751	+ 0·968518	+ 0·275757		
6	+ 0·872401	+ 0·585759	+ 0·954720	+ 0·329843		
7	+ 0·826800	+ 0·677412	+ 0·938455	+ 0·383350		
8	+ 0·774498	+ 0·766327	+ 0·919714	+ 0·436182		
9	+ 0·715628	+ 0·852126	+ 0·898610	+ 0·488246		
10	+ 0·650341	+ 0·934439	+ 0·875083	+ 0·539448	+ 3·936309	+ ·3784
20	− 0·310366	+ 1·489206	+ 0·516311	+ 0·984135		
30	− 1·628644	+ 1·351846	− 0·029614	+ 1·248628		
40	− 2·912855	+ 0·337304	− 0·688914	+ 1·265555		
50	− 3·716054	− 1·563371	− 1·366602	+ 0·993199		
100	+ 12·514840	− 10·691120	− 1·308236	− 3·811231	− 1·9434	− ·6646

If we write

$$A_n + i\,B_n = S_n e^{i\phi_n},$$

the above fraction becomes

$$\frac{\delta}{2n+1\,.\,2n+3}\cdot\frac{S_{n+1}}{S_{n-1}}\cdot e^{i\left(\phi_{n+1}-\phi_{n-1}+\frac{1}{2}\pi\right)}.$$

If we prefix the *minus* sign this gives the ratio (k') of the vertical forces. To get the ratio (k) of the horizontal forces we must multiply by $n/(n+1)$.

For large values of δ we may make use of the second form of χ_n. Thus

$$\chi_1 = 3\left\{\frac{\cosh\zeta}{\zeta^2} - \frac{\sinh\zeta}{\zeta^3}\right\},$$

$$\chi_3 = 3.5.7\left\{\left(\frac{1}{\zeta^4}+\frac{15}{\zeta^6}\right)\cosh\zeta - \left(\frac{6}{\zeta^5}+\frac{15}{\zeta^7}\right)\sinh\zeta\right\},$$

whence

$$\frac{\zeta^2}{5.7}\frac{\chi_3(\zeta)}{\chi_1(\zeta)} = \frac{(1+15\zeta^{-2})\cosh\zeta - (6\zeta^{-1}+15\zeta^{-3})\sinh\zeta}{\cosh\zeta - \zeta^{-1}\sinh\zeta}.$$

Here

$$\zeta = (i\delta)^{\frac{1}{2}} = (1+i)\,\delta^{\frac{1}{2}}/\sqrt{2} = (1+i)\,\beta, \text{ say.}$$

If β is moderately large we may put $\cosh\zeta/\sinh\zeta = 1$ approximately. The error thus committed is of the order $e^{-2\beta}$. Since the value of e^{-14} has six cyphers after the decimal point, this approximation is amply sufficient for $\beta > 7$, or say for $\delta > 100$. The above fraction is then

$$= \frac{1 - 6\zeta^{-1} + 15\zeta^{-2} - 15\zeta^{-3}}{1-\zeta^{-1}} = \frac{(1 - 3\beta^{-1} + \frac{15}{4}\beta^{-3}) + i(3\beta^{-1} - \frac{15}{2}\beta^{-2} + \frac{15}{4}\beta^{-3})}{(1-\frac{1}{2}\beta^{-1}) + i\,.\,\frac{1}{2}\beta^{-1}}.$$

It is by these methods that Tables XX. and XXI. above were calculated. The values of ρ, the specific conductivity, given in the fifth columns, were obtained from the formula

$$\rho = 4\pi p \mathrm{R}^2/\delta$$

by putting

$$2\pi\mathrm{R} = 4\cdot10^9 \text{ cm.,} \quad 2\pi/p = 86{,}400/m \text{ secs.,}$$

where m denotes the number of complete periods in a day, and is therefore $= 1$ for the diurnal and $= 2$ for the semidiurnal variations.

As the resistance diminishes, the difference of phase tends to zero, and the ratio of normal forces to the value -1; *i.e.*, the total normal force at the surface tends to zero, in accordance with the theory of electromagnetic screens.

The ratio of the total vertical to the total horizontal force in any assigned direction is

$$\frac{-\dfrac{d}{dr}(\Omega_n + \Omega_{-n-1})}{-\dfrac{d}{\mathrm{R}d\eta}(\Omega_n + \Omega_{-n-1})}$$

where $\mathrm{R}d\eta$ denotes a linear element drawn in the proper direction on the Earth's surface. By means of the preceding results this can be put in the form

$$\frac{n(n+1)\chi_n(k\zeta)}{k\mathrm{R}\chi'_n(k\mathrm{R}) + (n+1)\chi_n(k\mathrm{R})}\cdot\Omega_n\Big/\frac{d\Omega_n}{d\eta}.$$

The coefficient may be calculated independently, by a proper adaptation of the previous methods, or we may deduce its value from the results already obtained, in the manner explained by Professor SCHUSTER. The function actually computed by Professor SCHUSTER in Tables XXII. and XXIII. is the ratio

$$\frac{-\frac{d}{dr}(\Omega_n + \Omega_{-n-1})}{-\frac{d\Omega_n}{dr}},$$

which

$$= \frac{(n+1)\,\chi_n(k\mathrm{R})}{k\mathrm{R}\,\chi_n'(k\mathrm{R}) + (n+1)\,\chi_n(k\mathrm{R})}.$$

For large values of δ, *i.e.* for sufficiently small values of ρ, we may put $\cosh\zeta = \sinh\zeta$, whence, keeping only the most important term in $\chi_n(\zeta)$, the fraction written becomes

$$= (n+1)/\zeta = \frac{n+1}{\sqrt{\delta}} \cdot e^{-i\pi/4}.$$

The difference of phase therefore tends to the limit 45°, as remarked by Professor SCHUSTER. For $\delta = 100$, this formula gives for the reduction of amplitude the value ·3 and ·5 in the cases $n = 2$ and $n = 4$ respectively.

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HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY, ST. MARTIN'S LANE.

ERRATA.

'Phil. Trans.,' A, 1889.

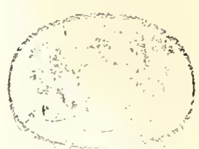
Page 376, line 4 from bottom, *for* solar diameter *read* solar semi-diameter.

Page 381, line 24, *for* 200 *read* 100.

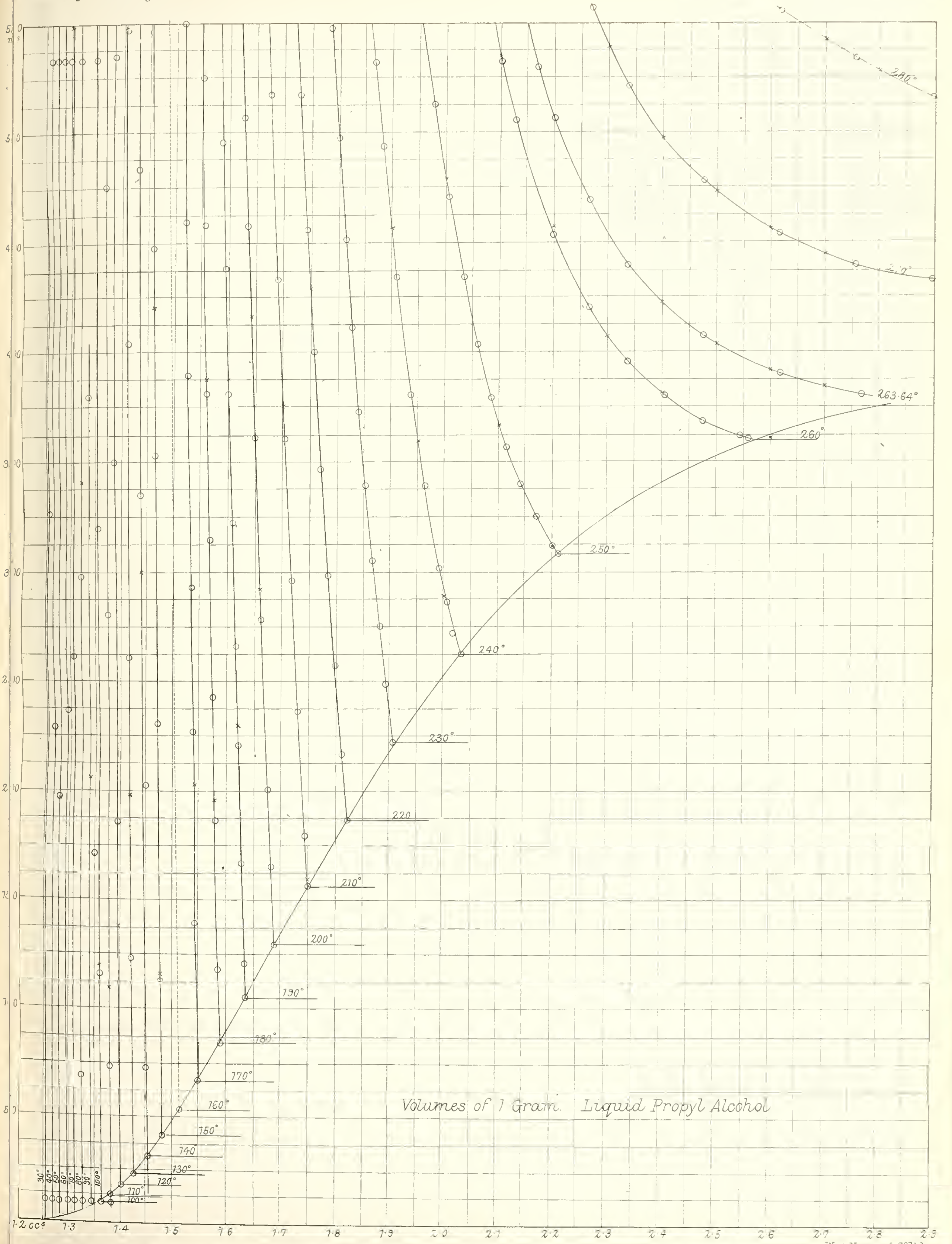
Page 381, line 26, *for* $\frac{1}{800}$ *read* $\frac{1}{80}$.

W. H. Wesley. del.

Annan & Swan. Photogravure



H. Wesley, del. Annan & Swan, Photogravure



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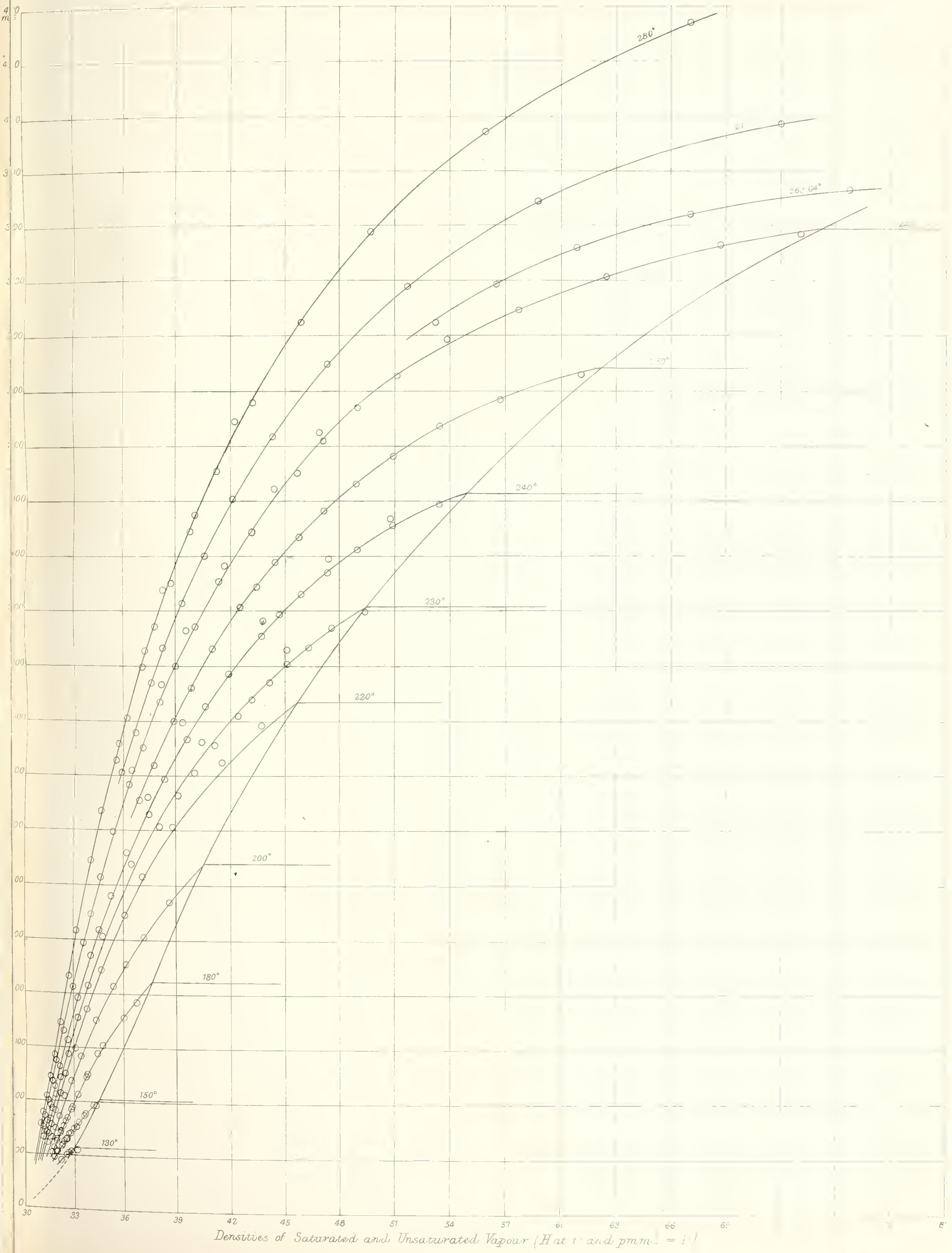

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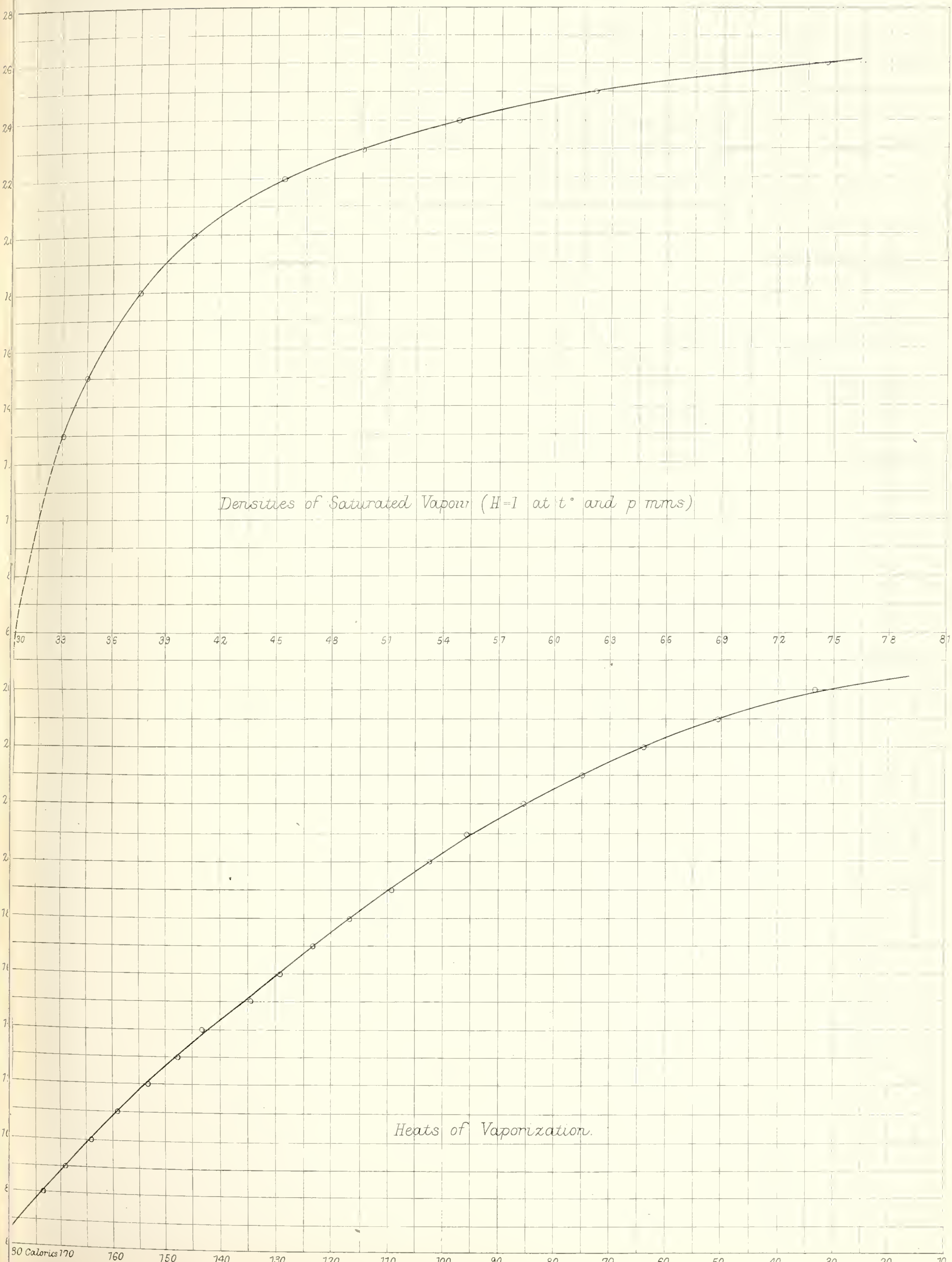
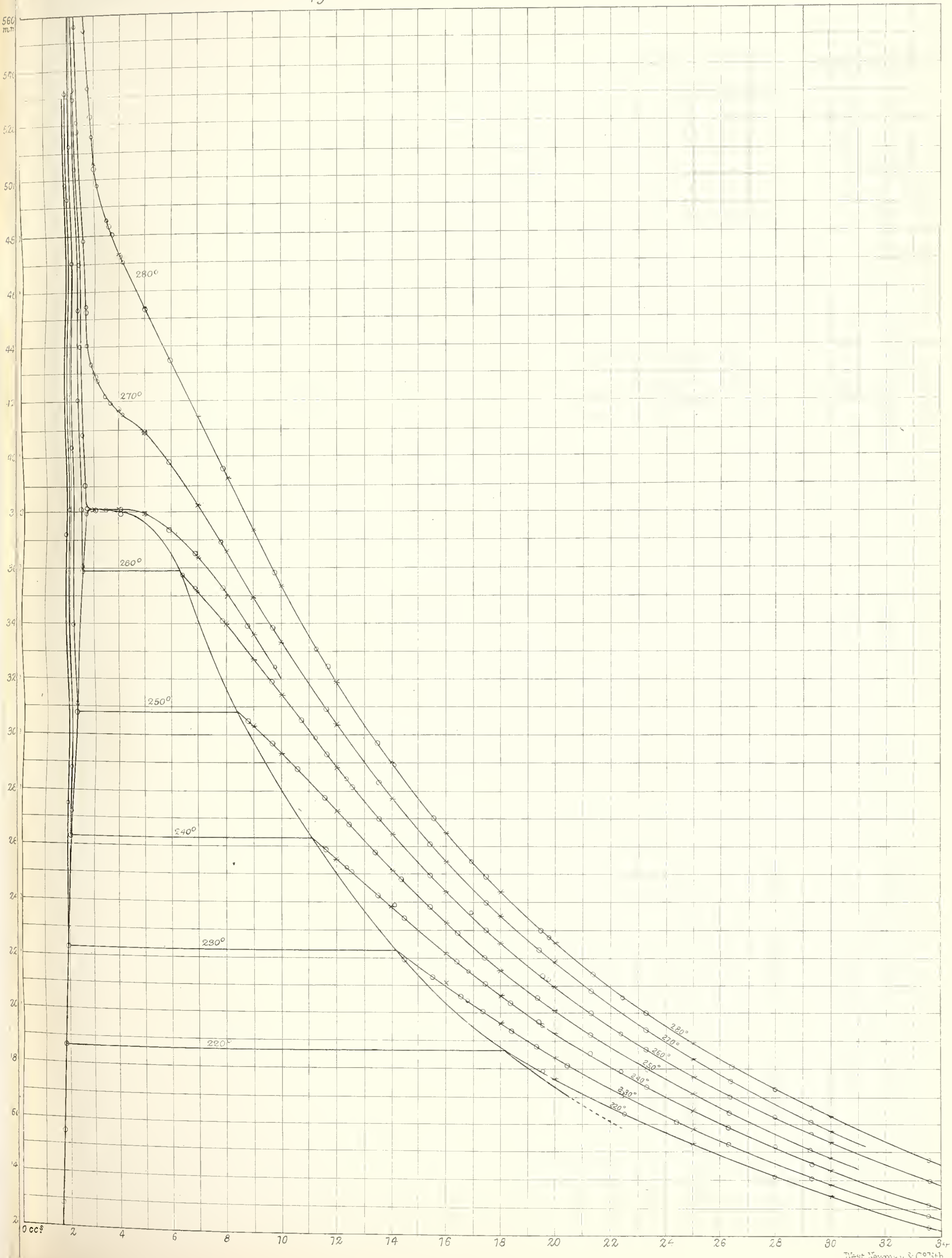
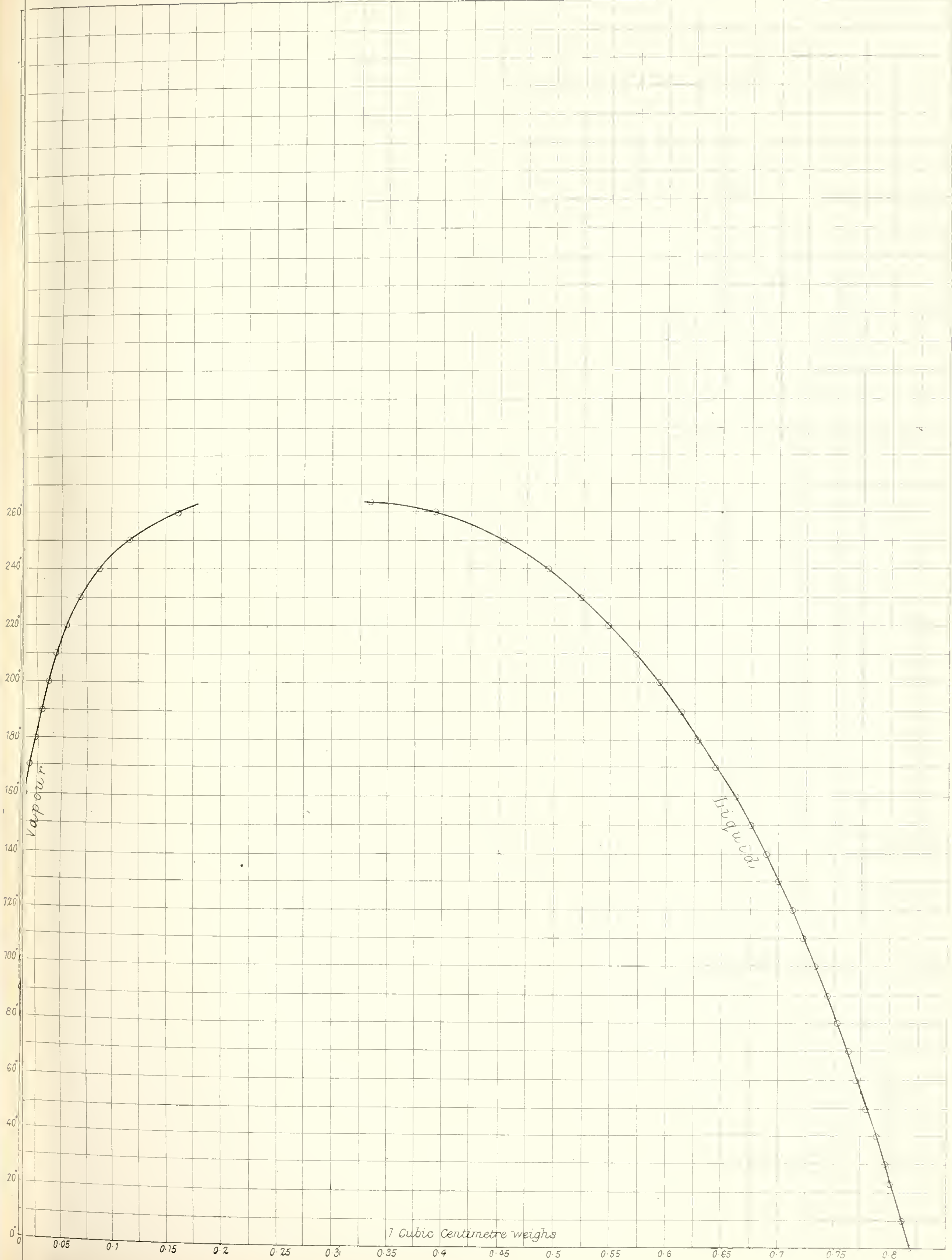


West, Newman & Co. lith.

Propyl Alcohol. Volumes of 1 Gram.



West, Newman & C° lith.



West, Newman & Co lith.

Fig. 1.

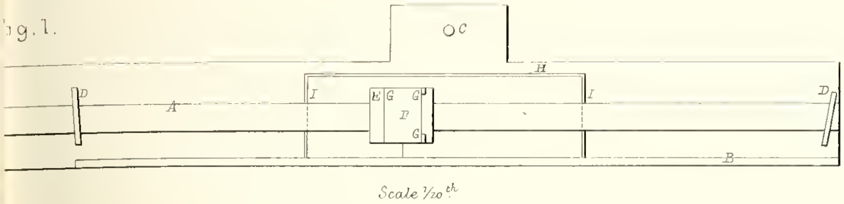


Fig. 3.

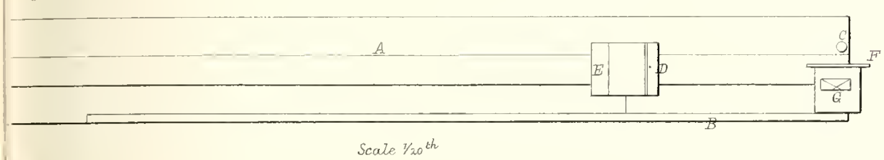
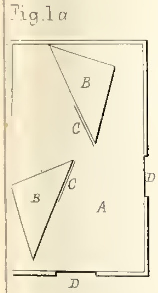
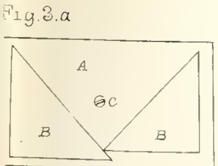


Fig. 1 a



Scale 1/5th

Fig. 3. a



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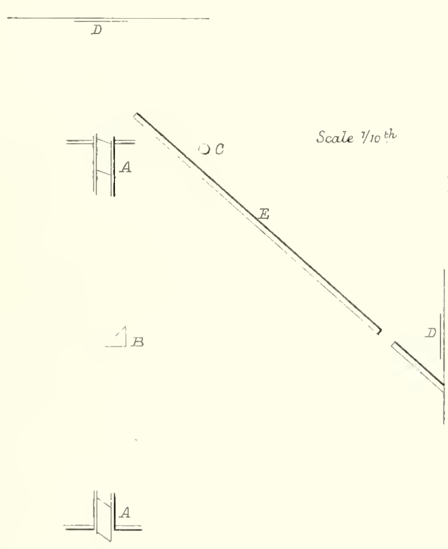
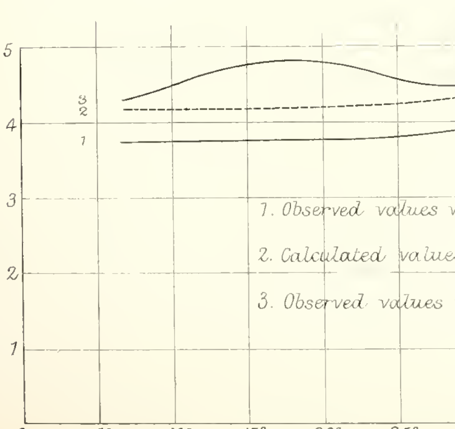
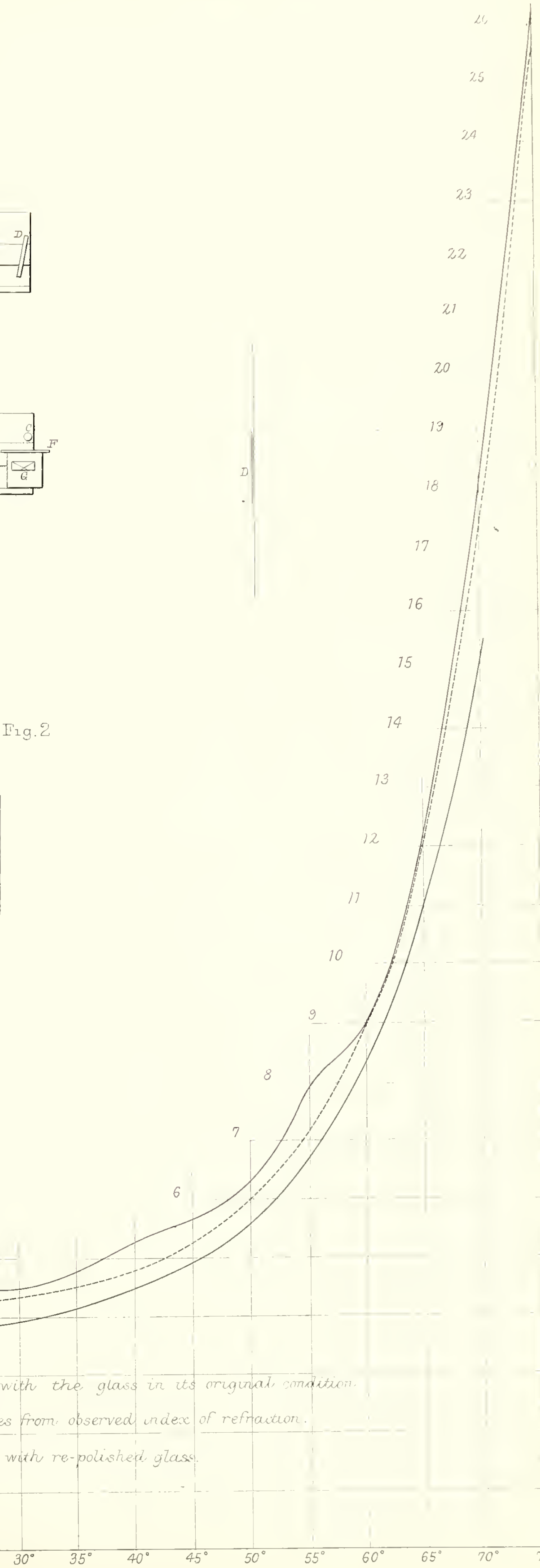


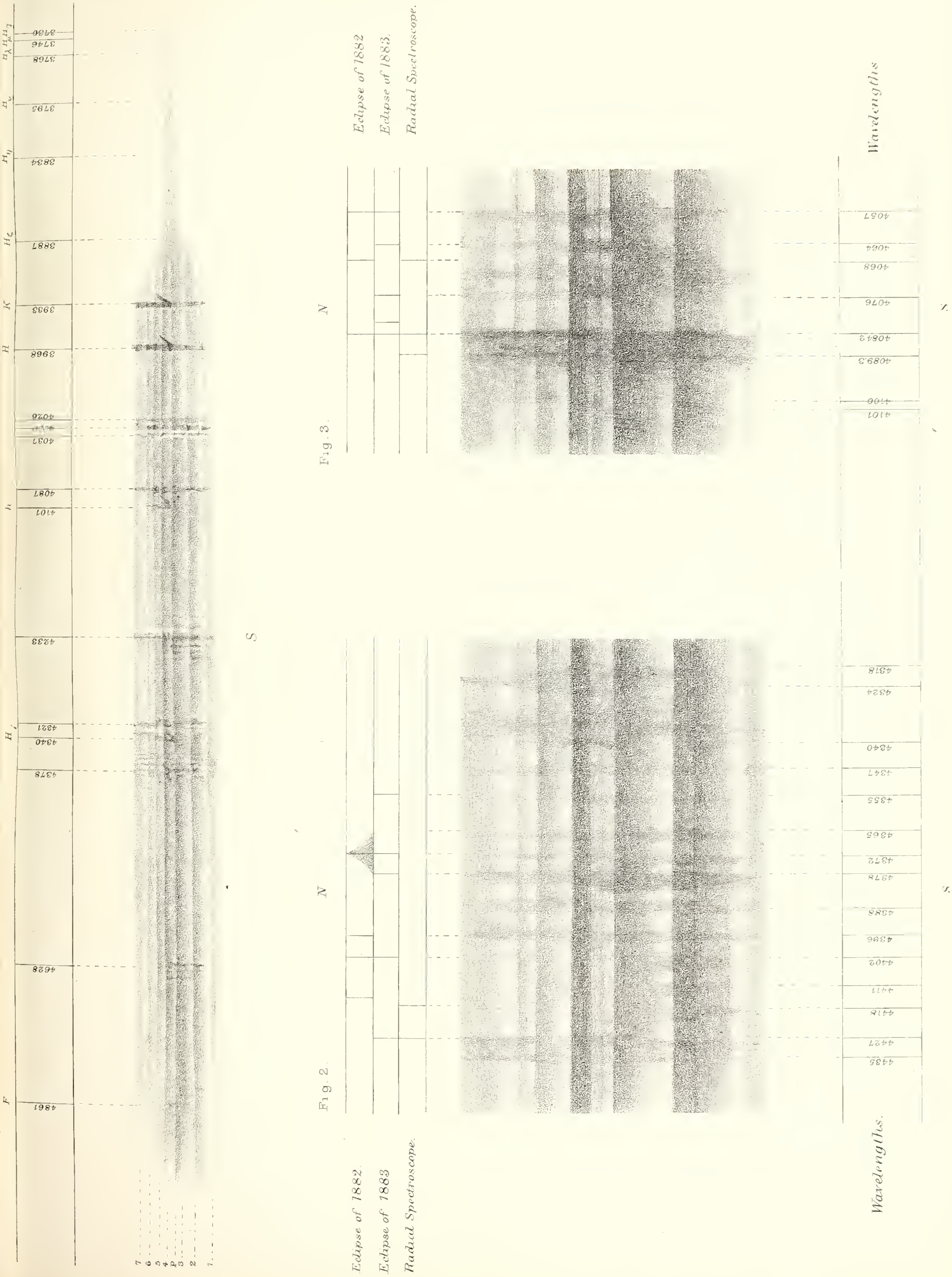
Fig. 2

Fig. 4

Per centage amount of light reflected by crown glass at various angles of incidence.

West, Newman & Co. lith.


Eclipse of 1882
Eclipse of 1883.
Radial Spectroscope.
Wavelengths
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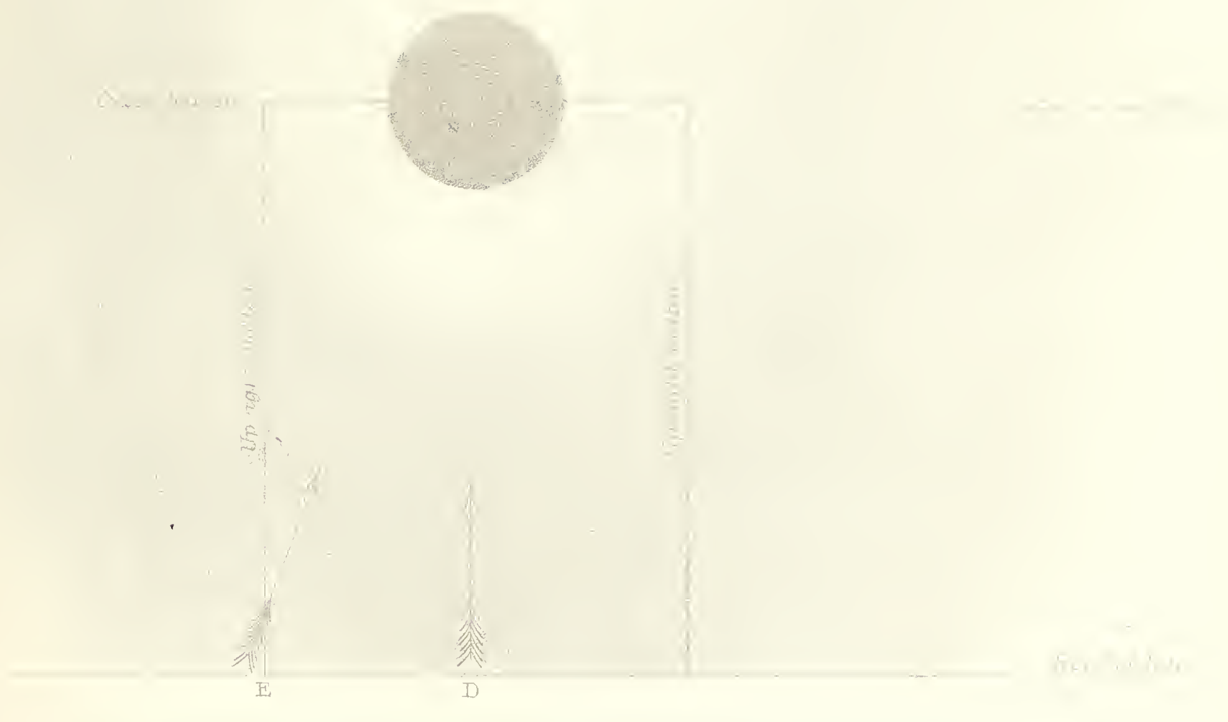
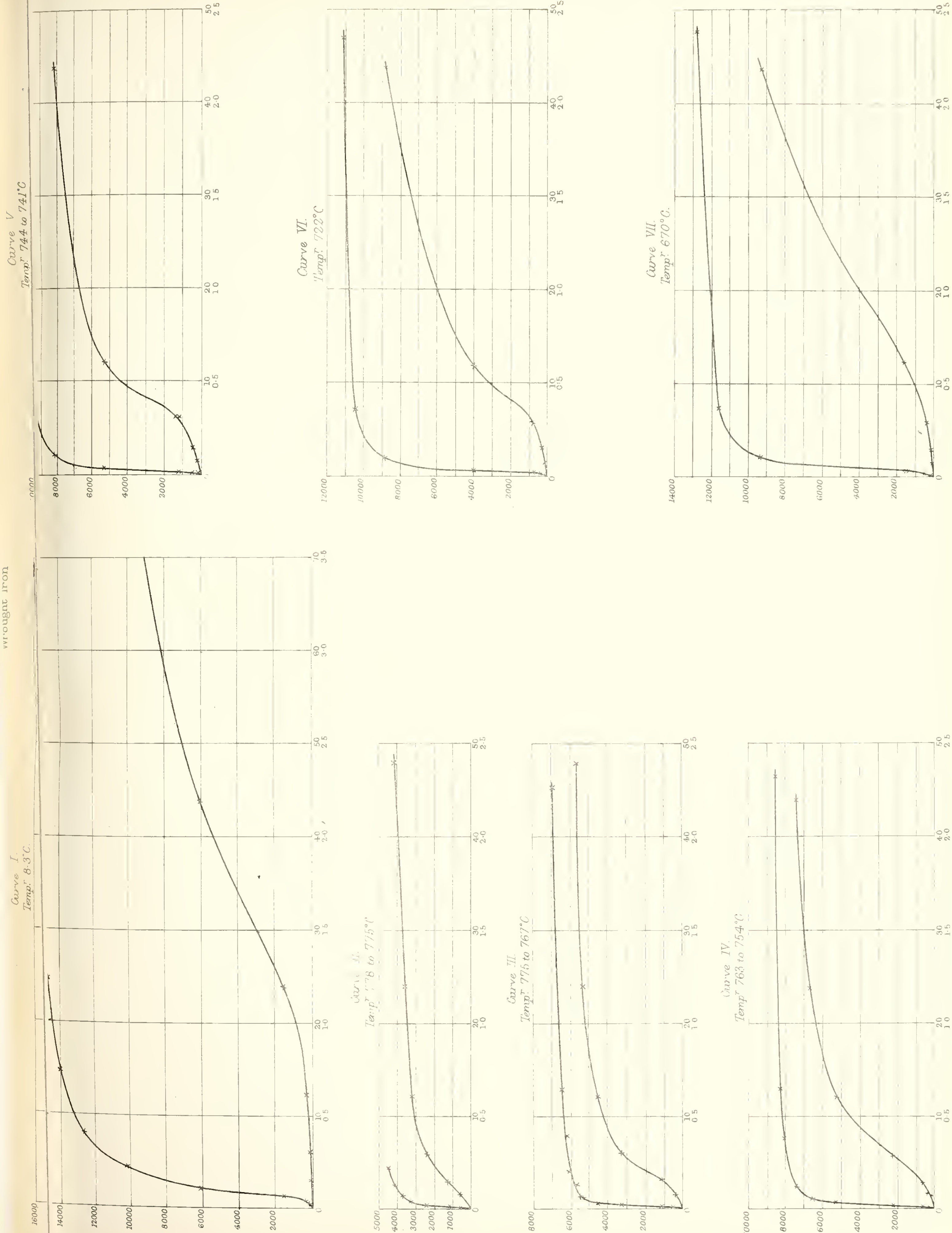
W. H. Wesley, del.

Annan & Swan. Photogravure.



TOTAL ECLIPSE of the SUN

Carriacou 29th August 1886



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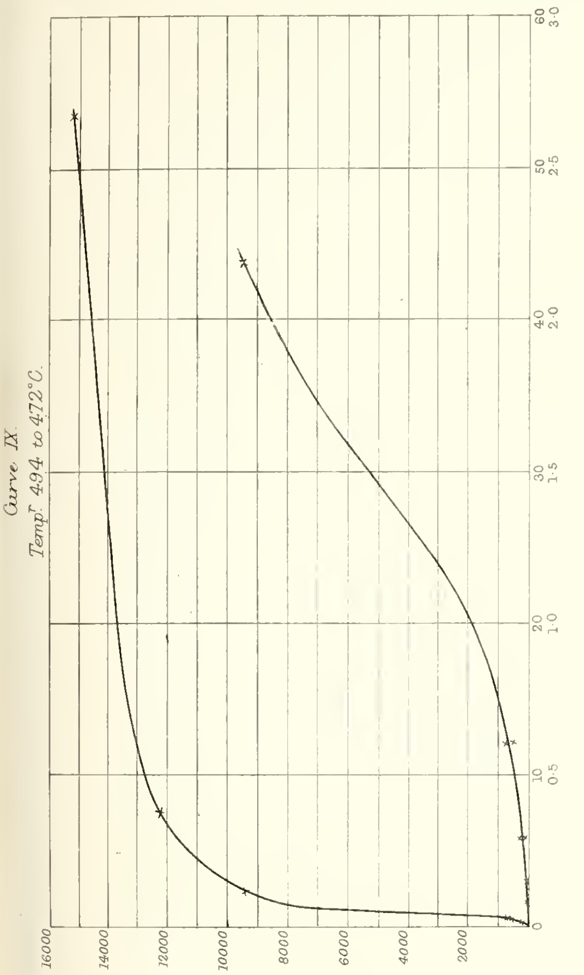
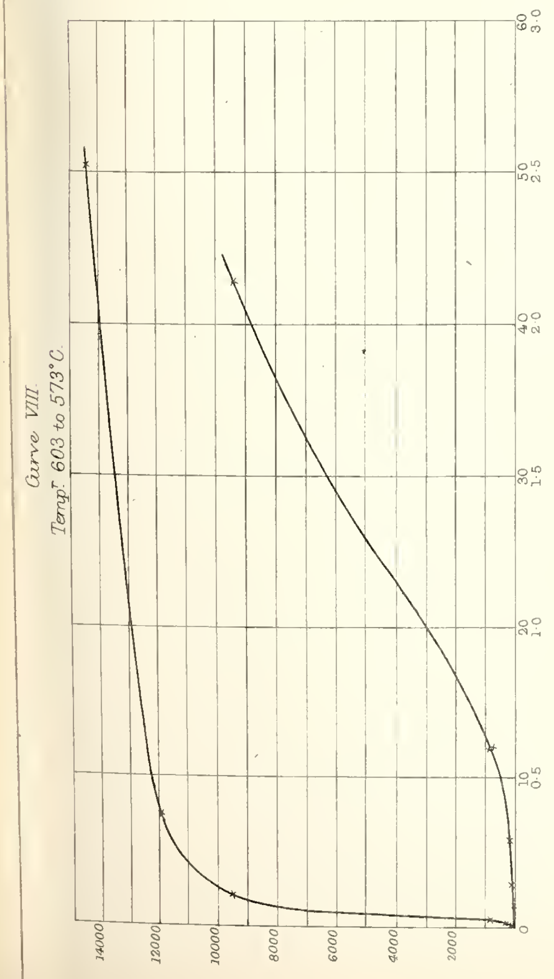
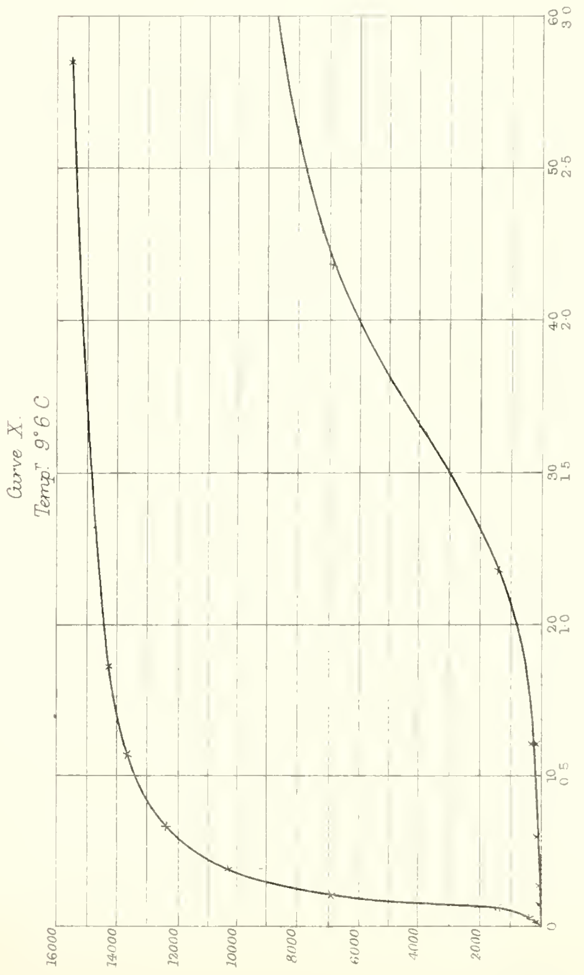




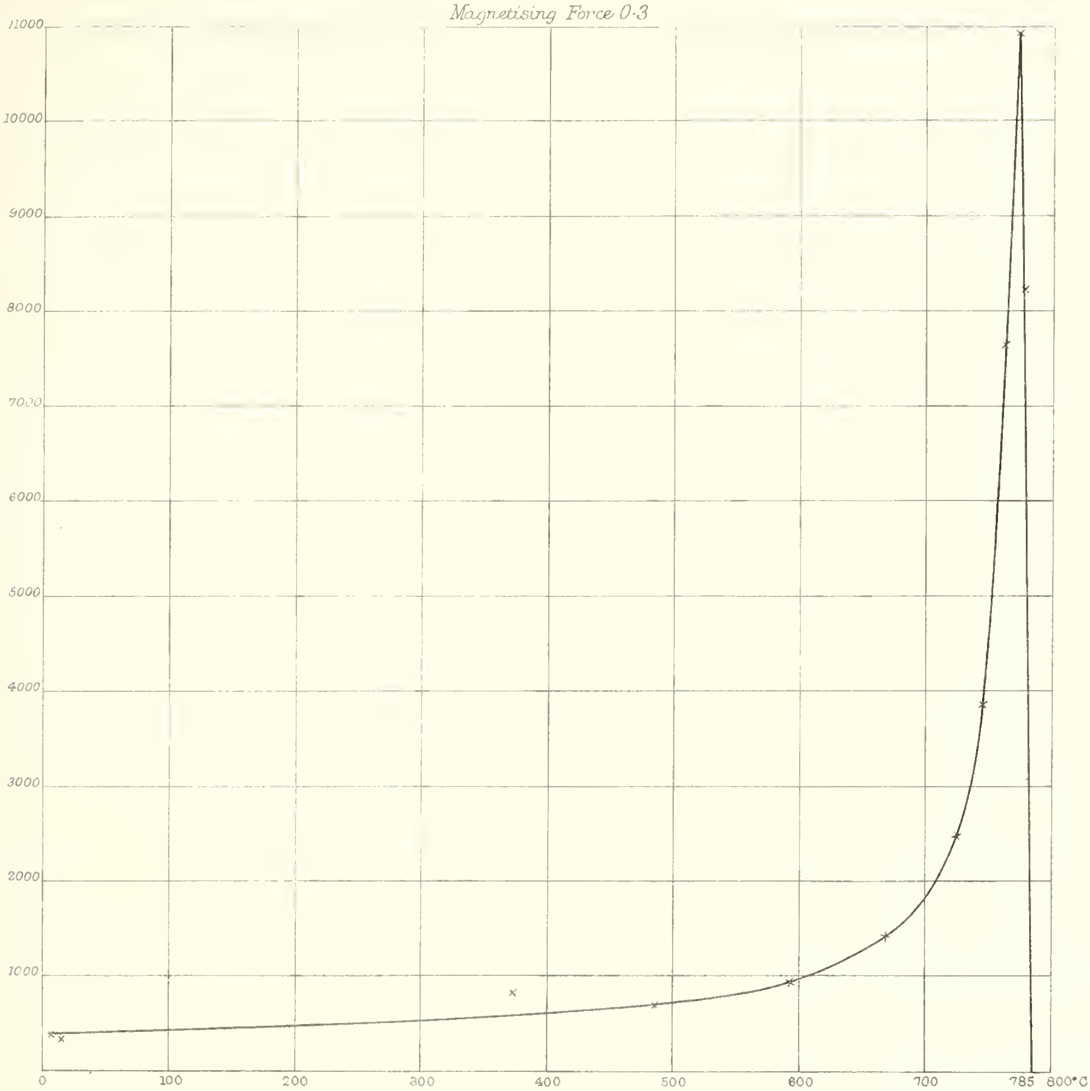


West, Newman & Co lith.

Wrought Iron.

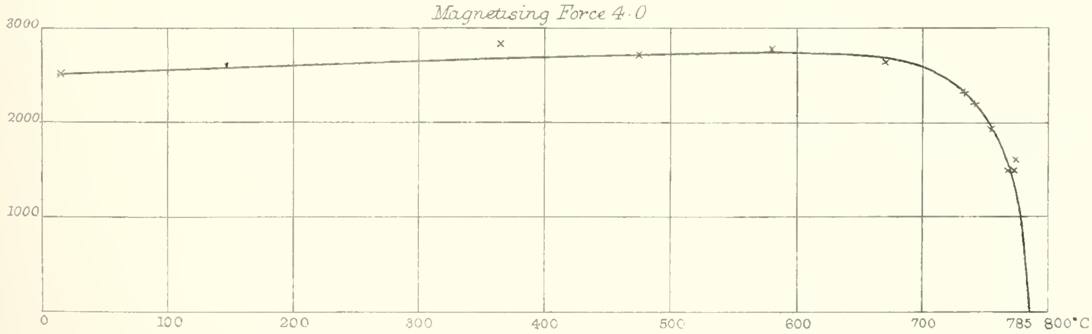
Curve XII.

Magnetising Force 0·3



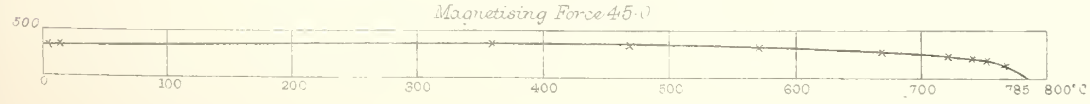
Curve XIII.

Magnetising Force 4·0

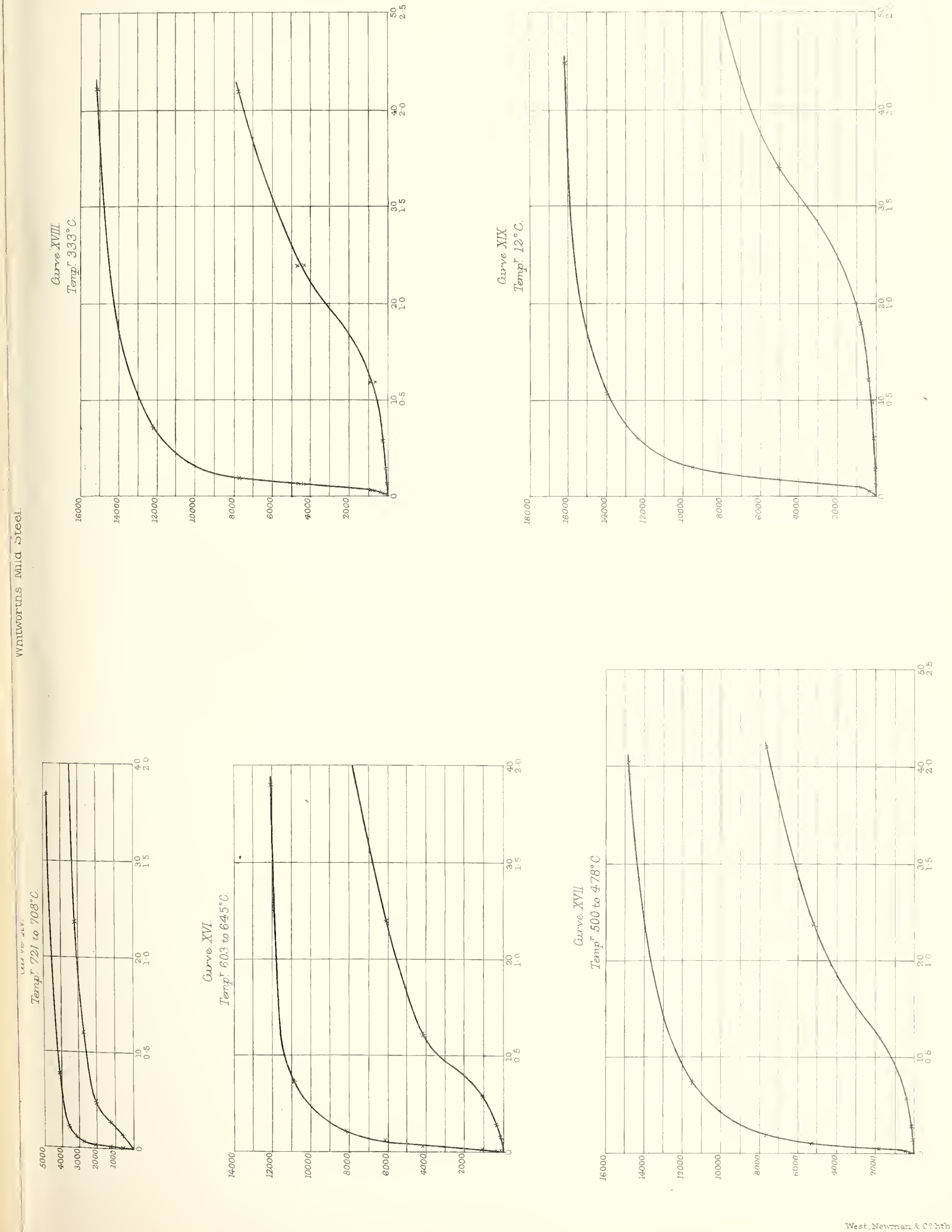


Curve XIV.

Magnetising Force 45·0



West Newman & Cº lith.

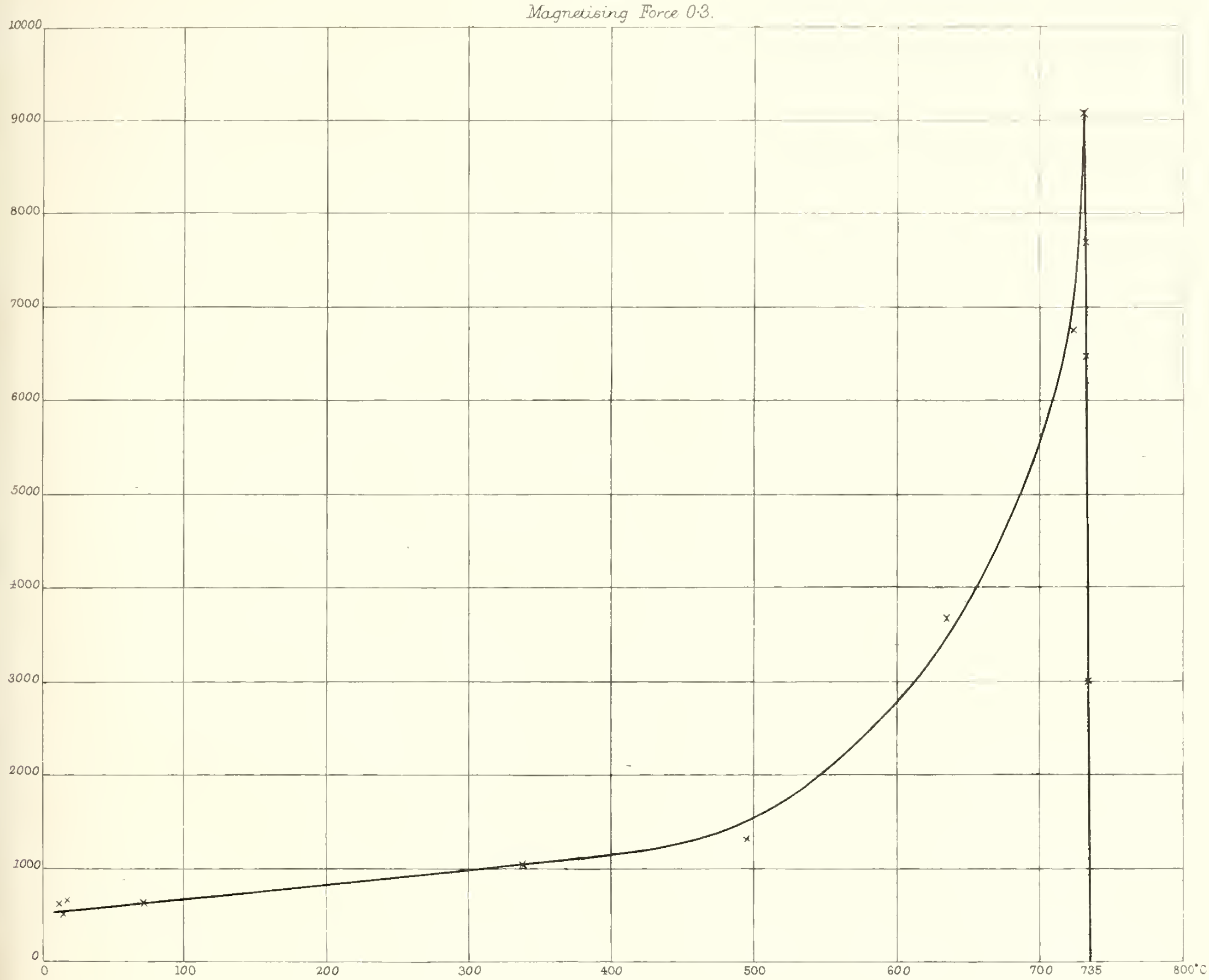


West, Newman & Co. lith.

Whitworth's Mild Steel.

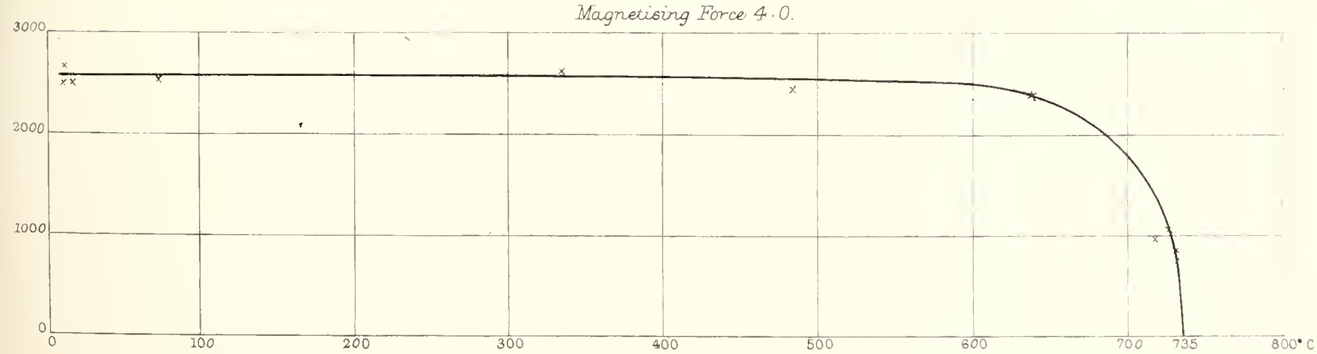
Curve XX.

Magnetising Force 0·3.



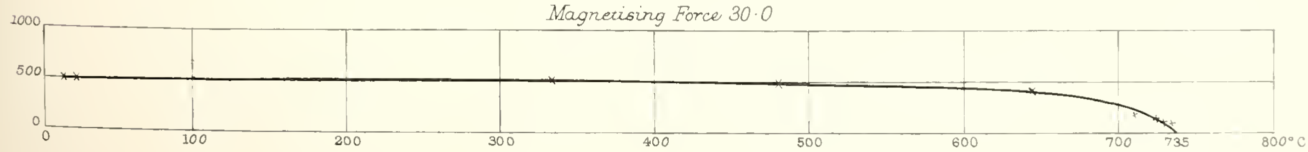
Curve XXI.

Magnetising Force 4·0.

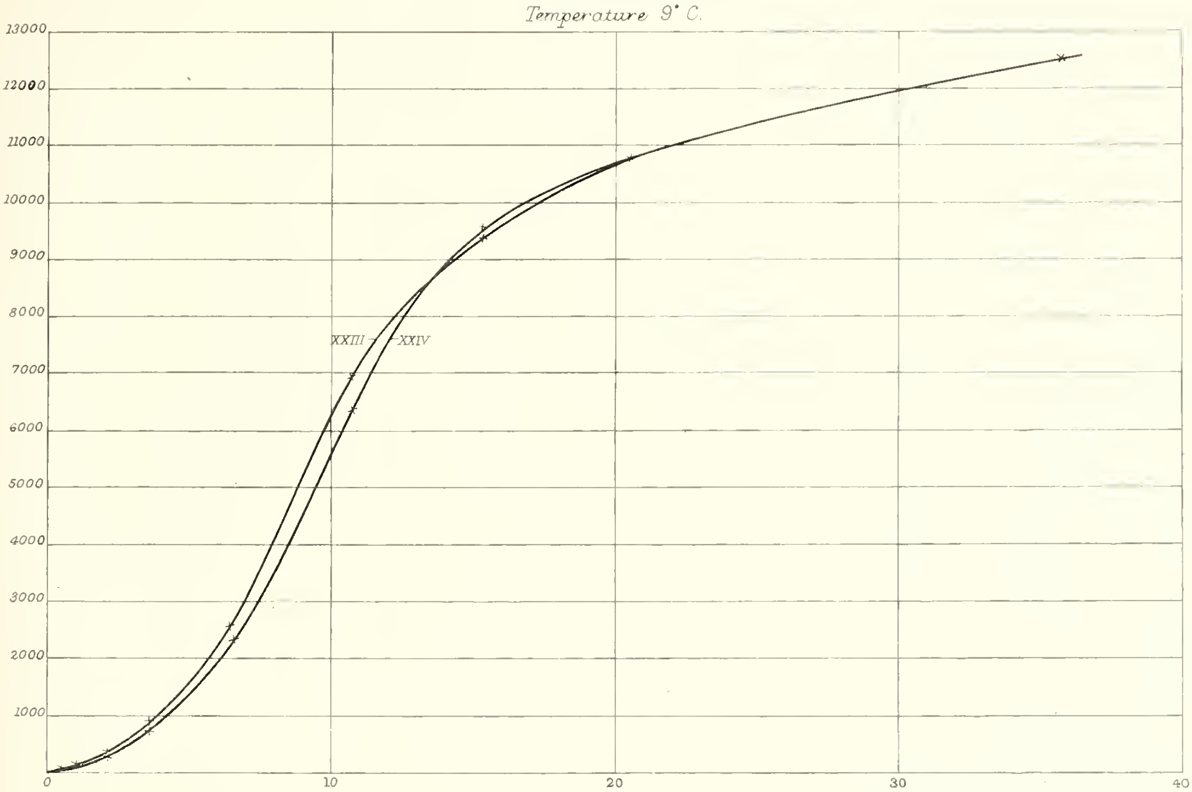


Curve XXII.

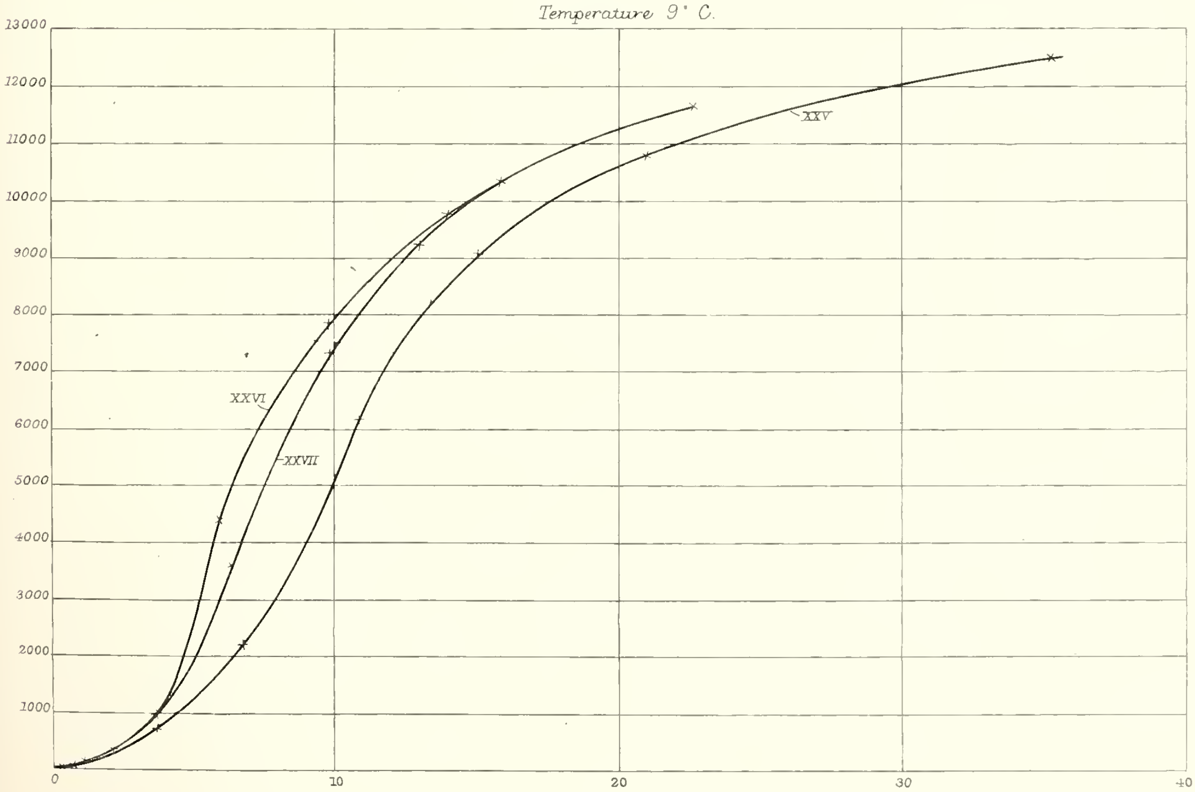
Magnetising Force 30·0



Whitworth's Hard Steel.
Curves XXIII & XXIV.
Temperature 9° C.



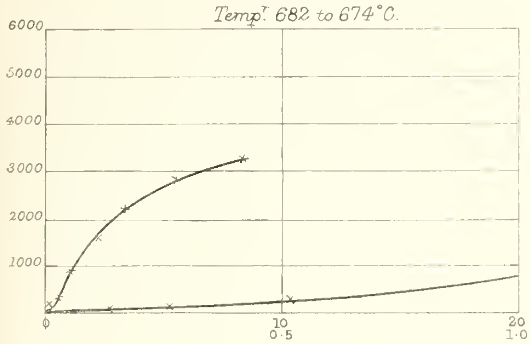
Curves XXV, XXVI, XXVII.
Temperature 9° C.



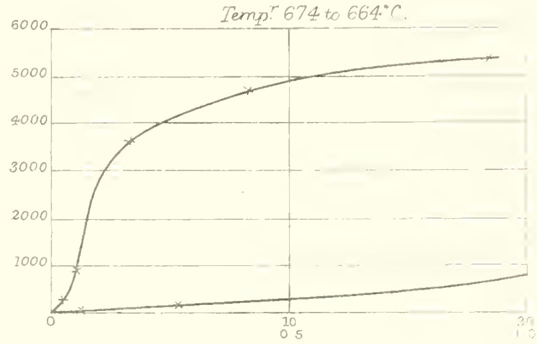
West, Newman & C° lith.

Whitworth's Hard Steel.

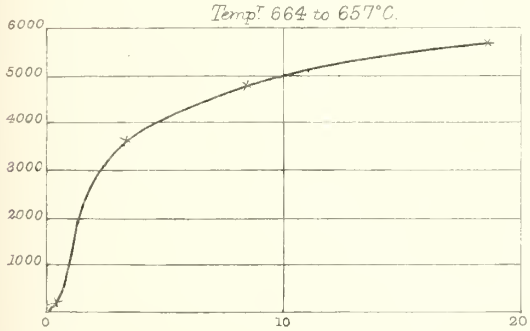
Curve XXVIII.
Temp.r 682 to 674°C.



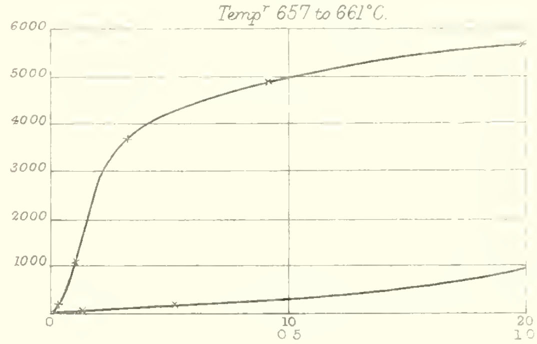
Curve XXIX.
Temp.r 674 to 664°C.



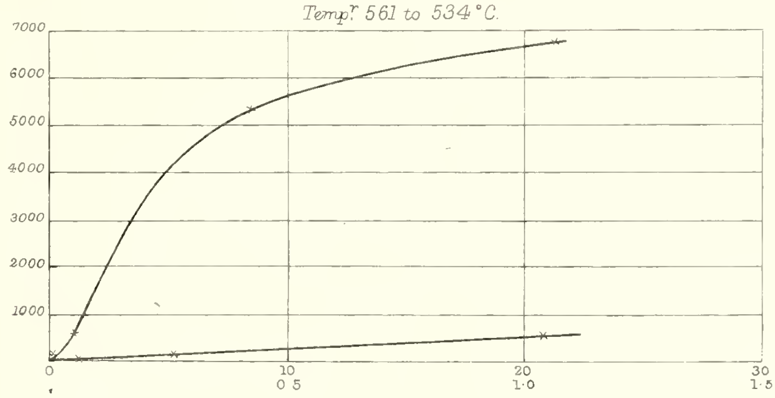
Curve XXX.
Temp.r 664 to 657°C.



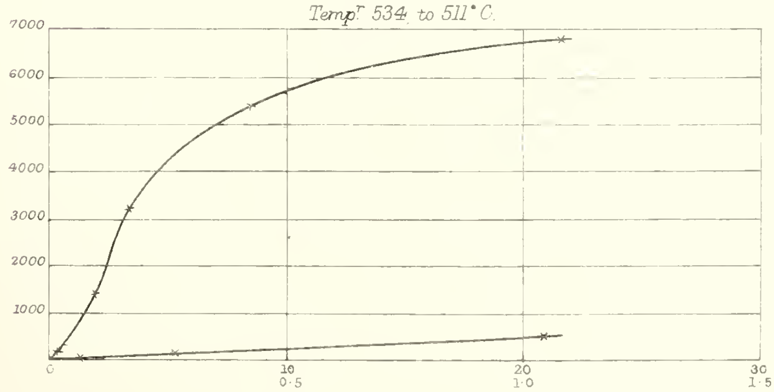
Curve XXXI.
Temp.r 657 to 661°C.



Curve XXXII.
Temp.r 561 to 534°C.



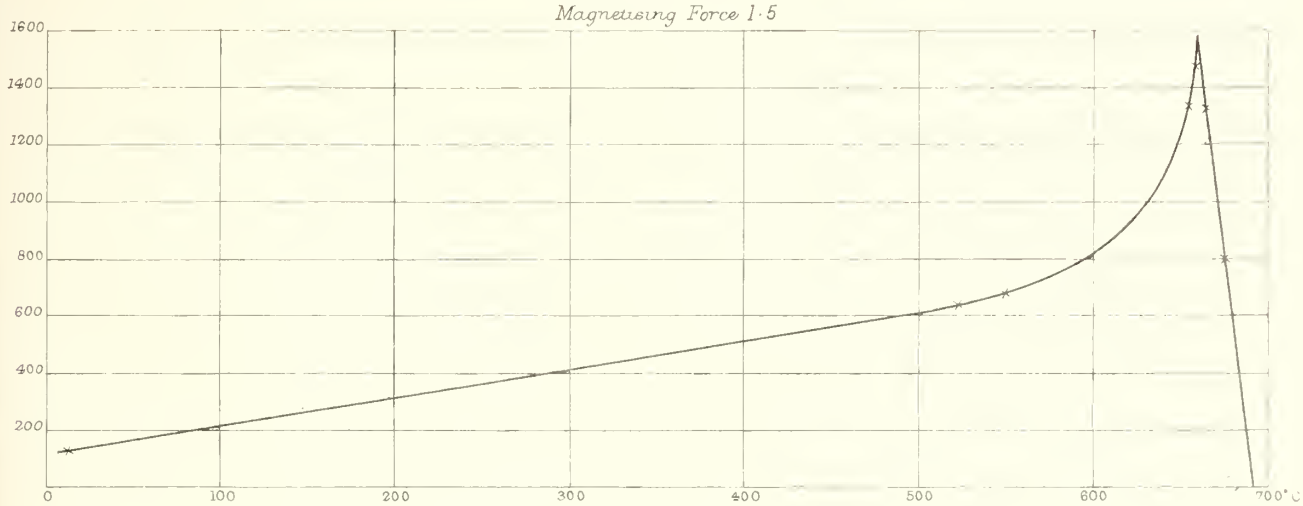
Curve XXXIII.
Temp.r 534 to 511°C.



Whitworth's Hard Steel.

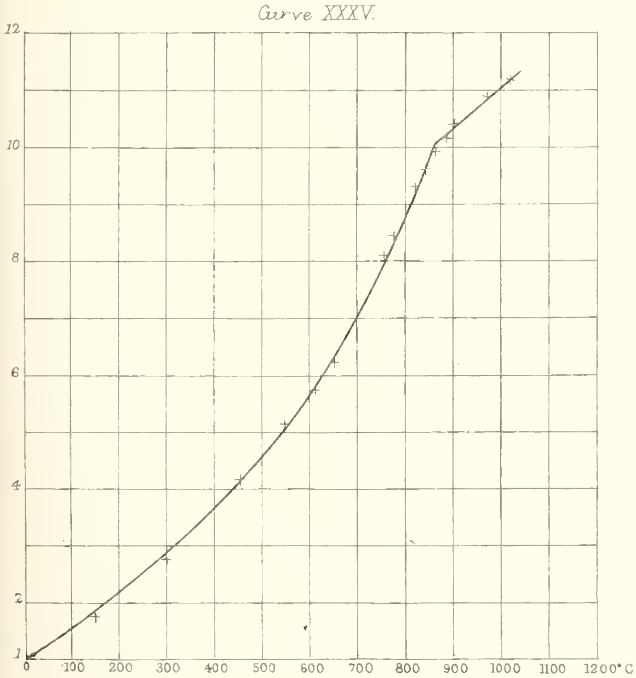
Curve XXXIV.

Magnetising Force 1·5



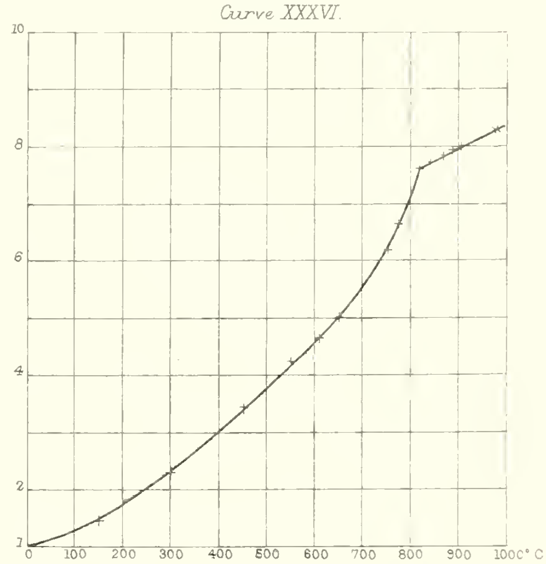
Soft Iron wire.

Curve XXXV.



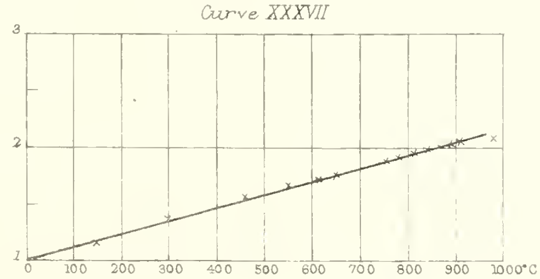
[illegible]

Curve XXXVI.



Manganese Steel Wire.

Curve XXXVII.



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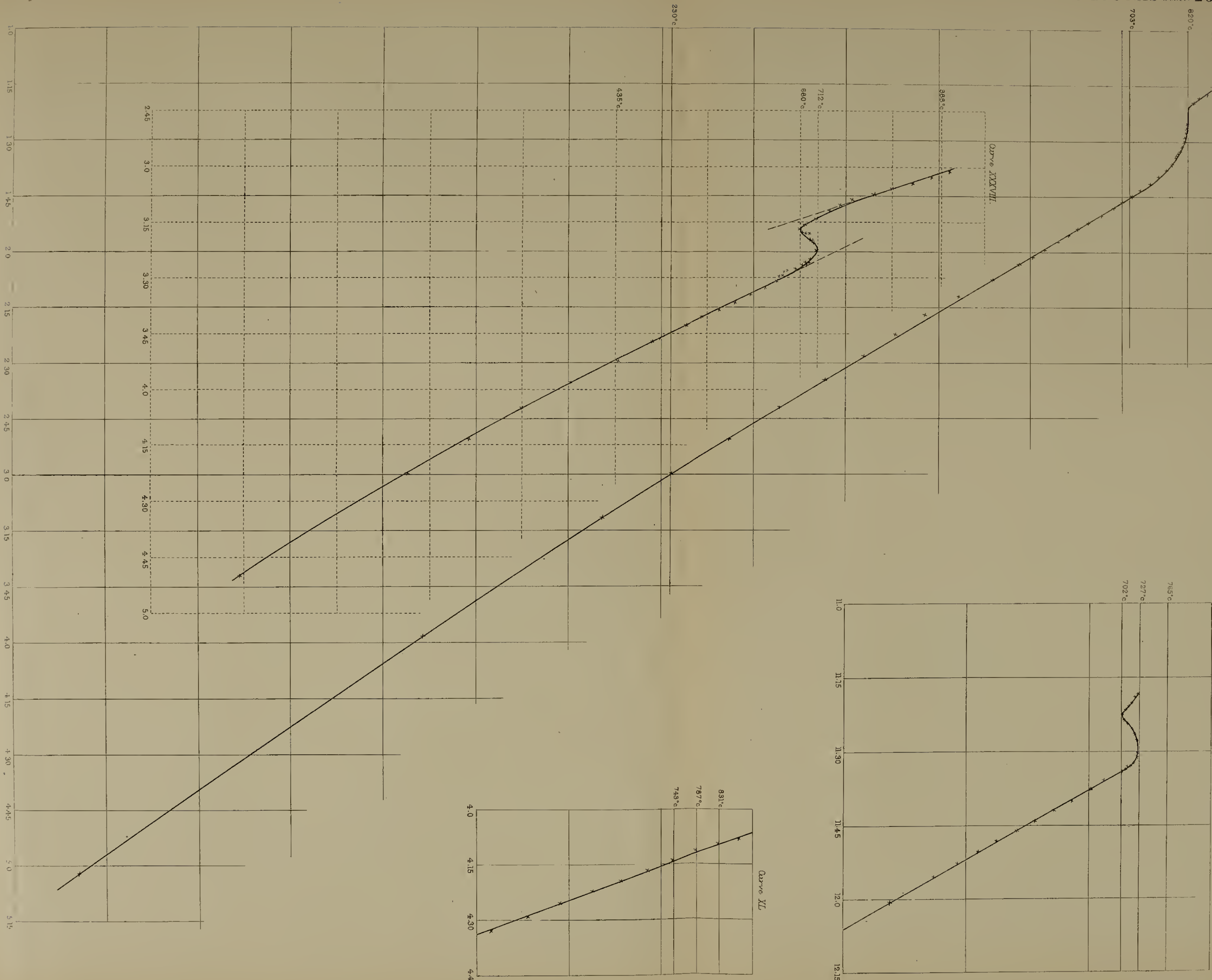

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AB. Hollandsche Maatschappij der Wetenschappen.

p. Musée Teyler.

Leyden.

AB. University.

Luxembourg.

p. Société des Sciences Naturelles.

Rotterdam.

AB. Bataafsch Genootschap der Proefondervindelijke Wijsbegeerte.

Utrecht.

AB. Provinciaal Genootschap van Kunsten en Wetenschappen.

New Zealand.

Wellington.

AB. New Zealand Institute.

Norway.

Bergen.

AB. Bergenske Museum.

Christiania.

AB. Kongelige Norske Frederiks Universitet.

Tromsoe.

p. Museum.

Trondhjem.

AB. Kongelige Norske Videnskabers Selskab.

Nova Scotia.

Windsor.

p. King's College Library.

Portugal.

Coimbra.

AB. Universidade.

Lisbon.

AB. Academia Real das Sciencias.

p. Secção dos Trabalhos Geologicos de Portugal.

Russia.

Dorpat.

AB. Université.

Kazan.

AB. Imperatorsky Kazansky Universitet.

Kharkoff.

p. Section Médicale de la Société des Sciences Expérimentales, Université de Kharkow.

Kieff.

p. Société des Naturalistes.

Russia (continued).

Moscow.

AB. Le Musée Publique.

B. Société Impériale des Naturalistes.

Odessa.

p. Société des Naturalistes de la Nouvelle-Russie.

Pulkowa.

A. Nikolai Haupt-Sternwarte.

St. Petersburg.

AB. Académie Impériale des Sciences.

AB. Comité Géologique.

p. Compass Observatory.

A. L'Observatoire Physique Central.

Scotland.

Aberdeen.

AB. University.

Edinburgh.

p. Geological Society.

p. Royal College of Physicians (Research Laboratory).

p. Royal Medical Society.

A. Royal Observatory.

p. Royal Physical Society.

p. Royal Scottish Society of Arts.

AB. Royal Society.

Glasgow.

AB. Mitchell Free Library.

p. Philosophical Society.

Servia.

Belgrade.

p. Académie Royale de Serbie.

Spain.

Cadiz.

A. Observatorio de San Fernando.

Madrid.

p. Comisión del Mapa Geológico de Espãna.

AB. Real Academia de Ciencias.

Sweden.

Gottenburg.

AB. Kongl. Vetenskaps och Vitterhets Samhälle.

Lund.

AB. Universitet.

Stockholm.

A. Acta Mathematica.

AB. Kongliga Svenska Vetenskaps-Akademie.

AB. Sveriges Geologiska Undersökning.

Upsala.

AB. Universitet.

Switzerland.

Basel.

p. Naturforschende Gesellschaft.

Switzerland (continued).

Bern.

AB. Allg. Schweizerische Gesellschaft.

p. Naturforschende Gesellschaft.

Geneva.

AB. Société de Physique et d'Histoire Naturelle.

AB. Institut National Genevois.

Lausanne.

p. Société Vaudoise des Sciences Naturelles.

Neuchâtel.

p. Astronomische Mittheilungen (Professor R. Wolf).

p. Société des Sciences Naturelles.

Zürich.

AB. Das Schweizerische Polytechnikum.

p. Naturforschende Gesellschaft.

Tasmania.

Hobart.

p. Royal Society of Tasmania.

United States.

Albany.

AB. New York State Library.

Annapolis.

AB. Naval Academy.

Baltimore.

AB. Johns Hopkins University.

Berkeley.

p. University of California.

Boston.

AB. American Academy of Sciences.

B. Boston Society of Natural History.

A. Technological Institute.

Brooklyn.

AB. Brooklyn Library.

Cambridge.

AB. Harvard University.

Chapel Hill (N.C.).

p. Elisha Mitchell Scientific Society.

Charleston.

p. Elliott Society of Science and Art of South Carolina.

Chicago.

AB. Academy of Sciences.

Davenport (Iowa).

p. Academy of Natural Sciences.

Madison.

p. Wisconsin Academy of Sciences.

Mount Hamilton (California).

A. Lick Observatory.

New Haven (Conn.).

AB. American Journal of Science.

AB. Connecticut Academy of Arts and Sciences.

New York.

p. American Geographical Society.

United States (continued).

New York (continued).

- *p.* American Museum of Natural History.
- *p.* New York Academy of Sciences.
- *p.* New York Medical Journal.
- *p.* School of Mines, Columbia College.

Philadelphia.

- AB. Academy of Natural Sciences.
- AB. American Philosophical Society.
- *p.* Franklin Institute.
- *p.* Wagner Free Institute of Science.

St. Louis.

- *p.* Academy of Science.

Salem (Mass.).

- *p.* Essex Institute.
- AB. Peabody Academy of Science.

United States (continued).

San Francisco.

- AB. California Academy of Sciences.

Washington.

- *p.* Department of Agriculture.
- A. Office of the Chief Signal Officer.
- AB. Patent Office.
- AB. Smithsonian Institution.
- AB. United States Coast Survey.
- *p.* United States Commission of Fish and Fisheries.
- AB. United States Geological Survey.
- A. United States Naval Observatory.

West Point (N Y.)

- AB. United States Military Academy.

Adjudication of the Medals of the Royal Society for the year 1889, by the President and Council.

The Copley Medal to the Rev. George Salmon, D.D., F.R.S., for his various Papers on subjects of Pure Mathematics, and for the valuable Mathematical Treatises of which he is the Author.

A Royal Medal to Walter Holbrook Gaskell, F.R.S., for his Researches in Cardiac Physiology, and his important Discoveries in the Anatomy and Physiology of the Sympathetic Nervous System.

A Royal Medal to Thomas Edward Thorpe, F.R.S., for his Researches on Fluorine Compounds, and his Determination of the Atomic Weights of Titanium and Gold.

The Davy Medal to William Henry Perkin, F.R.S., for his Researches on Magnetic Rotation in relation to Chemical Constitution.

The Bakerian Lecture, "A Magnetic Survey of the British Isles for the Epoch January 1, 1886," was delivered by Professor A. W. Rücker, F.R.S., and Professor T. E. Thorpe, F.R.S.

The Croonian Lecture, "Les Inoculations Préventives," was delivered by Dr. E. Roux.

CONTENTS.

(B.)

VOL. 180.

LIST OF ILLUSTRATIONS.

PHILOSOPHICAL TRANSACTIONS.

I. *On the present Position of the Question of the Sources of the Nitrogen of Vegetation, with some new Results, and preliminary Notice of new Lines of Investigation.*

By Sir J. B. Lawes, *Bart., LL.D., F.R.S., and Professor* J. H. Gilbert, *LL.D., F.R.S.*

Received, Part 1, July 20, 1887; Parts 2 and 3, May 3, 1888—Read May 17, 1888.

Contents.

Introduction.

A GREAT part of this paper was written in the spring of 1886, but its completion was unavoidably delayed. This has, however, not been altogether without advantage. Thus, in the first place, at the *Naturforscher-Versammlung*, held in Berlin, in September, 1886, the greater part of the sittings of two days was devoted, in the Section of *Landwirthschaftliches Versuchs-Wesen*, to the discussion of the subject from various points of view, one of ourselves taking part; and as it seemed desirable that the results and conclusions then brought forward by others should be considered, we have waited for the publication of the exact figures in some cases. Again, since the Berlin meeting, M. BERTHELOT has published some further results, to which reference should be made. And lastly, we are now enabled to give further new results of our own.

In Part 2 of the 'Philosophical Transactions' for 1861, a paper was given, by ourselves and the late Dr. PUGH, "*On the Sources of the Nitrogen of Vegetation, with special reference to the question whether plants assimilate free or uncombined Nitrogen.*" Since that time, the question of the sources of the nitrogen of vegetation has continued to be the subject of much discussion, and also of much experimental enquiry, both at Rothamsted and elsewhere. Until quite recently, the controversy has chiefly been as to whether plants directly assimilate the free nitrogen of the atmosphere; but, during the last few years, the discussion has assumed a somewhat different aspect. The question still is whether the free nitrogen of the air is an important source of the nitrogen of vegetation; but whilst few now adhere to the view that the higher chlorophyllous plants directly assimilate free nitrogen, it is, nevertheless, assumed to be brought under contribution in various ways—coming into combination within the soil, under the influence of electricity, or of micro-organisms, or of other low forms, and so indirectly serving as an important source of the nitrogen of plants of a higher order. Several of the more important of the investigations in the lines here indicated seem to have been instigated by the assumption that compensation must be found for the losses of combined nitrogen which the soil sustains by the removal of crops, and also for the losses which result from the liberation of nitrogen from its combinations under various circumstances.

At the meeting of the American Association for the Advancement of Science, held at Montreal in 1882, we gave a paper entitled—"*Determinations of Nitrogen in the soils of some of the Experimental Fields at Rothamsted, and the bearing of the results on the question of the Sources of the Nitrogen of our crops;*" and again, at the Meeting of the British Association, held at Montreal, in 1884, we gave further results on the subject, in a paper—"*On some points in the Composition of Soils; with results illustrating the Sources of the Fertility of Manitoba Prairie Soils.*"*

It is the object of the present paper to summarise some of our own more recently published results bearing on various aspects of the subject, to put on record additional results, to give a preliminary notice of new lines of enquiry, and to discuss the evidence so adduced with reference to the results and conclusions of others which have recently been put forward, as above alluded to.

PART I.

Results relating to other Sources than Free Nitrogen.

1. *Summary of previously published Rothamsted Results chiefly relating to nitric acid in soils and subsoils.*

Before directing attention to the new results it will be desirable, with the view of bringing out their significance the more clearly, to give a brief *résumé* of our previous results and conclusions bearing on the subject.

In the last mentioned paper, after reviewing previously existing evidence as to the sources of the nitrogen of crops, we concluded, as we had done before, that, excepting the small amount of combined nitrogen annually coming down in rain, and the minor aqueous deposits from the atmosphere, the source of the nitrogen of vegetation was, substantially, the stores within the soil and subsoil, whether derived from previous accumulations or from recent supplies by manure.

Results of determinations of the nitrogen as nitric acid, in soils of known history as to manuring and cropping, and to a considerable depth, in some cases to 108 inches, were given, which showed that the amount of nitrogen in the soil in that form was much less after the growth of a crop than under corresponding conditions without a crop. In the case of gramineous crops, the evidence pointed to the conclusion that most, if not the whole, of their nitrogen was taken up as nitric acid from the soil and

* Afterwards revised and published in the 'Transactions of the Chemical Society' for June, 1885.

subsoil. In the case of leguminous crops again, the evidence was in favour of the supposition that, in some cases, the whole of the nitrogen had been taken up as nitric acid, but that in others that source was inadequate.

The results further showed that, under otherwise parallel conditions, there was very much more nitrogen as nitric acid, in soils and subsoils, down to a depth of 108 inches, where leguminous than where gramineous crops had for some time been grown.

Table II., p. 6, gives, in a condensed form, the most important of the previously published results relating to this branch of the subject. It shows the amounts of nitrogen as nitric acid, calculated *per acre*, in lbs., according to determinations made in samples of soil collected in 1883, at each of 12 depths of 9 inches each, that is down to 108 inches in all, under the following conditions :—

1. Where wheat had been grown in alternation with fallow, without any manure, since 1850 ; that is for more than 30 years.

2. On a plot where mineral manures had occasionally been applied, but no nitrogen for more than 30 years; where *Trifolium pratense* had been sown 12 times during the 30 years, 1848–77, but in 8 out of the last 10 trials the plant had died off in the winter or spring succeeding the sowing, in 4 without giving any crop, and in the other 4 yielding very small cuttings; and where, consequent on the failure of the clover, during the 30 years 1 crop of wheat and 5 of barley had been taken, and the land had been 12 years left fallow. *Trifolium repens* was then sown, namely, in 1878, 1880, 1881, and 1883; and it yielded crops in 1879, 1881, and 1882, but none in 1883, when the soil samples were taken.

3. On 2 plots with the same previous history as that of the *Trifolium repens* plots prior to 1878, but where *Vicia sativa* has since been sown; and where, notwithstanding the previous failure of the red clover, the *Vicia* yielded fair crops in 1878, 1879, 1880, and 1881, and large crops in 1882, and in 1883, before the soil samples were taken.

We are now able to give amended, and more complete estimates, of the amounts of nitrogen removed per acre in the produce from the different plots, both during the preliminary period up to 1877 inclusive, and during the period of the direct experiments with the various leguminous plants. As each leguminous plot is only $\frac{1}{84}$ of an acre in area, it is obvious that calculations of the produce, per acre, can only be approximately correct. We have, therefore, in each case taken the average yield of three plots, 4, 5, and 6, with one and the same plant, but with somewhat different mineral manures. Thus, plot 4 has received a mixture of superphosphate of lime and potash sulphate; plot 5 a mixture of potash, soda, and magnesia salts; and plot 6 the same as plot 5, with superphospate of lime in addition. Direct determinations of nitrogen have, in almost all cases, been made ; and we believe that the estimates of it per acre may be considered as close approximations to the truth, and at any rate quite sufficiently so for the purposes of illustration and of argument for which they are used. The results are given in the following Table (I.).

Table I.—Estimated yield of Nitrogen per acre, in lbs., in wheat alternated with fallow, and in various leguminous crops without nitrogenous manure.

	Unmanured.	Mineral manures only.	
	Fallow-wheat.	*Trifolium repens.*	*Vicia sativa.*
Preliminary period:—Wheat and fallow, 27 years, 1851–77. Red clover, &c., &c., 29 years, 1849–77.			
	lbs.	lbs.	
Average per acre per annum	17	32	
Experimental period.			
	lbs.	lbs.	lbs.
1878	29	0	51
1879	Fallow	82	46
1880	24	0	58
1881	Fallow	8	65
1882	18	74	146
1883	Fallow	0	101
Total, 6 years	71	164	467
Average annual	12	27	78

The next Table (II.) shows the amounts of nitrogen as nitric acid found in the soils and subsoils of the several plots under the conditions above described.

TABLE II.—Nitrogen as Nitric Acid per acre, lbs., in the soils and subsoils of some experimental plots, without nitrogenous manure for more than 30 years. Hoosfield, Rothamsted. Soil samples collected July 17–26, 1883.

Depths.	Unmanured.	Series I. Mineral manures only.			*Trifolium repens.* + or –. Wheat land.	+ or –. *Trifolium repens.*	
	Wheat-fallow land.	*Trifolium repens.* Plot 4.	*Vicia sativa.* Plot 4.	*Vicia sativa.* Plot 6.		*Vicia sativa.* Plot 4.	*Vicia sativa.* Plot 6.
inches.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
1– 9	19·85	30·90	12·16	10·22	+ 11·05	– 18·74	– 20·68
10– 18	8·05	27·73	4·11	2·72	+ 19·68	– 23·62	– 25·01
19– 27	2·47	8·44	1·37	1·08	+ 5·97	– 7·07	– 7·36
28– 36	2·70	7·64	1·67	1·52	+ 4·94	– 5·97	– 6·12
37– 45	1·62	9·07	4·58	2·51	+ 7·45	– 4·49	– 6·56
46– 54	3·57	8·77	6·37	4·42	+ 5·20	– 2·40	– 4·35
55– 63	3·84	7·92	7·16	4·52	+ 4·08	– 0·76	– 3·40
64– 72	2·28	8·34	5·95	4·92	+ 6·06	– 2·39	– 3·42
73– 81	1·48	8·27	4·54	4·81	+ 6·79	– 3·73	– 3·46
82– 90	1·76	9·95	5·32	5·14	+ 8·19	– 4·63	– 4·81
91– 99	2·94	9·16	5·66	6·40	+ 6·22	– 3·50	– 2·76
100–108	1·84	9·51	5·32	6·46	+ 7·67	– 4·19	– 3·05
Total	52·40	145·70	64·21	54·72	+ 93·30	– 81·49	– 90·98

These wheat-fallow and leguminous plots are absolutely adjoining; and by their previous treatment their surface soils had become extremely poor in nitrogen. The results have been discussed in detail in the paper in the Transactions of the Chemical Society, and must only be briefly summarised here. Table I. shows that, for about 30 years, the *Trifolium repens* soil had yielded in crops nearly twice as much nitrogen per acre as the wheat-fallow soil. Yet it is seen that, whilst the wheat-fallow soil contained, down to the depth of 9 feet, only 52·4 lbs. of nitrogen as nitric acid per acre, the *Trifolium repens* soil contained 145·7 lbs. to the same depth. In other words—the *Trifolium repens* soil, from which so much more nitrogen had been removed, contained 93·3 lbs. more nitrogen as nitric acid than the wheat-fallow soil.

Now, excepting that the leguminous crop soil had received mineral manures, and the wheat soil had not, the characteristic difference in the history of the two plots was, that the one had grown a gramineous crop alternately with fallow for more than 30 years, and the other had, during the same period, besides growing 6 gramineous crops, and being frequently fallow, been sown 12 times with red clover, and, during the immediately preceding 6 years, 4 times with white clover. That is to say, the chief distinction was, that the one plot had, especially in the earlier and the later years, grown a leguminous crop, whilst the other had not; and it is under these circumstances that the leguminous crop soil is found to contain, down to 108 inches, nearly 3 times as much nitrogen as nitric acid as the gramineous crop soil.

The difference is the greatest near the surface, but it is very considerable down to the lowest depth. Hence it is obvious that any loss by drainage would be much the greater from the *Trifolium* plot, and so the difference between the two plots was probably in reality greater than the figures show. In both cases the actual amount is the greatest near the surface, indicating more active nitrification; and the excess is much the greater in the *Trifolium repens* soil, doubtless due to more nitrogenous crop-residue from the leguminous than from the gramineous crop. Indeed, about 74 lbs. of nitrogen had been removed in the *Trifolium repens* crop in 1882, and none in 1883, the year of the soil collections. On the other hand, only about one-fourth as much was removed in the wheat crop of 1882, and the land was fallow in 1883. Unless, however, there was considerably more nitrogen in the crop-residue than in the removed crops of the *Trifolium repens*, the excess of 93 lbs. of nitrogen as nitric acid found in the *Trifolium repens* soil, together with the increased amount lost by drainage, could not have had its source entirely in the nitrification of recent nitrogenous crop-residue. Some of the increased amount in the lower layers was indeed doubtless due to washing down from the surface. But as, notwithstanding much more nitrogen had been removed in crops from the leguminous than from the gramineous crop soil during the previous 30 years, the surface soil of the leguminous plots remained slightly richer in nitrogen than that of the gramineous plot, it cannot be supposed that the whole of the nitrogen of the crop, and of the nitric acid found, had its origin in the surface soil. If, therefore, nitrogen has not been derived from the atmosphere, the conclusion must be that some has come from the subsoil.

The indication was that nitrification had been more active under the influence of the leguminous than of the gramineous growth and crop-residue. In fact, under the influence of leguminous growth, not only will there be increased nitrogenous matter for nitrification, but it would seem that the development of the nitrifying organisms will be favoured. The question is, therefore, whether part of the result be not due to the passage downwards of the nitrifying organisms, and the nitrification of the nitrogen of the subsoil.

The alternative was suggested, that the soil and subsoil might still be the source of the nitrogen of the crops, but that the plants may take up, at any rate part of their supply, in other forms than as nitric acid—as ammonia, or as organic nitrogen, for example. It was pointed out that fungi do take up both organic carbon and organic nitrogen; but that, whilst existing direct experimental evidence was conflicting as to whether green leaved plants even assimilate carbon taken up by their roots as carbonic acid, the evidence was even less conclusive as to whether they take up either organic carbon or organic nitrogen as such from the soil. To this question we shall recur further on.

The next point is to compare the amount of nitrogen as nitric acid found in the *Vicia sativa* soils with that in the *Trifolium repens* soil. In the first place it is to be observed, that whilst from the *Trifolium repens* plot only 164 lbs. of nitrogen had been removed in the crops during the five years to 1882 inclusive, from the *Vicia sativa* plots 366 lbs. had been removed during the same period. Further, whilst from the *Trifolium repens* plot there was no nitrogen removed in 1883, the year of soil sampling, from the *Vicia* plots 101 lbs. were removed in the crop just before the soil sampling. It is seen that, under these circumstances, there remained, per acre, in one of the *Vicia* plots 81·5 lbs., and in the other 91 lbs., less nitrogen as nitric acid to the depth examined than in the *Trifolium repens* soil.

If we confine attention only to the amount of nitrogen removed in the *Vicia* crops in the year of the soil sampling, and assume that there had been only as much at the disposal of the plant as in the case of the *Trifolium* plot, it is obvious that the deficiency in the *Vicia* soils very nearly corresponds with the amount removed in the crop, which was about 100 lbs. Indeed, it may safely be concluded that most, if not the whole, of the nitrogen of the *Vicia* crops had been taken up as nitric acid.

But there had probably been more loss by drainage from the *Trifolium* plot without growth than from the *Vicia* plots with growth, and with, at the same time, much more upward passage and evaporation.

It must also be borne in mind, that the *Vicia* plots had, during the preceding 5 years, 1878 to 1882, yielded an average of more than 70 lbs. of nitrogen per acre, and in the immediately preceding year (1882), 146 lbs. Further, the amounts taken up by the plants each year must have been much greater than the amounts removed in the crops; for there must have been annually a large crop-residue, which would yield nitric acid for succeeding crops. Much of these large amounts of nitrogen must obviously have had some other source than the original surface soil, since it gained rather than lost under the treatment. If this source were not the atmosphere, but the subsoil, it must have been taken up, either as nitric acid, as some other product of the change of the organic nitrogen of the subsoil, or as organic nitrogen itself. Further, as the *Vicia* crops were large in the previous year, 1882, so also would their nitrogenous crop-residue be large, and contribute correspondingly large amounts of nitric acid for the crops of 1883. But the crops of 1883 were also large, and they, in their turn, would leave correspondingly large nitrogenous crop-residues; leaving a large proportion of the amount of nitrogen removed in the crops to be otherwise provided for than by previous residue.

Lastly in reference to these experiments, it is seen that at each of the 12 depths, down to 108 inches, the *Vicia* plots where there had been growth, contained less nitric-nitrogen than the *Trifolium repens* plot where there had been no growth. The difference is much the greatest in the first 18 inches, within which the *Vicia* throws out by far the larger amount of root; but it is very distinct below this point, and the supposition is that, under the influence of the growth of the *Vicia*, water had been brought up from below, and with it nitric acid. In fact, compared with the *Trifolium repens* plot, the mean for the two *Vicia* plots showed less water in the soil down to

108 inches, corresponding to between 6 and 7 inches of rain, or to between 600 and 700 tons of water per acre.

After this summary of previously published results we may now turn to the consideration of new results of the same kind.

2. *New Determinations of Nitric Acid in Soils and Subsoils.*

The plots experimented upon are in the same series, with the same previous history, as those already referred to. *Trifolium repens* was again selected as the weak and superficially rooting plant; *Melilotus leucantha* was taken as a deeper and stronger rooting one; and *Medicago sativa*, or lucerne, as a still deeper and still stronger rooting plant. Samples of soil were taken at the end of July and the beginning of August, 1885, from 2 places on each plot, and in each case, as before, to 12 depths of 9 inches each, equal to a total depth of 108 inches or 9 feet.

The following table (III.) shows the estimated yields of nitrogen per acre in the different crops during the experimental period from 1878 to 1885, the year of soil sampling, inclusive. The yields during the preliminary period have been already given in Table I.

TABLE III.—Estimated yield of Nitrogen per acre, in lbs., in wheat alternated with fallow, and in various leguminous crops, without nitrogenous manure.

	Unmanured.	Mineral manures only.		
	Fallow-wheat.	*Trifolium repens.*	*Melilotus leucantha.*	*Medicago sativa.*
	lbs.	lbs.	lbs.	lbs.
1878	29	0	53	Not sown
1879	Fallow	82	130	0
1880	24	0	36	28
1881	Fallow	8	60	28
1882	18	74	145	111
1883	Fallow	0	27	143
1884	29	0	56	337
1885	Fallow	97	58	233*
Total, 8 years . .	100	261	565	880
Average annual .	12	33	71	(110)

Thus, the wheat plot was again fallow when sampled; the total yield of nitrogen in the crops in the 8 years was only 100 lbs. per acre, and the average annual yield little more than 12 lbs.

The *Trifolium repens* plots, after giving no crop in either 1883 or 1884, yielded produce containing nearly 100 lbs. of nitrogen in 1885, before the soil sampling; the total yield of nitrogen in the 8 years was 261 lbs., and the average annual yield 33 lbs.

* First and second crops only; a third crop, cut after the soil sampling, yielded 37 lbs. nitrogen.

The deeper rooting, and freer growing *Melilotus leucantha* gave more or less produce in each year of the eight, large crops in 1879 and 1882, a total yield of nitrogen over the eight years of 565 lbs. per acre, and an average annual yield of 71 lbs.

Lastly, the still deeper rooting, and still freer growing *Medicago sativa*, sown first in 1879, gave no crop in that year, only small crops in 1880 and 1881, and then rapidly increasing amounts, until the yield of nitrogen was estimated at 337 lbs. per acre in 1884, and at 233 lbs. in 1885 before the soil sampling, and 37 lbs. afterwards, making a total for that year of 270 lbs. The total yield of nitrogen in the 6 years, prior to the soil sampling, was 880 lbs.; giving an average over the 8 years of 110 lbs., or over the 6 years when there was any crop, of 147 lbs. of nitrogen per acre per annum.

Table IV. shows the amounts of nitrogen as nitric acid found in the soils and subsoils of the different plots—in all cases calculated into lbs. per acre.

TABLE IV.—Nitrogen as Nitric Acid per acre, lbs., in the soils and subsoils of some experimental plots, without nitrogenous manure, for more than 30 years. Hoosfield, Rothamsted. Samples collected July 29 to August 14, 1885.

Depths.	Unmanured.	Series I. Mineral manures only.			*Trifolium repens.* + or − Wheat land.	+ or − *Trifolium repens.*	
	Wheat-fallow land.	*Trifolium repens.* Plot 5.	*Melilotus leucantha.* Plot 5.	*Medicago sativa.* Plot 5.		*Melilotus leucantha.* Plot 5.	*Medicago sativa.* Plot 5.
inches.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
1– 9	17·44	11·50	4·35	8·88	− 5·94	− 7·15	− 2·62
10– 18	3·67	1·38	1·40	1·11	− 2·29	+ 0·02	− 0·27
19– 27	2·76	0·90	2·12	0·78	− 1·86	+ 1·22	− 0·12
28– 36	2·16	1·86	2·94	0·81	− 0·30	+ 1·08	− 1·05
37– 45	1·68	7·08	5·22	0·99	+ 5·40	− 1·86	− 6·09
46– 54	1·47	11·31	6·21	0·93	+ 9·84	− 5·10	−10·38
55– 63	1·77	13·14	7·95	0·57	+11·37	− 5·19	−12·57
64– 72	1·83	12·63	10·08	0·81	+10·80	− 2·55	−11·82
73– 81	2·29	11·19	9·66	0·70	+ 8·90	− 1·53	−10·49
82– 90	2·01	10·70	9·16	0·61	+ 8·69	− 1·54	−10·09
91– 99	1·98	11·08	8·83	0·44	+ 9·10	− 2·25	−10·64
100–108	2·06	9·96	10·12	0·41	+ 7·90	+ 0·16	− 9·55
Total	41·12	102·73	78·04	17·04	+61·61	−24·69	−85·69
Summary and control.							
1– 9	17·44	11·50	4·35	8·88	− 5·94	− 7·15	− 2·62
10– 18	3·67	1·38	1·40	1·11	− 2·29	+ 0·02	− 0·27
Mixture of 19–108 inches	20·63	88·02	73·21	6·97	+67·39	−14·81	−81·05
Total	41·74	100·90	78·96	16·96	+59·16	−21·94	−83·94

The determinations of nitric acid in the soil extracts, the results of which are recorded in the table, as well as those given in Table II., were made in the Rothamsted Laboratory by Mr. D. A. Louis, by Schlœsing's method, as nitric oxide by the reaction with ferrous salts. For each of the twelve depths a mixture of the samples from the two holes was prepared, and in each of these mixtures duplicate determinations of nitric acid were made. As a control, determinations were also made in a mixture of the samples from the 10 lower depths, the third to the twelfth inclusive, and the results are given at the foot of the table.

The first point to remark is, that there was much less nitrogen as nitric acid in the *Trifolium repens* soil in 1885, after the removal of nearly 100 lbs. of nitrogen in the crops, than in 1883, when no crop had grown. The deficiency is the greatest in the 2 upper layers, but it extends to the fifth depth, amounting to that point, which represents the range of the direct or indirect action of the superficial roots of the plant, to about 61 lbs. Below the range of this action, however, there is even more nitrogen as nitric acid in 1885 than 1883; due doubtless in part to percolation from above during the two preceding seasons without growth, and possibly in part to percolation of the nitrifying organisms, and nitrification of the nitrogen of the subsoil.

Let us now turn to the results obtained on the *Melilotus leucantha* plot. As shown in Table III., it is estimated that, in 1882 as much as 145 lbs. of nitrogen was removed in the crop, and samples of soil taken that autumn, to the depth of 6 times 9 inches, or 54 inches in all, showed only 8·45 lbs. of nitrogen as nitric acid remaining, which was 17·8 lbs. less than was found to the same depth in the *Trifolium repens* plot which had yielded only 74 lbs., or only about half as much in the crop as the *Melilotus*.

After 1882, however, the produce of the *Melilotus* declined very much, and in 1885 the yield of nitrogen in the crop was estimated at only 58 lbs., against 97 lbs. estimated to have been removed in the *Trifolium repens* crop. Under these circumstances the *Trifolium repens* soil shows even rather less nitric acid than the *Melilotus* soil, in the second, third, and fourth depths, which comprise the chief range of action of the *Trifolium repens* roots. At every depth below the fourth, however (except the 12th, where the difference is very small), there is notably less nitrogen as nitric acid in the *Melilotus* than in the *Trifolium repens* soil, the *Melilotus* having yielded so much more in its crops in the preceding years than the *Trifolium repens*. To the total depth of 108 inches there was 24·69 lbs. less nitric nitrogen remaining in the *Melilotus* than in the *Trifolium repens* soil.

Admittedly we cannot know what was the stock of nitric nitrogen in either soil at the commencement of the growth of the season. But as during the 8 years 565 lbs. of nitrogen were removed in the *Melilotus* crops, against only 261 lbs. in the *Trifolium repens*, or more than twice as much in the *Melilotus* as in the *Trifolium repens*, it may be supposed that the *Melilotus* would both leave more nitrogenous crop-residue for nitrification, and with its deeper roots, would each year the more exhaust the nitric nitrogen especially of the lower layers. Hence, notwithstanding the much lower yield of nitrogen in the *Melilotus* than in the *Trifolium repens* crop in 1885, the lower layers of the *Melilotus* soil contained less nitric acid than those of the *Trifolium repens* soil. There can, indeed, be no doubt, that the *Melilotus* derived at any rate much of its nitrogen from nitric acid, either within the actual range of its roots, or within the range of their action in causing the passage upwards of water with its dissolved contents. Still, the figures show, that with the comparatively limited growth in the recent years, there remained per acre about 56 lbs. of nitrogen as nitric acid in the 6 lower depths of the *Melilotus* soil.

But by far the most striking results in the Table are those relating to the *Medicago sativa* (lucerne) soil, and to the comparison between the amounts of nitric nitrogen in the soil of the shallow rooting and weakly growing *Trifolium repens* and those in the soil of the very deep and strong rooted, and very free growing lucerne.

Table III. shows that the estimated yields of nitrogen per acre in the lucerne were in the 6 years, 1880–1885, respectively as follows:—28 lbs., 28 lbs., 111 lbs., 143 lbs., 337 lbs., and 233 lbs. That is to say, with the increasing root range, and consequently increased command of the stores of the soil and subsoil, the yield of nitrogen in the crop increased from 28 lbs. in the first and second years, to 337 in the fifth year; declining, however, somewhat in the sixth year, 1885, and it did so still further in 1886.

It is seen that under these circumstances of very large yields of nitrogen in the crops, there is, at every one of the twelve depths, less, and at most very much less nitrogen as nitric acid remaining in the soil than where so much less nitrogen had been removed in the *Trifolium repens* crops. The difference is distinct even in the upper layers, but it is very striking in the lower depths. Thus, there is, on the average, not one-twelfth as much nitric nitrogen in the lower ten depths of the deep rooting and high nitrogen-yielding *Medicago sativa* soil, as in those of the shallow rooting and comparatively low nitrogen-yielding *Trifolium repens* soil. Indeed, the nitric acid is nearly exhausted in the deep rooting *Medicago sativa* plot; there remaining, to the total depth of 9 feet, only about 17 lbs. of nitric nitrogen against more than 100 lbs. to the same depth in the *Trifolium repens* soil. The total deficiency of nitric nitrogen in the *Medicago* as compared with the *Trifolium repens* soil, is seen to be 85·69 lbs. according to one set of determinations, and 83·94 lbs. according to the other.

As already said, we cannot know what was the stock of nitric nitrogen in the soil at the commencement of the growth of the season, or the amount formed during the growing period. But with so much more *Medicago* growth for several previous years, it seems reasonable to assume that there would be much more nitrogenous crop-residue for nitrification than in the case of the *Trifolium repens* plot.

But even supposing, for the sake of illustration, that each year's growth would leave crop-residue yielding an amount of nitrogen as nitric acid for the next crop, or succeeding crops, approximately equal to the amount which had been removed in the crop, the increasing amounts of nitrogen yielded in the crops from year to year could not be so accounted for; and there would remain the amount of nitrogen in the crop-residue itself, still to be provided in addition. In fact, assuming the proportion of nitrogen in the crop-residue to that in the removed crop to be as supposed in the above illustration, nearly 700 lbs. of nitrogen would have been required for the *Medicago* crop and crop-residue of 1884; or if we assume the nitrogen in the residue to be only half that in the crop, about 500 lbs. would have been required. Doubtless, however, some of the nitrogenous crop-residue would accumulate from year to year.

The results can leave no doubt that the *Trifolium repens*, the *Melilotus leucantha*, and the *Medicago sativa*, have each taken up much nitrogen from nitric acid within the soil. But, at any rate so far as the *Medicago* is concerned, there is nothing in the figures to justify the conclusion that the whole of its nitrogen can have been so derived. It is obvious that if nitric acid were the source of the whole there must have been a great deal formed by the nitrification of the nitrogen of the subsoil. The alternative is—provided the atmosphere be not the source—that the deep-rooted plant takes up nitrogen from the subsoil in some other way.

3. *Percentage of Nitrogen in the Surface Soils of the Experimental Plots.*

It has been stated in general terms that, although much more nitrogen had been removed from the leguminous crop soils than from the fallow-wheat land, for nearly 30 years prior to the commencement of the experiments with various leguminous plants in 1878, yet the leguminous crop surface soil remained rather richer in nitrogen than the fallow-wheat soil. It has also been stated that, during the subsequent years of experiment with the various leguminous plants, the surface soil had gained rather than lost nitrogen. It will be well to give a summary of the actual experimental results relating to these points.

In the first place it is to be borne in mind, as Table I. (p. 5) shows, that whilst over the 27 years 1851–1877 inclusive, only about 17 lbs. of nitrogen were removed per acre per annum in the wheat grown in alternation with fallow, there was, over the 29 years, 1849–1877 inclusive, an average of about 32 lbs., or nearly twice as much, removed from the adjoining clover plots.

During the years 1878, 1879, and 1880, the yield of nitrogen in the wheat was about the same as the average of the preceding 27 years; whilst in most of the leguminous crops the yield was more than over the preceding 29 years. In the autumn of 1880 all the plots were ploughed up, and at the end of March, 1881, before resowing, soil samples were taken from five places on the leguminous crop-land, and also from five on the portion of the wheat land which was then fallow. The samples were, in each case, taken to the depth of 3 times 9 inches, or 27 inches in all.

It has already been stated that no nitrogenous manure had been applied to either the fallow-wheat, or the leguminous crop land, for more than 30 years; but that to the leguminous crop land different mineral manures had occasionally been applied.

They were as follows :—

Plot 2. Superphosphate of lime.

Plot 3. Sulphate of potash.

Plot 4. Sulphate of potash and superphosphate.

Plot 5. Salts of potash, soda, lime, and magnesia.

Plot 6. As 5, with superphosphate in addition.

Soil samples were taken from each of the 5 plots, and also from 5 places directly opposite to them on the immediately adjoining fallow-wheat land. The following table (V.) shows the percentage of nitrogen in the fine sifted surface soil, 9 inches in depth, as dried at 100° C., in the sample from each plot. It also shows the mean for the five plots, the mean of determinations made on mixtures from the five plots, and the general means of these two sets of determinations. It should be added that each figure given is the mean of three or more determinations made by the soda-lime method.

TABLE V.—Nitrogen per cent. in soil samples collected March, 1881.

Nitrogen per cent. in fine surface soil, 9 inches deep. Calculated on the soil as dried at 100° C.		
	Leguminous crop soils.	Fallow-wheat soil.
	Per cent.	Per cent.
Plot 2	0·1064	0·0938
„ 3	0·1036	0·0930
„ 4	0·0950	0·0931
„ 5	0·1100	0·0957
„ 6	0·1156	0·1007
Means	0·1061	0·0953
On mixture	0·1055	0·0984
General means	0·1058	0·0969

There can be no doubt that both the leguminous crop and the fallow-wheat surface soils had lost nitrogen during the preceding 30 years or more ; but whilst in 1881 the surface soil of the plots which had grown many leguminous crops showed an average of 0·1058 per cent. of nitrogen, that of the plot which had grown wheat in alternation with fallow, and yielded over so many years only about half as much in crops, contained only 0·0969 per cent. It may be thought that the difference is not great ; but a glance at the details which give these means can leave no doubt that it is real ; nor can there be any doubt that it is characteristic also. The further results will afford confirmation of this.

The next point to consider is, whether the continued growth of the various leguminous crops has reduced or increased the stock of nitrogen in the surface soils. The foregoing Table shows that in 1881 samples were taken from each of the five differently mineral-manured plots, but in 1882, 1883, and 1885, when samples were taken to considerable depths for the determination of the nitric acid, either plot 4, plot 5, or plot 6 was always selected, as on them the growth was better than on either plot 2 or plot 3.

The following Table (VI.) summarises the percentages of nitrogen in the surface soils (9 inches deep) of the fallow-wheat land, of the *Trifolium repens* plots, of the *Vicia sativa* plots, of the *Melilotus leucantha* plots, and of the *Medicago sativa* plots, in the years as indicated. The figures are as before in all cases the means of 2, 3, or more determinations on each sample. In each case the results given in the first line are the means of determinations made on the individual samples taken from different places on the plot, those in the second line are the means of the determinations made on mixtures of the individual samples, and the general means given in the third line are the means of the results on the individual and on the mixed samples, taken together. It may be further explained that the wheat-fallow samples were taken in 1883 from 4, and in 1885 from 3 places on the plot. The *Trifolium repens* samples were taken in 1882 from 2 places on plot 6, in 1883 from 2 places on plot 4, and in 1885 from 2 places on plot 5. The *Vicia sativa* soils were taken in 1883 from 2 places on plot 4, and from 2 on plot 6. The *Melilotus leucantha* soils were taken in 1882 from 2 places on plot 5, and from 2 on plot 6, and in 1885 from 2 on plot 5. Lastly, the *Medicago sativa* soils were taken in 1885 from 2 places on plot 5. The determinations were, as before, made by the soda-lime method.

TABLE VI.—Percentages of Nitrogen in the fine sifted surface soils (9 inches deep), reckoned as dry at 100° C., at different periods. Samples collected July 26–31, 1882; July 17–26, 1883; July 29–August 14, 1885.

	1882.	1883.	1885.	Mean.
Wheat-fallow land.				
	Per cent.	Per cent.	Per cent.	Per cent.
Means on individual samples	..	0·1044	0·1006	0·1025
Means on mixtures of individual samples . .	..	0·1026	0·1035	0·1031
General means	..	0·1035	0·1021	0·1028
Trifolium repens land.				
Means on individual samples	0·1149	0·1131	0·1269	0·1183
Means on mixtures of individual samples . .	0·1131	0·1125	0·1268	0·1175
General means	0·1140	0·1128	0·1269	0·1179
Vicia sativa land.				
Mean on individual samples	..	0·1203	..	0·1203
Mean on mixture of individual samples . . .	..	0·1178	..	0·1178
General mean.	..	0·1191	..	0·1191
Melilotus leucantha land.				
Means on individual samples	0·1095	..	0·1122	0·1109
Means on mixtures of individual samples . .	0·1123	..	0·1179	0·1151
General means	0·1109	..	0·1151	0·1130
Medicago sativa land.				
Mean on individual samples	..	..	0·1214	0·1214
Mean on mixture of individual samples . . .	..	..	0·1224	0·1224
General mean.	..	..	0·1219	0·1219

It is seen that even the fallow-wheat soils show slightly higher percentages of nitrogen in the autumns of 1883 and 1885 than in March, 1881; but the later samples were all taken from the end of the field, the samples from which showed somewhat higher percentages than the others even in 1881; and, as the figures show, the increase was in most cases much greater on the various leguminous plots. Thus, comparing the final means in the foregoing Table with those in Table V. (p. 14), the percentage in the wheat-fallow soil is 0·1028 against 0·0969 in 1881; in the *Trifolium repens* soil it is 0·1179, in the *Vicia sativa* soil 0·1191, in the *Melilotus leucantha* soil 0·1130, and in the *Medicago sativa* soil 0·1219, against a general mean of 0·1058 in 1881.

It should be stated that before taking the samples all above-ground growth is carefully cut off by scissors and removed; and that in the preparation of the soil-samples for analysis, all roots, indeed all visible vegetable débris, is carefully picked out; so that the results only include the nitrogen of that part of the crop-residue which has become thoroughly disintegrated, and may be considered as a proper constituent of the surface mould. It may be further stated that the separated residue from the leguminous crop soils contained more nitrogen than that from the wheat-fallow soil.

Going a little more into detail, it is seen that the *Trifolium repens* soil shows a mean of 0·1140 per cent. nitrogen in 1882, of 0·1128 per cent. in 1883, and of 0·1269 per cent. in 1885. That is to say, the lowest percentage is in 1883 when there had been no growth, when there had been a whole season for the disintegration and nitrification of the residue of the previous year, and when 146 lbs. of nitrogen as nitric acid were found to the depth of 108 inches, and more than four-fifths of it below the surface soil. On the other hand, the percentage is the highest in 1885, when nearly 100 lbs. of nitrogen had recently been removed in the crop, and the crop-residue would be comparatively large.

In the *Melilotus leucantha* soil somewhat more nitrogen was found in 1885, which was the eighth year of continuous crop, than in 1882, which was only the fifth season, but which yielded more than twice the amount of nitrogen in the crop, and left considerably more visible and separated residue in the surface soil.

Lastly, of the *Medicago sativa* soils, we have samples only in the sixth year of the growth, which had rapidly increased to an enormous amount in the fifth year, 1884, and yielded very large, though somewhat less, produce in 1885. Under these circumstances the mean percentage of nitrogen in the surface soil is 0·1219, or higher than in the case of any other plant or year, excepting in the *Trifolium repens* soil of the same year. This is the case notwithstanding that more visible crop-residue had been separated from the *Medicago sativa* soil samples than from any of the others; and in fact about three times as much as from the *Trifolium repens* soils of the same year. Indeed, it was estimated that the separated residue from the *Medicago sativa* soil-samples, the nitrogen in which was determined, represented a removal of about 100 lbs. of nitrogen per acre.

Without relying on the exact figures as representing exact gains or losses of nitrogen by the surface soils, we think it will be granted that the results are too consistent to leave any doubt that by the growth of the leguminous crops the surface soils had gained nitrogen, and that this gain bore some relation to the amount of growth and removal in the crops.

If then the surface soils have gained in nitrogen, it is obvious that they have not been the primary source of the nitrogen taken up by the plants. It must have come either from above or from below the surface soil—from the atmosphere or from the subsoil. The evidence does indeed point to the fact, that much nitric acid results from nitrification of nitrogen accumulated within the surface soil. But as this nitrogen either increases or does not diminish, much of the nitric acid produced must come from some other source than what may be called the original stock of nitrogen of the surface soil itself. Much doubtless comes from the nitrogen of crop-residue, itself derived from the atmosphere or from the subsoil.

Before discussing this subject further it will be well to call attention to another remarkable experiment, showing the amount of nitrogen that may be taken up by one leguminous plant growing on land where another had to a great extent failed.

4. *Experiments on the growth of Red Clover on Bean-exhausted Land.*

The results in question were obtained in a field in which beans had been grown almost continuously for 32 years, but had considerably declined in yield, as the following Table will show :—

TABLE VII.—Quantities of Nitrogen removed per acre per annum, in lbs., in bean-crops, over four periods of 8 years each. Geescroft Field, Rothamsted.

	8 years, 1847–1854.	8 years, 1855–1862.	8 years, 1863–1870.	8 years, 1871–1878.	Average 32 years, 1847–1878.
	lbs.	lbs.	lbs.	lbs.	lbs.
1. Without manure	48·41	25·26	9·12	16·36	24·79
2. With mineral manure containing potash	60·19*	34·25	23·46	26·66	35·36†
3. With mineral manure and nitrogen . .	68·94	36·87	35·05	28·69	42·39

The two upper lines show the amounts of nitrogen removed per acre per annum in the bean crops without any supply of nitrogen by manure. It will be seen that over each period of 8 years, the plot receiving a mineral manure containing potash yielded considerably more nitrogen than the one without any manure. In both cases, however, there was considerable decline from the first period to the last. Further, whilst over the 32 years the unmanured plot yielded an average of 24·8 lbs. of nitrogen per acre per annum, that with the potash manure yielded 35·4 lbs., or nearly one-half more, though without any supply of nitrogen from without. In the third experiment, where besides the potash manure some nitrogen was applied, in the early years as ammonium

* Average of 7 years only, results not available for 1849.

† Average 31 years only.

salts and in the later as sodium nitrate, there is some, but comparatively little increase in the amount of nitrogen in the crop.

In connection with the fact of the gradual decline in yield, it should be explained that owing to failure of the beans there was—in the second period one year of fallow, and one year of wheat; in the third period, one year of fallow; whilst in the fourth period, the first crop failed, and the land was left fallow during the second, third, and sixth seasons. The yields of nitrogen are, however, in each case, averaged for the period of 8 years.

After the 32nd year, 1878, the land was left fallow for between four and five years. Under these circumstances, as will be seen presently, the stock of total nitrogen in the surface soil had become very low, direct determinations of the nitrogen as nitric acid showed that the already existing amount of nitric-nitrogen down to the depth of 72 inches, was extremely small, whilst after several years of fallow there would be a minimum amount of crop-residue remaining for nitrification.

On this land, exhausted for one leguminous crop, barley and clover were sown in the spring of 1883. The clover grew very luxuriantly from the first, much interfering with the growth of the barley.

In our paper in the 'Transactions of the Chemical Society,' for June, 1885, we gave the amount of nitrogen as nitric acid found to the depth of 72 inches on the plot without manure, in that with the mineral manure alone, in that with the mineral and nitrogenous manure, and in that with farm-yard manure. We further estimated that the barley and clover crops would probably remove more than 200 lbs of nitrogen per acre. The amounts have, however, since been determined, and they are as follows:—

TABLE VIII.—Nitrogen removed per acre in the barley and clover crops.

Previous condition of manuring.	1883. Barley and clover.	1884. Clover.	1885. Clover.	Total.
	lbs.	lbs.	lbs.	lbs
Without manure	45·0	183·2	52·7	280·9
Mineral manure and some nitrogen . .	57·2	193·1	79·9	330·2
Mineral manure only	59·3	206·4	81·6	347·3

It should be stated that the plots, the yield of nitrogen of which is here given, do not exactly correspond with those as given in the preceding Table, some of the crops being taken together where no difference in the produce was observable. Thus half the plot represented as without manure has been unmanured from the commencement, that is, for nearly 40 years, the other half having received small quantities of nitrogen to 1878 inclusive, but has since been entirely unmanured. Again, the results given in the second line relate to the produce on the plot with the purely mineral manure containing potash, given in Table VII. as No. 2, together with that of the plot No. 3 to which some ammonium salts, or nitrate had, up to 1878, been applied, but which has received no manure since. The results given in the third line of the above Table (VIII.), relate, however, to a plot which has not received any nitrogenous manure from the commencement, but was not brought into experiment until five years later than the other plots.

Here then, in a field where beans had been grown for many years, had frequently yielded only small crops, and sometimes failed, and the land had then been left fallow for several years, where the surface soil had become very poor in total nitrogen, where both surface and subsoil were very poor in ready formed nitric acid, and where there was a minimum amount of crop-residue near the surface for decomposition and nitrification, there were grown very large crops of red clover containing very large amounts of nitrogen. On a plot where a purely mineral manure, containing potash, had been applied for 27 years up to 1878, but no manure whatever since, 347 lbs. of nitrogen were gathered, almost wholly by the clover. On a plot, on half of which the mineral manure only, and on the other half the same mineral manure with some ammonium salts or nitrate had been applied up to 1878, but nothing since, 330 lbs. of nitrogen were removed in the crops. Lastly, where on half the plot there had been no manure whatever for nearly 40 years, and on the other half ammonium salts or nitrate to 1878, but nothing since, 281 lbs. of nitrogen were yielded in the crops.

It may be said, therefore, that about 300 lbs. of nitrogen had been gathered by the clover growing on a soil upon which beans had yielded smaller and smaller crops, and, in fact, had eventually practically failed, and which was very poor both in total nitrogen near the surface, very poor in ready formed nitric acid to a considerable depth, and very poor in nitrogenous crop-residue for nitrification. If therefore the clover had taken up its nitrogen either wholly or mainly as nitric acid, the supply could not be due to recent crop-residue.

Not only was so much nitrogen removed in the crops, but the surface soil became determinably richer in nitrogen, as the following results will show. The plots are the same as those to which Table VII. refers; and the determinations are those made in samples of surface soil collected in April, 1883, before the sowing of the barley and clover, and in November, 1885, after the removal of the crops.

TABLE IX.—Nitrogen per cent., and per acre, in the surface soils, before and after the growth of the barley and clover. Geescroft Field, Rothamsted.

	Nitrogen in sifted dry soil.				
	Per cent.		Per acre.		
	1883.	1885.	1883.	1885.	1885 + or − 1883.
	p. c.	p. c.	lbs.	lbs.	lbs.
1. Without manure	0·0993	0·1083	2441	2662	+ 221
2. With mineral manure containing potash	0·1087	0·1149	2672	2824	+ 152
3. With mineral manure and nitrogen	0·1163	0·1225	2859	3011	+ 152

Without assuming that the figures represent accurately the amounts of nitrogen accumulated per acre, it cannot be doubted that the surface soils had become considerably richer. If, for the sake of illustration, we assume that 300 lbs. of nitrogen were removed per acre in the crops, and 200 lbs. were accumulated in the surface soil, we have 500 lbs. to account for as gathered by the crops, and chiefly by the clover, within about 2 years.

In our former paper, when we assumed that perhaps 200 lbs. would be removed in the crops, we admitted that there was in the experimental results no conclusive evidence as to the source of so large an amount of nitrogen, but that it must obviously have been derived either from the atmosphere or from the subsoil; and assuming it to be the subsoil rather than the atmosphere, the question arose whether it was taken up as nitric acid, as ammonia, or as organic nitrogen? It was pointed out that as yet no direct proof existed that green-leaved plants did take up the organic nitrogen of the soil as such; and that although there was more evidence from analogy in favour of a nitric acid source than of any other, proof was equally wanting to establish the conclusion that so much nitrogen had been available as nitric acid. The much larger amounts now known to have been gathered by the clover crop, of course renders this explanation still less adequate.

On a review of the whole of the results that have been adduced, it cannot be doubted that nitric acid is an important source of the nitrogen of the Leguminosæ. Indeed, so far as existing experimental evidence goes, that relating to nitric acid carries us quantitatively further than any other line of explanation. But it is obviously quite inadequate to account for the facts of growth, either in the case of the *Medicago sativa* grown on the clover-exhausted land, or in that of the clover on the bean-exhausted land. There is, in fact, nothing in the results relating to the clover experiment to justify the conclusion that there had been such a large production of nitric acid in the subsoil, due to the increased development of the nitrifying organisms under the influence of the leguminous growth and crop-residue, and their distribution, favoured by the action of the roots, and the increased activity in the interchange of moisture and air which must take place under such circumstances. Nor is it explicable how such large quantities of nitric acid could have been produced as would be required for the rapidly increasing growth of the *Medicago sativa*, and for the large amounts of nitrogen in it, if nitric acid had been the exclusive or even the main source of supply.

5. *Experiments on the Nitrification of the Nitrogen of Subsoils.*

In our paper in the 'Transactions of the Chemical Society' already referred to, we showed, in the case of some prairie land subsoils, that their nitrogen was susceptible of nitrification, and that when, after repeated extraction, the action became very feeble, or ceased, it was renewed on the soils being seeded by 0·1 gram of rich garden mould, which would contain nitrifying organisms. Considering, however, that from the circumstances of the collection and the transmission of those samples, the entire exclusion of comparatively recent organic residue from the upper layers was uncertain, it was decided to experiment in a similar way with some of the Rothamsted raw clay subsoils. Those selected were :—

1. A mixture of samples from the third to the twelfth depths of 9 inches each, that is representing the layer of 90 inches thick from 19 to 108 inches deep, from each of 3 out of the 4 holes from which samples were taken on the wheat-fallow land from July 17–26, 1883.

2. Similar mixtures from the third to the twelfth depths, from the samples taken from July 17–26, 1883, from the *Trifolium repens*, and from each of the two *Vicia sativa* plots, respectively.

3. From holes opened specially for subsoil samples only, one on the wheat-fallow land, on May 7, 1886, and one on April 16, 1886, in a field where red clover had been sown with barley in rotation in the previous spring, but from which no crop had yet been taken.

The first column of the next Table (X.) shows, for each of these samples, the percentage of total nitrogen calculated on the dry sifted soil, as determined by the soda-lime method; and the second column shows the quantity of already existing nitric-nitrogen per million of fine dry soil in each case. The other columns show the amounts of nitric-nitrogen per million dry soil, as determined by SCHLŒSING's method, in watery extracts made by the aid of the water pump, after successive periods of exposure under suitable conditions as to temperature and moisture. In the case of the wheat-fallow and leguminous crop subsoils, the results relating to which are given in the upper two divisions of the Table, they were each seeded by the addition of 0·1 gram of rich garden soil after the first period of exposure and subsequent extraction, and again by the addition of 0·2 gram after the third period and extraction. These experiments, with the exception of the determinations of the total nitrogen, were made in the Rothamsted Laboratory by Mr. D. A. LOUIS.

In the case of the wheat-fallow, and rotation clover land samples, to which the bottom division of the Table refers, there was a seeding with 0·2 gram of the garden soil after the extraction of the already existing nitric nitrogen, and before the first period of exposure; and there was again a similar seeding of the "seeded" lots, after the third period of exposure in the case of the wheat-fallow subsoil, and after the fourth period in that of the rotation clover land subsoil. There was also added, in the case of the wheat-fallow subsoil after the third, and in that of the clover subsoil after the fourth period, 0·8 gram of a mixture of 1 part potassium phosphate, 1 part magnesium sulphate, and 5 parts calcium carbonate; the mixture containing as impurity 0·000616 gram nitrogen per gram.

TABLE X.—Results showing the amount of the Nitrification of the nitrogen of subsoils.

	Nitrogen in fine dry soil.	Nitrogen as nitric acid per million fine dry soil.												Total.	
		Original.	Periods of exposure.												
			1st.		2nd.		3rd.		4th.		5th.†				
Wheat-fallow subsoils, collected July 17–26, 1883. Mixture of 3rd–12th depths.															
	Per cent.		Days.		Days.		Days.		Days.		Days.		Days.		
Hole 1	·0492	0·77	28	·106	28	·109	28	·237	28	·247	246	·084	358	·783	
„ 2	·0562	0·48	28	·071	28	·208	28	·043	28	·162	246	·225	358	·709	
„ 4	·0484	0·54	28	·127	28	·166	28	·066	28	·299	246	·224	358	·882	
Means	·0513	0·60	28	·101	28	·161	28	·115	28	·236	246	·178	358	·791	
Leguminous crop subsoils, collected July 17–26, 1883. Mixture of 3rd–12th depths.															
Trifolium repens. Plot 4	·0592	2·50	28	·132	28	·083	28	·156	28	·303	246	·338	358	1·012	
Vicia sativa. „ 4	·0508	1·63	28	·145	28	·015*	28	·185	28	·379	246	·375	358	1·099	
Vicia sativa. „ 6	·0360	1·40	28	·108	28	·079	28	·176	28	·276	246	·421	358	1·060	
Means	·0487	1·84	28	·128	28	·059	28	·172	28	·319	246	·378	358	1·056	
Wheat-fallow samples collected May 7, 1886, at a depth of 5 feet. Clover land samples collected April 16, 1886, at 7 feet 4 inches deep.															
Unseeded { Wheat-fallow	·0768	0·720	28	·053	35	·052	49	·107	129†	[·312]‡			241	·524	
Unseeded { Clover land	·0772	0·618	28	·077	35	·060	28	·134	30	·177	139	[·358]	260	·806	
Means . .	·0770	0·669	28	·065	35	·056	39	·121	79	·245					
Seeded { Wheat-fallow	·0768	0·714	28	·140	35	·081	49	·118	129†	[·358]			241	·697	
Seeded { Clover land	·0772	0·626	28	·262	35	·103	28	·437	30	·192	139	[·580]	260	1·574	
Means . .	·0770	0·670	28	·201	35	·092	39	·278	79	·275					

Referring to the results, it is in the first place to be observed that there is very considerable variation in the percentage of total nitrogen in the different subsoils.

* Probably too low.

† The number of days represents the periods of exposure, the periods of activity are uncertain.

‡ The figures given between brackets thus [ ] show the results obtained after the addition of the mineral mixture described in the text.

Indeed, so variable is the amount of nitrogen in samples of our Rothamsted subsoils taken on one and the same plot, dependent on the varying proportions of clay, sand, gravel, chalk, &c., that as we have fully illustrated in former papers, no estimates of the difference in the amounts of total nitrogen, either in the subsoil of the same plot at different periods, or in the subsoils of plots differently manured, or differently cropped, can be relied upon.

Again, as the amount of the "original" or already existing nitric-nitrogen is, as has been very fully shown, greatly dependent on the description, and on the amount of crop that has been grown, and other circumstances, it is not to be expected that it would bear any direct relation to the richness or poverty of the subsoil in total nitrogen.

Referring to the amounts of nitric-nitrogen formed in the different subsoils, and within the different periods of exposure, there is, as is to be expected in the case of an action depending on the development and activity of an organism of the habits, requirements, and mode of action of which we know but little, considerable irregularity, both from period to period with the same sample, and within each period with the different samples.

Confining attention in the first place to the results relating to the wheat-fallow, and the leguminous crop subsoils, recorded in the upper two divisions of the Table, it will be seen that the first, second, third, and fourth periods of exposure each comprised 28 days; whilst the fifth period extended over 246 days, or about 35 weeks, during a considerable portion of which, however, the soils had doubtless become too dry for activity and nitrification. It is probable that a period of 28 days is too short to insure the active development of the organisms, and consequent energetic nitrification. Then again, the extraction of the soils by water under pressure must, it is to be supposed, remove some of the organisms, instead of allowing of their natural multiplication. Comparison of the results from period to period must, therefore, be made with some reservation.

But apart from any irregularities in the case of individual samples, or at individual periods, if we compare for each period the mean results for the three wheat-fallow samples, with the means for the leguminous crop subsoils, it is seen that during four of the five periods, the leguminous crop subsoils show considerably more nitrification than the wheat-fallow ones; and whilst over the total period the wheat-fallow subsoils show an average of 0·791 nitrogen nitrified per million of soil, the leguminous crop subsoils show 1·056 per million.

Again, the figures in the bottom division of the Table, relating to the wheat-fallow and the rotation clover land samples, collected in the spring of 1886, show in all cases comparable as to length of period, more nitrification in the clover land than in the wheat-fallow land subsoil; and this is so both with the unseeded and the seeded samples.

Thus, then, these results with the raw, and mostly clay, Rothamsted subsoils, containing not more than 6 or 8 parts carbon to one of nitrogen, confirm those previously obtained with the prairie subsoils containing much higher proportion of carbon, in showing that their nitrogen is susceptible to nitrification, provided the organisms, and other essential conditions, are not wanting. These new results also consistently show that there is more active nitrification in the leguminous than in the gramineous crop subsoils. This it must be supposed, is partly due to more active development, and greater distribution, of the organisms themselves, under the influence of the leguminous growth, with its excretions and residue, and partly to the greater actual amount of such easily changeable matters.

The results are also confirmed by those of experiments made in the Rothamsted Laboratory by Mr. WARINGTON, for the most part on quite distinct lines. Thus, in most cases, instead of determining the amount of nitrification taking place in the different subsoils when exposed under suitable conditions, he introduced a portion of the subsoil into a sterilised nitrogenous liquid, and determined whether nitrification took place; the result being taken to show whether or not the organisms were present in the subsoil. In the first experiments, the samples were taken with precautions to avoid any contamination by roots or other organic matter, and the conditions of the sterilised liquids were such as the experience of the time indicated as favourable for nitrification. Upon these results he says ('Chem. Soc. Trans.,' 1884, p. 645): "I am disposed to conclude that in our clay soils the nitrifying organism is not uniformly distributed much below 9 inches from the surface. On much slighter grounds, it may perhaps, be assumed, that the organism is sparsely distributed down to 18 inches, or possibly somewhat further. At depths from 2 feet to 8 feet, there is no trustworthy evidence to show that the clay contains the nitrifying organism. It is however probable that the organism may occur in the natural channels which penetrate the subsoil at a greater depth than in the solid clay."

Subsequently ('Chem. Soc. Trans.,' 1887, p. 118) he experimented with a greater variety of subsoils, taking samples from the wheat-fallow, the *Trifolium repens*, the *Melilotus leucantha*, and the *Medicago sativa* subsoils, when these were exposed for the collection of the samples for the various experiments, our own results relating to which we have given in some detail. Further, some of the samples were now taken in the immediate neighbourhood of lucerne roots, and gypsum was added to the sterilised liquids.

Among the 69 trials made in this new series of experiments, there was no failure to produce nitrification by samples down to 2 feet; there was only one failure out of 11 trials down to 3 feet; but below 3 feet, the failures were more numerous. Taken at 6 feet about half the samples induced nitrification. The order of priority of nitrification diminished from the upper to the lower depths; indicating more sparse occurrence, and more feeble power of development and action.

Examination of the results shows, however, that quite consistently with those which we have described, there was notably more active nitrification with the leguminous than with the gramineous crop subsoils. Thus, compared with the results yielded by the wheat-fallow subsoils, those by the white clover subsoils were more marked; but this was especially the case with the lucerne plot subsoils, of which more samples, and those from a greater depth, induced nitrification. The same is also observable on a comparison of the results obtained by the samples from the wheat-fallow plot, with those from the rotation red clover plot.

It is then established that the nitrogenous matters of raw clay subsoils are susceptible of nitrification, if the organisms, with the other necessary conditions, are present. It is further indicated, not only that the action is more marked under the influence of leguminous than of gramineous growth and crop-residue, but that the organisms become distributed to a considerable depth even in raw clay subsoils, especially where deep-rooted and free-growing Leguminosæ have grown.

The next question is, how far, in a quantitative sense, do the results aid us in explaining the source of the large amounts of nitrogen taken up by some leguminous crops—as for instance in the case of the *Medicago sativa* grown on the clover-exhausted land, and of the red clover grown on the bean-exhausted land.

In the case of the three leguminous crop subsoils there was, over the total period, only about 1 part of nitrogen nitrified per million of soil; and as the subsoil to the depth experimented on would weigh about 30 million lbs. per acre, the amount of nitrification supposed would represent only about 30 lbs. per acre. Obviously, the conditions of nitrification in which the samples are exposed in the laboratory are very different from those of the subsoil *in situ.* Thus, whilst in the case of the samples in the laboratory, the conditions as to temperature and aëration would be the more favourable, the successive extractions by water under pressure would be liable to remove, not only the mineral matters essential for the development of the organisms, and for the production of nitric acid, but the organisms themselves, whereas in the case of the natural subsoil the tendency would be to multiplication.

Compared with the small amount of nitrification of the nitrogen of the raw clay subsoils shown in the foregoing experiments, in which some of the conditions were more and others less favourable than in the natural subsoil, the following results obtained by Mr. WARINGTON ('Chem. Soc. Trans.,' 1887, pp. 127–9), show how very large may be the amount of nitrification of the nitrogen of such subsoils under more favourable circumstances than those of their natural condition. Thus, he mixed raw clay subsoil with an equal weight of coarsely powdered flint, seeded the mixture with rich garden soil, moistened it, and placed it in a vessel allowing for free access of washed air. Under these conditions he found, when no mineral food was added, in one case 2·4, and in another 3·0 per cent. of the total nitrogen of the subsoil nitrified; and when mineral food was added, he found in one case 4 per cent., and in another 3·6 per cent. of the total nitrogen was nitrified.

Indeed, the greatest difficulty in the way of the supposition that much nitrogen is available to plants by the nitrification of the nitrogen of the subsoil, is, in fact, the want of sufficient aëration. Independently of the greater or less porosity of the subsoil itself, and of the channels formed by worms, it is obvious that wherever the roots go, water and its contents can follow; and that, with deep-rooted plants and free growth, there will be active movement of water, and there must be of air also, in the lower layers of the soil. In our former paper we called attention to the fact that in the experiments in 1882, with the greater growth of the *Melilotus*, there remained in the soil less water than in that of the *Trifolium repens* soil, corresponding down to a depth of 54 inches, to a loss of 540 tons per acre, or nearly 5½ inches of rain; and again, in 1883, the *Vicia sativa* soil showed down to 108 inches, less water than the *Trifolium repens* soil, in amount corresponding to between 600 and 700 tons per acre, or to between 6 and 7 inches of rain. Obviously too, the still deeper rooting, and still freer growing *Medicago sativa* would remove still more water.

Although much experiment and much calculation have been devoted by several investigators to the estimation of the degree of aëration of soils and subsoils of different character, the data at command do not justify any very definite conclusions on the subject. The results seem to indicate a probable range of aëration from about 30 to over 50 per cent. of the volume of the soil. But these estimates do not take into account the varying amounts of water in the soil or subsoil. In the case of the subsoils referred to in this paper, each layer of 9 inches in depth retained from about 2 to nearly 4 inches of water, the amount varying very much according to the nature of the subsoil, and especially according to the amount of growth, and the consequent withdrawal of water from below, and its evaporation, chiefly through the plant, but partly also from the surface soil. The amount must obviously also vary very much according to the character of the season.

It may here be observed that supposing the subsoil contained at one time, air equal to one-third of its volume, this would not suffice for the nitrification of as much nitrogen as was taken up for several years in succession by the *Medicago sativa*, or during two years in the case of red clover on the bean-exhausted land. But the nitrogen is not taken up all at once, though most of it will be within a few months of the year, during which period there would be the most active withdrawal of water from below, and evaporation by the plant and surface soil. The replacement of this subsoil water by an equal volume of air would, however, still not suffice. The question obviously arises, how far, or how rapidly, the used up oxygen will be replaced, and on this point there is very little experimental evidence to aid us. We shall refer to the subject again further on.

6. *Can Roots, by virtue of their Acid Sap, attack, and render available, the otherwise insoluble Nitrogen of the Subsoil?*

Thus, then, although the evidence is clear that the nitrogen of raw clay subsoils, which constitutes an enormous store of already combined nitrogen, is susceptible of nitrification, provided the organisms are present, and the supply of oxygen is sufficient, yet, the data at command do not indicate that these conditions could be adequately available in such cases as those of the very large accumulations of nitrogen by the red clover on the bean-exhausted land, and of the increasing and very large accumulations by the *Medicago sativa* for a number of years in succession.

Accordingly, on September 3, 1885, when the holes were open for the soil sampling on the *Medicago sativa* plot, a specimen of the deep, strong, fleshy root of the plant was taken, and on examination it was found that the root-sap was very strongly acid. The roots of three plants were then collected. Of these, No. 1 had four branches, which were respectively—6 feet $4\frac{1}{2}$ inches, 5 feet $10\frac{1}{2}$ inches, 3 feet $6\frac{1}{2}$ inches, and 2 feet $9\frac{1}{2}$ inches in length; No. 2 had two branches—4 feet 10 inches and 2 feet 2 inches in length; and No. 3 had two branches, respectively 3 feet 9 inches and 1 foot 9 inches in length.

The roots were rapidly washed in distilled water, dabbed with clean cloths, weighed, rapidly cut into small pieces, and bruised into a pulp in Wedgwood mortars, with a measured quantity of ammonia-free distilled water. The pulp was then put upon a vacuum filter, and the resulting extract was made up to a given volume with pure distilled water. It was, however, found impossible to get the extract perfectly clear within the limited time it could be exposed to treatment without risk of change, and hence, in these initiative experiments, it was dealt with whilst still somewhat turbid.

The dry matter, ash, and nitrogen were determined in the original root, in the root extract, and in the exhausted root.

The important question was whether the acid root juice would take up nitrogen from a raw clay subsoil such as that from which the *Melilotus leucantha*, the *Medicago sativa*, and the red clover, were supposed to have derived such large quantities in some way. Accordingly, 20 grams of subsoil from the unmanured wheat-fallow plot immediately adjoining the *Melilotus* and *Medicago sativa* plots, were added to a known quantity of the acid root extract in a stoppered bottle, well shaken, and the soil and liquid were left in contact for some weeks, the autumn holidays intervening. It was found, however, on examination, that the extract had lost, and the soil had gained nitrogen, nitrogenous organic matter having been deposited.

In November, 1885, one of the *Medicago sativa* holes was reopened, and fresh quantities of root were collected. These were prepared in substantially the same way as before, but with much greater expedition, more persons being employed. The roots were rapidly passed successively through 4 basins of distilled water, which was renewed as needed.* They were then dabbed with clean dry cloths as before, weighed, cut first into short lengths, and then into very small pieces, the whole being kept in basins covered with glass plates as much as possible to lessen evaporation. The weight was then again taken, a known quantity of pure distilled water added, the whole bruised in Wedgwood mortars, and re-weighed.

* In subsequent experiments it has been found that the wash-water did not become acid during the process.

One weighed portion of the pulp was put on the vacuum filter as before, and another was submitted to dialysis, the diluted pulp being put into a parchment paper sausage dialyser which was then placed in pure distilled water. However, neither the vacuum filtrate nor the dialysate was quite clear. Both these extracts showed a less degree of acidity, and it was evident that the root was now in a very inactive state compared with that of the specimens collected early in September. Various qualitative examinations were made, and it was found that the extracts contained a large amount of nitric acid. It was decided, however, that further detailed investigation of the subject must be postponed until the return of the actively growing period.

It was intended to undertake the subject in the spring and summer of 1886, but owing to the pressure of other work nothing more was accomplished than the comparative testing of the acidity of the sap of the roots of a number of plants representing very various natural families. The matter was, however, again taken up in April, 1887; and, benefiting by previous experience, some advance was made, but still the attempts to entirely free the acid extract from nitrogen were not successful.

It is of interest to observe that the degree of acidity of the sap of the roots collected in April, May, and June, that is during the periods of the most active growth of the season, was considerably higher than in that of the roots collected in September, 1885, after the cutting of the first crop.

The investigation is, however, at present little more than commenced, and any further reference to the results must be postponed to some future occasion.

7. *Action of Dilute Organic Acid Solutions on the Nitrogen of Soils and Subsoils.*

In the autumn of 1885, when it was found necessary to postpone further experiment with the acid root-sap, it was decided in the meantime to examine the action on soils and subsoils, of various organic acids, in solutions of a degree of acidity either approximately the same as that of the *Medicago sativa* root juice, or having a known relation to it. The acids experimented upon were the malic, citric, tartaric, oxalic, acetic, and formic.

In the first experiments, pure water, and dilute solutions of malic, citric, tartaric, oxalic, and acetic acids, were used; each of the acid solutions being of approximately the same degree of acidity as the sap of the *Medicago sativa* roots collected in September, 1885, that is after the removal of the first crop, which represented the greater part of the growth of the season. The subsoil employed was a mixture of the sifted soil of the fifth, sixth, seventh, eighth, ninth, and tenth depths of 9 inches each, from the unmanured wheat-fallow land immediately adjoining the leguminous plots, and contained, as dried at 100° C., 0·047 per cent. of nitrogen.

The mixtures were made in wide-mouthed stoppered bottles, in the proportion of 200 grams of the subsoil to 1000 c.c. of the water or acid solution. After being well shaken, in the case of the water, the malic acid, and the acetic acid extracts, a given quantity was drawn off at the expiration of one hour. The remainders of these, and the mixtures of the other acid solutions, were frequently shaken, finally left to settle, and after contact for between two and three days the extracts were decanted off, and the soils drained on a vacuum filter. The several extracts being filtered, portions were at once evaporated to dryness on a steam bath, and the nitrogen in them was determined by the soda-lime method. The actual quantity of nitrogen involved in each determination was, however, so small, that recourse was afterwards had to KJELDAHL'S method; and comparative results led to the conclusion that those obtained by it were the more trustworthy. Accordingly, only the general indications obtained by the soda-lime method are here given.

In the case of the water, and of the malic and acetic acid solutions, nitrogen was taken up after 1 hour's contact with the raw clay subsoil; the most being taken up by the malic acid. In each case, after contact for between two and three days, the amounts of nitrogen in the extract were less than after only 1 hour's contact. There had thus obviously been re-precipitation of nitrogenous matter at first taken up, and as the extracts showed scarcely any remaining acidity, the explanation seemed to be that the longer the contact, the more was the acid neutralised by the fixed bases of the subsoil. Of the five organic acid solutions left in contact between two and three days, the malic retained the most nitrogen; next came the acetic and tartaric, then the citric, and lastly the oxalic.

In a second series of experiments, besides the same five acids as before, formic acid was included. The acid solutions were however now twice as strong as those used in the first series. In the case of the malic acid, the periods of contact were 1 hour and 48 hours, and in that of each of the others 1 hour and 24 hours. In each case the acidity of the solution was much reduced by contact with the subsoil, and in each the more the longer the contact. Again the malic acid took up the largest amount of nitrogen; and with the malic, and the formic acids, less was found in the extracts after the longer periods of contact. With the oxalic acid, however, in a striking degree, and less in that of the tartaric, the amount of nitrogen taken up was greater after 24 hours' than after 1 hour's contact, probably owing to the precipitation of the lime in these cases.

It was next decided, with the view of getting larger amounts of nitrogen taken up, to make 3 series of experiments as follows:—

1. With double the quantity of subsoil to a given volume of the acid solution.
2. With double the quantity of acid solution to a given quantity of the subsoil.
3. To add a second quantity of the acid solution to the already once extracted subsoil.

Further, it was decided to experiment with malic acid only; and for comparison with the results on the subsoil, to make parallel experiments with the surface soil of the *Medicago sativa* plot. Lastly, duplicate determinations of the nitrogen in the extracts were made, one by the soda-lime method, and the other by KJELDAHL'S sulphuric acid method.

In the following Table the results obtained in these experiments by KJELDAHL'S method are given.

TABLE XI.—Showing the amount of the Nitrogen of surface soil and subsoil dissolved by malic acid solution of approximately twice the acidity of the sap of the *Medicago sativa* roots collected in September, 1885.

		Nitrogen dissolved per million soil.	
		After 1 hour's contact.	After 24 hours' contact.
400 grams soil, 1000 c.c. acid solution, each extraction.			
		Per million.	Per million.
Wheat-fallow subsoil	First extraction	2·43	1·82
	Second extraction	2·19	2·19
Lucerne surface soil	First extraction	9·72	8·51
	Second extraction	6·08	7·59
200 grams soil, 1000 c.c. acid solution, each extraction.			
Wheat-fallow subsoil	First extraction	3·28	7·29
	Second extraction	4·03	3·61
Lucerne surface soil	First extraction	8·14	10·81
	Second extraction	4·35	7·31

First as to the experiments the results of which are given in the upper division of the table, in which 400 grams of soil were mixed with 1000 c.c. of acid solution, in each extraction, that is to say, after the removal of the first extract by decantation and the filter pump, a second quantity of the acid solution was added. After 1 hour's contact with the subsoil, the liquid remained only slightly acid, and the amount of nitrogen taken up was very small, representing only 2·43 parts per million of subsoil. After 24 hours' contact the liquid was still less acid, and the amount of nitrogen found in the extract was, calculated per million of subsoil, considerably less than after only 1 hour's contact.

After the addition of a second quantity of the acid solution to the already once extracted subsoil, the liquid remained much more acid than in the case of the first extraction, both after 1 hour's and 24 hours' contact. Still, the amount of nitrogen taken up was very small; being only 2·19 per million soil after 1 hour's, and the same after 24 hours' contact. That is to say, with the greater remaining acidity in the second extraction, there was not less nitrogen taken up after 24 than after 1 hour's contact. It may be observed that, under these conditions, much more total matter, remained dissolved in the second than in the first extract.

Turning now to the parallel results obtained with the lucerne surface soil, which, though poor, still contained about twice and a half as much nitrogen as the subsoil, it is seen that much more nitrogen was found in the extracts than in those from the subsoil. At the same time the liquids after contact showed scarcely a trace of acidity, and they were found to contain much more of other dissolved matters. In the first extraction, after 1 hour 9·72, and after 24 hours 8·51 parts of nitrogen were taken up per million of soil; and in the second extraction 6·08 and 7·59 parts per million. That there was less nitrogen taken up by the second quantity of acid than by the first, is doubtless due to the more readily soluble portion having been already removed. Even in the second extraction of this richer, though still poor, surface soil, about three times as much nitrogen was taken up as from the subsoil.

In the experiments so far considered, nearly the whole of the acid was neutralised in the first extraction of the subsoil, and in both extractions of the surface soil. In the experiments, the results of which are recorded in the lower division of the table, only half the quantity of subsoil or surface soil was mixed with 1000 c.c. of the acid solution; and here, in the case of the subsoil the liquids remained distinctly acid in the first extraction, even after 24 hours' contact, and more strongly acid in the second extraction. In the case of the surface soil, however, in the first extraction the acidity was entirely neutralised, and even in the second extraction nearly so.

The figures show that considerably more nitrogen was taken up, even from the subsoil, when twice the quantity of acid solution was used to a given quantity of it, and when, accordingly, the extracts remained more or less strongly acid. In the first extraction the quantities of nitrogen found in solution were, after 1 hour 3·28, and after 24 hours 7·29, per million soil; that is the more the longer the contact when the liquid remained distinctly acid. In the second extraction, with still greater remaining acidity, the amounts were 4·03 after 1 hour, and only 3·61 after 24 hours. That notwithstanding there was much more remaining acidity, there should be less taken up after 24 hours in the second than in the first extraction, again indicates that a certain quantity of the nitrogen exists in a more readily attackable condition than the remainder. It may be added, that much more mineral matter as well as nitrogen was taken up with the larger proportion of acid solution to a given weight of the subsoil.

With the larger quantity of acid solution to a given weight of the surface soil, much more nitrogen was taken up than under parallel conditions with the subsoil. But, in the first extraction there was little more, and in the second even less, than with twice the quantity of the surface soil to a given quantity of the acid solution. In fact, there was, taking the two extractions together, even less nitrogen taken up with the larger than with the smaller proportion of acid solution to a given weight of soil; but with the larger proportion there was much more mineral matter taken up, whereby the acid would be to a greater degree neutralised. There is, both after 1 hour and after 24 hours, much less nitrogen taken up in the second than in the first extraction ; again showing that a certain proportion of the nitrogen of the soil is more easily attacked than the remainder.

All the foregoing results illustrating the action of dilute organic acid solutions on the organic nitrogen of soils and subsoils were obtained in 1885, and 1886, by Mr. D. A. Louis, and the strengths adopted had reference to the degree of acidity of the sap of the lucerne roots collected in September, 1885, after the main growth of the season was past. But finding the sap so much more strongly acid in April, 1887, that is at the commencement of the active growth of the season, it was decided to experiment with much stronger malic acid solutions.

The following Table gives the results of experiments made by Dr. N. H. J. Miller ; in which the malic acid solution was of approximately 10 times the acidity of the September, 1885, root-sap, and the mixtures were made in the proportion of 200 grams of the wheat-fallow subsoil to 1000 c.c. of the acid liquid. As before, a portion of the extract was removed after 1 hour's contact, and the remainder after 24 hours'. A second quantity of the acid solution was then added to the already once extracted subsoil, and portions were examined as usual after 1 hour's and after 24 hours' contact.

Table XII.—Showing the amount of the Nitrogen of subsoil dissolved by a malic acid solution of a degree of acidity much greater than that of lucerne root-sap.

	Nitrogen dissolved per million soil.	
	After 1 hour's contact.	After 24 hours' contact.
Wheat-fallow subsoil { First extraction . . .	8·16	13·75
Wheat-fallow subsoil { Second extraction . . .	11·71	7·03

Even in the first extraction more than half the acid remained unneutralised, and a larger proportion still in the second extraction. Under these conditions of constant excess of acid, the raw subsoil gives up considerably more nitrogen, though there was, at the same time, much more mineral matter taken up. In the first extraction the amounts of nitrogen taken up per million subsoil were 8·16 parts after 1 hour, and 13·75 parts after 24 hours ; that is more after the longer contact. In the second extraction, however, less remained in solution after 24 hours' than after 1 hour's contact, from which it would appear that nitrogen once taken up had been deposited.

Obviously the conditions of experiments in which an acid solution is agitated with a quantity of soil are not comparable with those of the action of living roots on the soil. The root action would necessarily affect only a very small proportion of the total soil. But the results recorded clearly show that the greater the acidity of the solution, the more nitrogen is taken up, and the question arises, whether the root action would not effect more resolution on the surfaces actually attacked? Indeed, this must necessarily be the case if such an action is really quantitatively an important source of the nitrogen taken up by deep and strong rooting plants, with strongly acid sap. In illustration of this necessity it may be stated that, even if as much as 20 parts of nitrogen were taken up per million of soil, as was the case in the last-mentioned experiments in the first and second extractions taken together, this would only represent 600 lbs. of nitrogen per acre to the depth examined, namely 108 inches.

Upon the whole, then, the experiments on the action of weak organic acid solutions on raw clay subsoil, or even on a poor surface soil, have not given results from which any very definite conclusions can be drawn, as to the probability that the action of roots on the soil, by virtue of their acid sap, is quantitatively an important source of the nitrogen of plants having an extended development of roots, of which the sap is strongly acid.

That roots do attack certain mineral substances by virtue of their acid sap, was established by SACHS. He sowed seeds in a layer of sand on polished marble, dolomite, and osteolite, and he found that the polished surfaces were, so to speak, corroded, where in contact with the roots. In regard to these results, SACHS says: ('Text-Book of Botany,' 2nd English edition, p. 702) "every root has dissolved at the points of contact a small portion of the mineral by means of the acid water which permeates its outer cell walls." It was to carbonic acid that SACHS attributed the action in these cases; but there seems no reason to suppose that other acids in the root-sap may not exert a similar action. The results which have hitherto been published have however reference only to the taking up of mineral substances from the soil by virtue of such an action; and so far as we are aware the possibility or probability that the nitrogen of the soil or subsoil is so taken up has not been considered.

Provided it were clearly established that the organic nitrogen of the soil, and especially of the subsoil, was rendered soluble by the action of the acid sap of the root, the question would still remain, whether the nitrogenous body is merely dissolved, and taken up by the plant as such, as the evidence at command seems to show is probable in the case of the fungi, or whether the nitrogenous body, after being attacked by the acid, is subjected to further change before entering the plant? To this point we shall recur presently.

Since the experiments at Rothamsted, above referred to, on the character and the action of the root-sap were undertaken, a preliminary notice of experiments on the nitrogenous organic compounds of the soil has been published by Dr. G. LOGES ('Versuchs-Stationen,' vol. 32, p. 201). He found that the hydrochloric acid extracts of soils rich in humus left on evaporation a residue containing a large proportion of the nitrogen of the original soil. In his notice he does not state the strength of the acid used; but from the results it is to be concluded that it was somewhat concentrated—indeed of a strength not at all comparable with that of root-sap. Then, the soils, the results relating to which he gives, were extremely rich in nitrogen, and in this respect again bear no comparison with the subsoils from which the lucerne, and other plants experimented upon at Rothamsted, are supposed to have taken up much nitrogen.

Thus, whilst in LOGES' experiments, one of the soils acted upon contained 0·804, and the other 0·367 per cent. of nitrogen, the surface soil of the lucerne plot at Rothamsted which yielded such large amounts of nitrogen in the crops contained little more than 0·120 per cent., and the subsoil from which a large quantity of the nitrogen must have been derived, only from 0·04 to 0·05 per cent. Again of the 0·804 per cent. in the one soil, 0·322, or 40 per cent. of the whole was taken up by the acid, and of the 0·367 per cent. in the other soil, 0·083 or 22·6 per cent. of the whole was taken up. In the richer soil the relation of carbon to nitrogen was as 13·78 to 1, and in the other as 11·74 to 1, whilst the relation of carbon to nitrogen in the hydrochloric extract was in the case of the richer soil 6·8 to 1, and in that of the other about 11 to 1.

By phospho-tungstic acid LOGES obtained precipitates from the acid extracts, which in the case of the richer soil showed only 6·67, and in that of the other 5·74 carbon to 1 of nitrogen. In reference to these results, it may be observed that this is approximately the relation of carbon to nitrogen in the raw clay subsoil at Rothamsted below the depth at which it is materially affected by manuring or cropping.

Thus, we have found the proportion of carbon to nitrogen to be in the surface soil of rich prairie, or permanent grass land, between 13 and 14 to 1; in that of somewhat exhausted arable surface soil, between 10 and 11 to 1; and in raw clay subsoil about 6 to 1.

LOGES states that he has experimented on a great variety of soils, and that he has found in all, without exception, that the hydrochloric acid extract gives the phospho-tungstic precipitate; and he hopes soon to be able to report further on the nature of the highly nitrogenous humic compound obtained. It would thus seem, however, to be an amide or peptone body.

It is of much interest that the nature of the nitrogenous body existing in, or dissolved out of, soils and subsoils should be determined; and to this end it seems desirable to act on soils with stronger acids than those hitherto employed in our own experiments. But results so obtained can obviously have only an indirect bearing on the special question we have in view, namely—whether roots do, by virtue of their acid sap, attack the otherwise insoluble organic nitrogen of the soil and subsoil, and either take it up as such, or bring it into a condition in which it is easily susceptible to further change, and so rendered available as a source of nitrogen to the plant?

Still more recently, MM. BERTHELOT and ANDRÉ ('Compt. Rend.,' vol. 103, 1886, p. 1101,) have published the results of experiments to determine the character of the insoluble nitrogenous compounds in soils, and of the changes they undergo, when submitted to the action of hydrochloric acid, of various strengths, for shorter or longer periods, and at different temperatures. The soil they employed contained 0·1744 per cent. of nitrogen. It was therefore much richer than our lucerne surface soil, and about four times as rich as our wheat-fallow subsoil. It was shaken in a flask with water, or dilute hydrochloric acid, in the proportion of 500 grams soil to 1000 c.c. liquid. The clear liquid was just neutralised by potash, then made slightly acid, calcined magnesia added, and the ammonia distilled off, collected, and determined. The remaining liquid was then acidified by sulphuric acid, evaporated to dryness, and the nitrogen determined by the soda-lime method, the result indicating the amount of soluble amide. The following is a summary of their results, which we give in parts of nitrogen per million soil, so as to compare with our own :—

	Nitrogen per million soil.					
	As ammonia.			As soluble amide.		
	18 hours, cold.	5 days, cold.	2 hours, at 100° C.	18 hours, cold.	5 days, cold.	2 hours, at 100° C.
1. Pure water	1·7			8·3		
2. 10 c.c. hydrochloric acid to 400 water (= HCl 3·5 gr.)	4·8	8·75	48·8	27·75	30·25	123·6
3. 50 c.c. hydrochloric acid to 400 water (= HCl 17·5 gr.)	14·4	21·4	101·0	60·6	90·5	356 9
4. 100 c.c. hydrochloric acid to 400 water (= HCl 35 gr.)	14·9	30·4	124·1	68·6	96·5	430·3

The authors call attention to the facts, which are clearly brought to view in the above arrangement of their results, that the amounts, both of ammonia, and of soluble amide obtained, increase with the strength of the acid, the time of contact, and the temperature. They point out that these are products of the action of the acid on certain insoluble nitrogenous bodies in the soil, and that the reaction is similar to that which they have observed in the case of urea, asparagin, and oxamid—that is with well defined amides. The insoluble nitrogenous compounds in the soil are in fact, as previously supposed, amide bodies. They also call attention to the fact that when the clear, filtered, acid extract is exactly neutralised by potash, one portion of the amide still remains soluble, whilst another is precipitated, showing that the amides rendered soluble constitute two groups. The fact of such re-precipitation is quite in accordance with the results obtained in our own experiments, in which less nitrogen remained dissolved after 24 than after only 1 hour's contact, when, with the longer contact, the acidity of the extract became neutralised.

A special point of interest in these results, as compared with those of LOGES, is in the gradation of effect under the varying conditions as to strength, time, and temperature, and in the evidence as to the proportion of the total nitrogen taken up, which is found as ammonia. The proportion of ammonia-nitrogen to amide-nitrogen ranges from about 1 to 5 to 1 to 3, dependent on the conditions. According to the figures, it would seem that the proportion of the total nitrogen dissolved which is determined as ammonia is the greater, the stronger the acid and the longer the contact.

As in LOGES' experiments, so in these of MM. BERTHELOT and ANDRÉ, the strength of acid used was in all cases much greater than that in any of the Rothamsted experiments, and very much greater than is likely to occur in any root-sap. Indeed, not only was the soil operated upon by MM. BERTHELOT and ANDRÉ about four times as rich in nitrogen as the Rothamsted subsoils, but in the most extreme case, that with the strongest acid, and a temperature of 100° C., nearly one-third of the total nitrogen of the soil was dissolved. Hence, although their results are of great interest as indicating the character of the nitrogenous bodies existing in soils, and of the changes to which they are subject when acted upon by acids, they, like those of LOGES, have only an indirect bearing on the question whether by the action of the organic acids of the root-sap, the insoluble organic nitrogen of the soil, and especially of the subsoil, is rendered available as a source of nitrogen to the plant. Supposing this to be the case, as already said, the further question still remains—whether the dissolved amide is taken up as such, or whether it is subject to further change within the soil before serving as food for the plant?

The fact that the formation of ammonia seems to be an essential element in the reaction, points to the conclusion that at any rate part of the nitrogen liberated from the insoluble condition is available in other forms than as soluble or dissolved amide; and, as our experiments show that nitric acid, as well as ammonia, is a constituent of the root-sap, the question arises—whether the liberated ammonia is oxidated into nitric acid before being taken up? Then, again, is the soluble amide taken up as such, or subjected to further change, perhaps first yielding ammonia, and this again nitric acid?

On this supposition we are met again with the difficulty as to the sufficient aëration of the subsoil for such a purpose. It has already been pointed out, that such evidence as exists on the subject clearly shows that the amount of oxygen within the soil at any one time is totally inadequate for the nitrification of the amount of nitrogen taken up by some plants within the season; whilst the replacement by air of the water evaporated would still be quite insufficient. With what rapidity, or to what extent, the oxygen of the subsoil air would be replenished from above as it is used up, there is no experimental evidence at command to show. But whether this would take place adequately or not, it must be supposed that it would occur to some extent.

Turning to the alternative aspect, inasmuch as the insoluble organic nitrogen of the soil exists in the condition of amide bodies, and the chief first product of the action of acids is soluble amide, it is of interest to consider, whether plants can take up such bodies and assimilate their nitrogen? There can be little doubt that fungi can utilise both the organic carbon and the organic nitrogen of the soil, though they seem to develop the more freely when the humic matters have not undergone the final stages of change by which the compound of so low a proportion of carbon to nitrogen as is found in raw subsoils, has been produced.

8. *Evidence as to whether Chlorophyllous Plants can take up Complex Nitrogenous Bodies, and Assimilate their Nitrogen.*

The first direct experiments to determine whether green leaved plants can take up organic nitrogen were made in 1857 by Dr. (now Sir CHARLES) CAMERON. He experimented with barley, in an artificial soil, and found that when urea was the only soil-source of nitrogen, the plants grew luxuriantly, and took up much of the nitrogen so supplied. Ammonia was not detected in the soil. Hence he concluded that the urea was taken up by the plant as such. No reference is made to nitric acid, and in the absence of evidence to the contrary, it is possible that nitrates were formed, and served as the source of nitrogen to the plants.

In 1861, Professor S. W. JOHNSON, of Yale, made experiments with maize in an artificial soil. A given quantity of nitrogen was supplied, in one case as uric acid, in a second as hippuric acid, and in a third as guanine. Compared with results in a control experiment without nitrogenous supply, the growth was very greatly increased; and there was no doubt that the substances named had supplied nitrogen to the plants. Professor JOHNSON states that the conditions of the experiments were not such as to demonstrate that the nitrogenous organic bodies entered the plants without previous decomposition, but from the results of CAMERON, and of HAMPE, he concludes that this was the case.

In 1865, 1866, and 1867, Dr. W. HAMPE ('Versuchs-Stationen,' vol. 7, p. 308, vol. 8, p. 225, vol. 9, p. 49, and vol. 10, p. 175) made several series of experiments, all by the water-culture method. Maize was the plant selected, and the sources of nitrogen supply were—urea, ammonium phosphate, uric acid, hippuric acid, and glycocoll.

At first the experiments with urea were not very successful, apparently owing to an unfavourable condition of the solutions as to mineral supply. Afterwards the plants produced were nearly as good as those grown in a garden; ripe seeds being formed, which, when sown, germinated.

Urea was found in the leaves, stems, and roots. Small quantities of ammonia were sometimes found in the solutions, but only when there was some decomposition of the roots or their excretions, and such formation of ammonia was the most prominent after the blooming. To obviate such formation as far as possible, the solutions were in the later experiments frequently renewed. In corresponding solutions without plants, ammonia was only found twice throughout the summer. Neither nitrates nor nitrites were ever found in the solutions.

HAMPE concluded that the urea was taken up by the plants as such, and that it served as a source of nitrogen to them. This result he considered not inconsistent with the view that plants, other than fungi, cannot utilise products of the plant itself, such as the alkaloids, which they can no more assimilate as a source of nitrogen, than they can sugar as a source of carbon. Urea being, on the other hand, a product of the degradation of animal substance, there seemed no reason why it should not serve as a source of nitrogen to plants.

In his experiments with ammonia HAMPE used the phosphate; and small and large maize were the plants selected. In their early stages the plants seemed to suffer rather than to benefit by the ammoniacal supply; but eventually they gave good growth, and produced ripe seeds, which on being re-sown germinated.

In the experiments with uric acid the same descriptions of maize were employed. The solutions were frequently renewed, and in those which were removed ammonia was always found, but not uric acid. But even in solutions without a plant the uric acid rapidly decomposed. From the results it was concluded that the uric acid had served as a source of nitrogen to the plants; though probably not directly, but by its products of decomposition.

In the case of hippuric acid, applied as hippurate of potash, and to the same descriptions of plant, the growth was somewhat dwarf; but seeds, which were found to germinate, were produced. Benzoic acid was always found in the solutions after vegetation, and also in corresponding solutions without a plant. In both cases a mould formed on the surface, but not in the body of the liquid. The question arose whether the benzoic acid was only formed in the solution under the influence of the mould acting as a ferment, or whether in part in the plant itself, glycocoll being at the same time produced, and serving as the nitrogenous supply? If the latter were the case, the action would be the convrese of that which takes place in the animal, when benzoic acid unites with glycocoll, forming hippuric acid, which is eliminated.

Direct experiments were also made with glycocoll itself. With it, the plants were better than in any of the other experiments. At each renewal of the solution, the old liquid was examined both for glycocoll and for ammonia. Glycocoll was always found, but ammonia only in very small quantity, and its occurrence was apparently connected with decomposition of plant-substance. HAMPE concluded that glycocoll was as available as nitrogenous food to plants as nitric acid.

In 1868 Dr. P. WAGNER ('Versuchs-Stationen,' vol. 11, p. 287) made experiments in continuation of those by HAMPE above described. He repeated, with some modifications, the experiments with ammonium salts, hippuric acid, and glycin, and also experimented with kreatin.

With ammonium phosphate good growth was obtained. Neither nitrate nor nitrite could be found in the plant, and it was concluded that, as in HAMPE's experiments, the ammonia had served as a supply of nitrogen.

When ammonium carbonate was used, nitric acid was found both in the solution and in the plant; and it was concluded that the ammonia had not served directly as a supply of nitrogen.

In HAMPE's experiments with hippuric acid, it was proved that it served as a supply of nitrogen to the plant; but as benzoic acid was found not only in the vegetation solution, but in a corresponding solution without a plant, and there was, in both cases, fungoid growth on the surface, it was uncertain whether the breaking up of the hippuric acid had taken place only externally to the plant, under the influence of the fungus acting as a ferment, or also within the plant itself, benzoic acid being excreted. By excluding the access of the air, and by frequently passing carbonic acid through the solutions, the formation of the fungus was prevented. Benzoic acid was, however, still found in the plant-solution, but not in the solution without a plant.

WAGNER concluded that hippuric acid was broken up within the plant itself, benzoic acid being excreted, and that it also suffered decomposition in the solution by the agency of the fungus.

HAMPE had obtained very good growth with glycin, but WAGNER thought it desirable to prevent the formation of the mould on the surface of the solution. This he succeeded in doing by frequently passing carbonic acid through it, and glycin was then easily detected in it. Ammonia was only found when there was some decay of the roots. WAGNER concluded that the glycin had been taken up by the plant as such, and had contributed nitrogen to it.

Kreatin was used as being closely allied to urea, which had been proved to serve as a supply of nitrogen to plants. For some time neither mould, nor ammonia, nor smell, was developed in the vegetation-solution; when they were, it was renewed; and some ammonia again appeared when the roots showed signs of decay. WAGNER could not detect kreatin in the plant, as HAMPE had urea. But from its constant presence in the solution, and the very little development of ammonia, he concluded that it served as nitrogenous food to the plant as did urea.

WAGNER considered it established that the higher plants can obtain nitrogen from complex organic bodies as well as from ammonia and nitrous and nitric acids, and that thus the doctrine of the nutrition of plants was much extended. He did not suppose that such a source was essential, and whether in the case of plants growing in soil such substances would serve as a direct supply would depend on the length of time they could remain in such a medium in an undecomposed condition.

The last experiments of this description to notice are those of W. WOLFF with tyrosin ('Versuchs-Stationen,' vol. 10, p. 13). He had formerly experimented with KNOP, on leucine, tyrosin, and glycocoll; and he now repeated the experiment with tyrosin, to determine whether it served directly, or only by its products of decomposition, as the source of nitrogen. To this end the water-culture method was adopted, and rye was the plant selected.

According to the report, the vegetation went on for more than a year—430 days! The amount of the dry substance produced was 365 times that of the seed sown; but no seed was developed. Neither ammonia nor nitric acid was found in the solutions. But, on boiling, a small quantity of an organic body was deposited. From 4·5 grams tyrosin the vegetation acquired 0·18 nitrogen, corresponding to 2·3 tyrosin. No tyrosin was found in the extract of the stems and leaves, but traces were detected in that of the roots.

W. Wolff concluded that tyrosin suffered change as soon as it entered the plant, and that thus the action differed from that found by Hampe in the case of urea. He considered that the tyrosin was, at any rate in part, transformed in the solution, under the influence of the roots; but that ammonia was not one of the products of the change. If the tyrosin were taken up at all as such by the roots, it did not pass unchanged to the upper organs; but when its nitrogen, in whatever form, was assimilated by the plant, it was distributed through the various organs, as in the case of land plants growing under natural conditions.

From the various results above quoted it seems at any rate very probable, if not absolutely demonstrated, that green-leaved plants can take up soluble complex organic bodies, and assimilate their nitrogen, when they are presented to them under such conditions as in water-culture experiments. Even under such conditions, however, if the nitrogenous substance supplied was readily subject to change in the solution itself, it was doubtful whether it was taken up as such, or only after first undergoing change; and it is pretty certain that such substances supplied to the soil, would either in great part or entirely suffer change before being taken up by the plant.

The probability that the higher plants can, under any circumstances, take up complex nitrogenous bodies, and appropriate their nitrogen, is of considerable interest from a theoretical point of view. But under the ordinary condition of the growth of plants in soil, such substances will seldom if ever be available to them, excepting it may be under the influence of the action of the root-sap in rendering soluble the nitrogenous compounds of the soil and subsoil, which exist in them in an insoluble condition.

It will be of interest next to consider what evidence exists as to other modes in which green-leaved plants may acquire nutriment from compounds existing in an insoluble condition in the soil and subsoil.

Dr. Frank has observed that the feeding roots of certain trees are covered with a fungus, the threads of which force themselves between the epidermal cells into the root itself, investing the cell, but not penetrating the fibro-vascular tissue. In such cases the root itself has no hairs; but there were similar bodies external to the fungus-mantle, which were prolonged into threads among the particles of soil. The fungus-mantle dies off on the older portions of the root, and its extension is confined to the younger roots—those which are active in the acquirement of nutriment.

This fungus development was always observed in the case of teaks, beeches, hornbeams, and hazels:—in seedlings of 1, 2, or 3 years old, and in trees more than a century old. It was, however, not found on the roots of the associated woodland plants, even when these were growing close to a tuft of the mycorhiza. Nor was it found on the roots of maples, elms, alders, birches, mulberry, buckthorn, planes, walnut, apple, service-tree, hawthorn, cherry, cornel ash, syringa, or elder, &c. Thus, the majority of woodland trees appear to be free, and the occurrence seemed to be almost limited to the Cupuliferæ; though outside of this family the development has nevertheless been observed, as on willows, and some conifers; and it is supposed probable that it may be found to be more general as investigation extends.

In the case of the Cupuliferæ the occurrence seems to be universal. It has been observed in the most widely distant localities, at very different altitudes, in very different aspects, in soils of the most varied geological character, and with very varied amounts of humus, with great variation in the associated herbage, and even in a flower-pot. The growth is perhaps the most luxuriant on chalk soils. It is also the more developed in the first 2 inches, or the richer-in-humus layer of the soil.

The occurrence of a fungus on the roots of certain trees has indeed been recorded before. It has sometimes been considered to be connected with a diseased condition, though it has also been noticed on healthy trees. The observations have, however, not before been generalised.

FRANK considers that the conditions are those of true symbiosis. He in fact concluded that the chlorophyllous tree acquires the carbon, and the fungus the water and the mineral matters, that is the soil nutriment.

FRANK did not refer to nitrogen. But there is no reason to suppose that the fungus could not, as do the fungi in the case of fairy rings for example, avail itself of the organic nitrogen of the soil.

Here then we have a mode of accumulation of soil nutriment by some green-leaved plants, which so far allies them very closely to fungi themselves. Indeed, it is by an action on the soil which characterises non-chlorophyllous plants, and by virtue of which they are enabled to take up nutriment not available to most green-leaved plants, that the chlorophyllous plant itself acquires its soil-supplies of nutriment. Under such circumstances, it can indeed readily be supposed that the tree may acquire not only water and mineral matter, but organic nitrogen from the soil, and if so probably organic carbon also. In reference to this point, it has already been stated that, from the evidence so far at command, it was concluded that the action is the most marked in the surface layers of the soil rich in humus.

So far as this is the case, it is obvious that such an action of fungi on the soil does not aid us in the explanation of the acquirement of nitrogen from raw clay subsoil by the deep and strong rooted Leguminosæ. Further, it is distinctly stated that the fungus development in question has not been observed on the roots of any herbaceous plants. It is nevertheless a point of interest, should it be established, that by special means, in special cases, the organic nitrogen of the soil may serve as a supply of nitrogen to chlorophyllous plants. To this point further reference will be made in the course of the discussions upon which we have now to enter.

PART II.

Recent Results and Conclusions of others, relating to the Fixation of Free Nitrogen.

In our introductory remarks it was stated that the object of the present paper was not only to discuss our own results bearing on various aspects of the question of the sources of the nitrogen of vegetation, but to consider the recent results and conclusions of others, and to endeavour to determine how far the evidence yet available is conclusive on the subject. And, as there can be no doubt that the Memoirs of M. Berthelot have materially influenced the course of inquiry in recent years, it will be well to commence with a statement and discussion of his results and conclusions.

1. *The Experiments of* M. Berthelot.

It was, we believe in 1876, that M. Berthelot first called in question the legitimacy of the conclusion that plants do not assimilate the free nitrogen of the air, when drawn from the results of experiments in which the plants were so enclosed as to exclude the possibility of electrical action. More recently he has objected to experiments so conducted with sterilised materials, on the ground that, under such conditions, the presence, development, and action of micro-organisms are excluded. Such objections, if valid, of course put out of court the results and conclusions of Boussingault, ourselves, and others, from experiments so conducted. They at the same time, obviously suggest, though it is true they do not actually necessitate, the adoption of less exact methods of experimenting—methods in which the soils, or plants, or both, are almost unavoidably exposed to accidental sources of unknown amounts of combined nitrogen, and in which the personal equation becomes a very prominent element. At any rate, since the announcement and acceptance of M. Berthelot's objections, numerous experiments have been made without the enclosure of the plants; and results have been obtained showing very various and, in some cases, very large gains of nitrogen, assumed to be due to the fixation of the free nitrogen of the air in some way.

In 1876 ('Compt. Rend.,' vol. 72, pp. 1283–5) M. Berthelot published the results of experiments in which he found that free nitrogen was fixed by various organic compounds, under the influence of the silent electric discharge, at the ordinary temperature. Such fixation was determined in the cases of benzene, oil of turpentine, marsh gas, and acetylene. In each case a solid nitrogenous body was obtained, from which ammonia was evolved on strongly heating. The electricity was developed by a large Ruhmkorff coil, so that the conditions were comparable with those between the clouds and the ground during a thunder-storm, and the application of the results to vegetation was legitimate for such conditions. He suggests that similar reactions probably take place in the air during storms, and when the air is charged with electricity, organic matters absorbing nitrogen and oxygen.

Again in 1876 ('Compt. Rend.,' vol. 82, pp. 1357–1360), he recurs to the subject. He says that under the influence of the silent electric discharge, nitrogen, whether pure or mixed with oxygen, is fixed by moist filter paper, and by dextrine, to a degree that is very noticeable within a few hours. Neither ammonia, nor any nitrogen acid is a product of the reaction; and thus the fixation may take place in nature without the preliminary formation of ozone, ammonia, or nitrogen acids.

Subsequently ('Compt. Rend.,' vol. 83, 1876, pp. 677–682), he used currents of much weaker tension, more comparable with those incessantly occurring in the air, and the substances experimented upon were moistened filter paper, and a strong solution of dextrine. The tension would correspond to that between the ground and a layer of air two metres above it. The experiments lasted about two months, during which, however, the tension varied considerably, but averaged $3\frac{1}{2}$ elements Daniell. In all cases nitrogen was fixed by the organic substance, forming a nitrogenous compound from which ammonia was evolved by soda-lime.

In 1877 ('Compt. Rend.,' vol. 85, p. 173) he gives further results of the same kind. In experiments in which the difference of electrical potential was not greater than that frequently existing between strata of the atmosphere not far from the ground, he found that filter paper moistened with water and containing 0·010 per cent. of nitrogen, after a month contained 0·045 per cent., whilst similar paper moistened with a solution of dextrine had its percentage of nitrogen raised from 0·012 to 0·192. He considered that his experiments indicated the true explanation of the fixation of nitrogen in nature. The gains are in amount such as would explain how crops acquire the amounts of nitrogen which he considers they must derive from the atmosphere.

In the autumn of 1885 ('Compt. Rend.,' vol. 101, pp. 775–784) M. BERTHELOT gave the results of experiments on the fixation of atmospheric nitrogen by certain argillaceous earths. He refers to his experiments which established the fact that nitrogen was fixed in some of the immediate principles of plants by the agency of electricity of such feeble tension as is operative all over the globe. He has now to call attention to another mode in which free nitrogen is brought into combination—namely by argillaceous soils under the influence of micro-organisms.

He experimented with two argillaceous sands, and two pure clays—crude kaolins. Some of the experiments were commenced in 1884, but others not until April, 1885;

and for all of these comparative results are given for the period from April 30 to October 10, 1885. It may be mentioned that at the commencement of this period the initial amounts of nitrogen in these materials were very much lower than in any cultivated soils, being respectively 0·0091, 0·01119, 0·0021, and 0·01065 per cent.

Each of these descriptions of soil was exposed under the following conditions :—

1. From 50 to 60 kilog., in open glazed pots, in a closed chamber free from emanations.
2. From 0·08 to 0·10 mm. depth of soil, in open pots, on a trestle 0·7 metre above the ground, in a meadow, with a roof protecting from vertical, but not from oblique rain, or from free air.
3. In similar pots, uncovered, placed on a plank on a tower 29 metres high.
4. 1 kilog. soil, placed in a 4-litre flask, moistened, and closed with a ground stopper; one set being exposed to diffused day light, and a duplicate set kept in a closed cupboard.
5. 1 kilog. of soil put into a 4-litre balloon, heated at 100° C. for 2 hours, steam passed through for 5 minutes, and cooled in filtered air previously heated to 130° C.; then closed and exposed from July 10 to October 6, 1885.

The following tabular statement, summarises the results obtained in the first, second, third, and fourth series of experiments. The upper division shows the actual percentages of nitrogen found, before and after exposure, and the lower division, the gains in the percentage of nitrogen. We give the results in percentages, instead of in parts per kilogram, to compare the better with the figures given relating to our own experiments.

	Initial.	In closed chamber.	In meadow.	On tower.	In closed flasks in light.
Nitrogen found—per cent.					
Yellow argillaceous sand I.	0·00910	0·01179	0·00983	..	0·01289
„ „ „ II.	0·01119	0·01639	0·01295	0·01396	0·01503
White clay	0·00210	0·00407	0·00353	0·00557	0·00494
Crude kaolin	0·01065	..	0·01144	0·01497	0·01236
Gain of nitrogen—per cent.					
Yellow argillaceous sand I.		0·00269	0·00073	..	0·00379
„ „ „ II.		0·00520	0·00176	0·00277	0·00384
White clay		0·00197	0·00143	0·00347	0·00284
Crude kaolin		..	0·00079	0·00432	0·00171

Thus, although the actual amounts of gain are small, there is in every case some gain. Determinations of nitric acid and ammonia showed that the gains were not correlative with the amounts of either. Further, calculations showed that the amounts far exceeded those which could be due to ammonia in the air, or to ammonia and nitric acid in the rain; whilst the gains in the closed flasks showed that they could not be due to combined nitrogen from the air or rain.

The author considers the results establish the fact that there is gain of nitrogen quite independently of any absorption of combined nitrogen.

From the evidence so far it might be concluded that the gains in the meadow and on the tower were due to electrical action; but the fifth series of experiments, in which the soils were sterilised by heat, and then left in the balloons from July 10 to October 6, 1885, indicate another influence. In the case of each of the four soils so sterilised, and afterwards exposed, there was, instead of any gain, a slight loss of nitrogen, which was attributed to the heating at the commencement. The cause of the fixation of nitrogen had at the same time been destroyed; nor did the soils recover the power of fixing nitrogen, either by exposure to the air of the chamber, or when a small quantity of the unsterilised soil was added.

It was concluded that there was a fixation of free nitrogen due to living organisms. It was shown that the action was not manifested during the winter, that it was the most effective during the periods of active vegetation, and that it was exercised in closed vessels as well as in the free air.

M. Berthelot estimated that the gains corresponded to gains of nitrogen per hectare of 20 kilog. by sand No. 1, of 16 and 25 kilog. by sand No. 2, and of 32 kilog. by the kaolin No. 3. These estimates are, however, said to be much too low, as they are on the assumption of only 0·08 or 0·10 m. depth of soil, whilst the action extends much deeper. He compares these amounts with 17 kilog. the amount of combined nitrogen in the rain, &c., at Montsouris in 1883; and with 8 kilog., the amount formerly estimated as annually so coming down at Rothamsted; which, however, more recently we have estimated at less than this. On the other hand, taking the amount of nitrogen removed in a crop of hay at from 50 to 60 kilog. per hectare (= 45 to 54 lbs. per acre), he estimates that the loss to the soil will be from 40 to 50 kilog. per hectare (=36 to 45 lbs. per acre). Hence, if it were not for compensation by fixation of free nitrogen, the soil would gradually become exhausted. He considers that the results bring to view not only one of the methods by which fertility is maintained, but that they also show how argillaceous soils, which are almost sterile when first brought into contact with the air, come to yield more and more flourishing crops, and in time become vegetable moulds.

Quite recently, March, 1887 ('Compt. Rend.,' vol. 104, pp. 625 *et seq.*), M. Berthelot has published the results of experiments on the fixation of free nitrogen by vegetable mould supporting vegetation. The experiments were commenced in May, and concluded in November, 1886. He determined the nitrogen by the soda-lime method, and also as nitric acid, in the soil before and after the growth; also in the initial plants (*Amaranthus pyramidalis*), and in the final products. He also determined the amount of atmospheric ammonia absorbed by sulphuric acid, and the amount of combined nitrogen in the rain; and finally the amount of combined nitrogen in the drainage waters.

The following is a summary of the amounts of nitrogen involved (in grams):—

Initial—In soil 54·09, in rain 0·053, in ammonia of air 0·048, in plants 0·35, total	= 54·541
Final—In soil 56·54, in drainage 0·403, in plants 2·235, total .	= 59·178
Gain	= 4·637

M. Berthelot points out that the gain of nitrogen is nearly equally divided between the soil and the plant, the latter having taken it up from the soil, which he considers is the true source of the gain. He compares the results with those formerly obtained without vegetation thus—

Gains with vegetation	4·64 and 7·58 grams.	
Gains without vegetation	12·70 and 23·15 grams.	

He assumes that there is with the higher plants, as with animals, a constant loss of nitrogen. He admits however that more evidence is needed absolutely to demonstrate that the plants themselves do not fix, and that they do set free, nitrogen. But he considers it proved by his experiments that vegetable soil does fix free nitrogen; and he thinks it probable that this is the chief source of the gain by the higher plants. Thus, it can be understood how concentrated production exhausts faster than the natural actions restore fertility; whilst in natural vegetation, on the other hand, the fixing of nitrogen may exceed the liberation, and accumulation may thus take place.

Reviewing the whole of these results and conclusions of M. Berthelot, it is in the first place to be observed that whilst the results obtained under the influence of the silent discharge in bringing nitrogen into combination with certain vegetable principles, owed their special interest to the inference that thus free nitrogen might be brought into combination within the plant, he now considers it at least doubtful, whether the higher plants do bring free nitrogen into combination at all, and that probably the gain of nitrogen is by the soil, and not by the plant.

Obviously, if there are organic compounds existing within the soil which have the power of bringing free nitrogen into combination under the influence of electricity of feeble tension, such as occurs in the atmosphere, the soil and not the plant may be the source, and yet the agent be the feeble electric current. So far, however, as it is assumed that nitrogen is so brought into combination in the atmosphere itself, the resulting compound or compounds will be found in the air, and in the aqueous depositions from it; and the extent, or rather the limit, of the amount of combined nitrogen so available over a given area, in Europe at any rate, is pretty well known.

As to the results obtained with soils, with and without vegetation, it must be admitted that M. BERTHELOT has carefully considered, and endeavoured to estimate, all other apparent sources than free nitrogen. At the same time, the conditions of risk and exposure to accidental sources of gain in the experiments in the chamber, in the meadow, and on the tower, are such that the results could not of themselves be accepted as at all conclusive. To the distinct gains observed in the experiments in closed vessels no such objection can however be raised; whilst the negative results in the sterilised soils constitute another element in favour of the conclusion at which M. BERTHELOT has arrived.

It is, however, one thing to accept experimental results on the authority of M. BERTHELOT, and another to adopt his arguments and conclusions in the application of them to the conditions of practical agriculture. To avoid repetition, however, further reference to this part of the subject must be postponed until the results and conclusions of other experimenters have been considered; for, to a great extent, the same facts and arguments are applicable in reference to them, as to M. BERTHELOT'S results and conclusions.

2. *The Experiments of* M. P. P. DEHÉRAIN.*

The plan and methods of M. DEHÉRAIN'S experiments to determine the losses or gains of combined nitrogen were totally different from those adopted by M. BERTHELOT. They were indeed much on the lines of some of the Rothamsted investigations. He sought to determine the actual losses or gains in the field, under the influence of different manures, of different crops, and of different modes of cultivation. His experiments were made on the farm of the Agricultural School, at Grignon, near Paris, and extended from 1875 to 1885 inclusive. The land had been in lucerne for 5 years, 1870–1874. Four plots were then devoted to each experimental crop as under:—

No. 1 received farm-yard manure, No. 2 nitrate of soda, No. 3 ammonium sulphate, and No. 4 was left unmanured. Each of the manures was applied 3 years in succession, and then the crops were grown for four years more without further manuring.

On one of the sets of four differently manured plots, green maize was grown. On a second set potatoes were grown during the 3 years of manuring, and for two years afterwards, and then wheat for the two remaining years. On the third set beet was grown for 3 years, green maize for 1 year, and then sainfoin for 5 years, and mixed grasses for 2 years, to 1885 inclusive.

The nitrogen was determined in the soil, before the commencement of the experiments in 1875, in 1878 after the three years of manuring and cropping, in 1881 after 4 years cropping without further manuring, and in case of the sainfoin followed by mixed grasses, in 1885 also. Lastly the nitrogen was estimated in the crops. From

* 'Annales Agronomiques,' vol. 8, p. 321, vol. 12, p. 17, and vol. 12, p. 97.

these data, the losses or gains of nitrogen by the soil, during the different periods under the influence of the different manures and crops, were calculated.

M. Dehérain further gives the results of numerous determinations of carbon in the soils, and shows that with a loss of nitrogen there is also a loss of carbon; and that where in the case of the growth of sainfoin, the nitrogen in the surface soil increased, there was not a reduction in the carbon.

In his first paper M. Dehérain summarises his conclusions as follows:—

1. The soil of each experimental plot lost nitrogen from 1875 to 1878 and 1879, when it had grown green maize or potatoes; it also lost when beet was grown.

2. The loss much exceeded the amount due to the removal of the crop.

3. The loss was very sensible even when the soil received abundance of manure, and it continued from 1878 or 1879 to 1881, when the soil grew maize, or potatoes followed by wheat.

4. When, from 1879 to 1881, sainfoin was substituted for beet, not only was loss no longer manifested, but the nitrogen of the soil augmented, and at the same time abundant crops of sainfoin were obtained, which contained large quantities of nitrogen.

5. This nitrogen has not come from the deeper layers of the soil, for these showed an equal, or even rather greater richness in 1881 than in 1879.

With regard to the actual amounts of loss or gain of nitrogen found in M. Dehérain's experiments, the losses especially are extremely large, as the following results will show:—

When farm-yard manure was applied, in very heavy dressings for three years in succession, in amount estimated to supply 400 kilog. nitrogen per hectare per annum (= 357 lbs. per acre per annum), there was, when green maize was grown, a loss of nitrogen by the soil, besides that removed in the crops, amounting to 288 kilog. per hectare (= 257 lbs. per acre) per annum, over the 3 years of the application; when potatoes were grown there was a loss of 242 kilog. per hectare (= 216 lbs. per acre) per annum; and when, with the same manuring for 3 years, beet was grown for 3 years and maize for one year, there was an average annual loss over the 4 years of 679 kilog. nitrogen per hectare (= 606 lbs. per acre).

When nitrate of soda, supplying 192 kilog. nitrogen per hectare (= 171 lbs. per acre) per annum, was applied, the annual loss of nitrogen was, when maize fodder was grown, 401 kilog. per hectare (= 359 lbs. per acre); when potatoes, 436 kilog. per hectare (= 389 lbs. per acre); and when beet was grown for 3 years and maize for one year, the average annual loss of nitrogen by the soil over the four years, besides that removed in the crop, was 557 kilog. per hectare (= 498 lbs. per acre).

When ammonium sulphate was used, supplying annually 252 kilog. nitrogen per hectare (= 225 lbs. per acre), the annual losses were—after the green maize 359 kilog. per hectare (= 321 lbs. per acre), after the potatoes 555 kilog.

(= 496 lbs.), and after the beet and maize 615 kilog. per hectare (= 549 lbs. per acre) per annum.

Lastly, without any manure, the losses were, after 3 years of green maize 379 kilog. per hectare (= 338 lbs. per acre), after 3 years of potatoes 307 kilog. (= 274 lbs.), and after 3 years of beet and 1 year of maize, 476 kilog. per hectare (= 425 lbs. per acre) per annum.

Over the next 4 years, without further manure on the previously manured plots, and still without manure on the previously unmanured plots, the losses of nitrogen by the soil though still large, were generally much less. Besides that in the crops, they were per annum as follows :—

After farm-yard manure, with green maize 133 kilog. per hectare (= 119 lbs. per acre), and with potatoes 2 years and wheat 2 years 308 kilog. per hectare (= 275 lbs. per acre).

After nitrate of soda, with green maize 4 years 206 kilog. per hectare (= 184 lbs. per acre), and with potatoes and wheat, 38 kilog. per hectare (= 34 lbs. per acre).

After ammonium sulphate, with green maize, 148 kilog. per hectare (= 132 lbs. per acre), and after potatoes and wheat 140 kilog. per hectare (= 125 lbs. per acre).

Without manure for 7 years, the annual loss over the last 4 years was with maize 104 kilog. per hectare (= 93 lbs. per acre), whilst with potatoes and wheat there was a gain of 9 kilog. per hectare (= 8 lbs. per acre).

All the losses during the 3 years of the application of the manures, and especially those after 3 years of beet and one year of maize, are far in excess of anything that has come within our own knowledge and experience, and they are in amount such as reflection must show cannot possibly occur in actual practice.

For example, although it is estimated that the farm-yard manure supplied 1200 kilog. nitrogen per hectare (= 1071 lbs. per acre), in the 3 years, and that only 451 kilog. per hectare (= 403 lbs. per acre) were removed in the 3 crops of green maize, leaving a balance of the nitrogen of the manure of 749 kilog. per hectare (= 668 lbs. per acre), yet the surface soil was estimated to lose, not only this amount, but 116 kilog. per hectare (= 104 lbs. per acre) more, or in all 865 kilog. per hectare (= 772 lbs. per acre) in the 3 years. Again, with the same supply, 1200 kilog. nitrogen per hectare (= 1071 lbs. per acre) by manure, in 3 years, and the removal of 561 kilog. per hectare (= 501 lbs. per acre) in beet and maize in 4 years, leaving a balance from the manure of 639 kilog. (= 570 lbs.), the soil is estimated to have lost 2076 kilog. (= 1854 lbs.) more; or in all 2715 kilog. (= 2424 lbs.). It is true that when excessive amounts of farm-yard manure are applied there will probably be some loss by the evolution of free nitrogen, but here the estimated losses amounted to much more than the total nitrogen of the manure after deducting that removed in the crops. Indeed, M. Dehérain calls attention to the fact that, according to the figures, one-fourth of the total nitrogen of the surface soil has been lost! We repeat that such losses certainly do not occur in practical agriculture. But, if such loss could take place with heavy manuring for 3 years, and the removal of 3 crops of beet and one of maize, what would be the result with ordinary manuring and cropping ?

How are these results to be explained ? The accuracy of the analytical results recorded by M. DEHÉRAIN may be taken for granted. It seems to us, however, that in the method of taking the samples of soil for analysis, an explanation may be found ; and we have the less hesitation in suggesting this, since we have found our own early results obtained under somewhat similar conditions, quite inapplicable for anything like accurate estimates of nitrogen per acre.

Perhaps it is no undue assumption to suppose that there has been more experience of soil sampling at Rothamsted than anywhere else ; and we have, accordingly, learnt that very special precautions must be taken, when comparative estimates are to be made of the amount of nitrogen in the soil to a given depth, at different periods. When this is the object, it is absolutely essential that the samples taken should represent very exactly, both the same depth, and the same measure horizontally throughout the depth at the two periods. That is to say, it is essential that they should contain exactly the same proportions of the corresponding layers at the different dates ; and this is the more important when the layer to be estimated includes both the manured and worked surface soil, and some of the unmanured and unworked subsoil.

Our own plan is to drive down a square iron frame, without top or bottom, having an exact measure superficially, and the exact depth for which the result is to be calculated. Even when this method is adopted, serious error may arise if at the different periods the soil is in a different state of consolidation, the result of manuring, the working of the land, the cropping, or the seasons. In other words, it is essential that a sample of a given area and depth should contain the same weight of dry soil at the two periods. We have given an illustration of the error possible, and of the correction necessary, when this is not the case, in a paper we published in 1882.*

Now, according to the description of his method given by M. DEHÉRAIN, he adopted the same plan as we did ourselves in our early experiments ; that is, he took his samples, not by means of a frame of exact dimensions, but merely with a spade, with which it would be quite impossible to take a sample of exactly the same area throughout the depth adopted. Nor was the depth exactly the same in all cases. It is stated that it ranged from 25 to 30 cm. (= 9·8 — 11·8 inches), whilst the calculations per hectare are made for a depth of 35 cm. (= 13·8 inches). It is obvious, that if the samples were only taken to a depth of 25 or 30 cm., and upon the results obtained the calculations were made for a depth of 35 cm., the amount of nitrogen reckoned per acre, or per hectare, must be too high, as the subsoil from 25 or 30 cm. to 35 cm. deep would doubtless contain a much lower percentage of nitrogen than the layer above the depth of 25 or 30 cm. Indeed, M. DEHÉRAIN's determinations of nitrogen in the subsoils showed less than half as high a percentage as in the surface soils.

* "Determinations of Nitrogen in the Soils of some of the Experimental Fields, at Rothamsted, and the Bearing of the Results on the Question of the Sources of the Nitrogen of our Crops," pp. 32 *et seq.*

We trust, therefore, that M. Dehérain will accept our comments in all friendliness, when we say that our own dearly bought experience leads us to believe that the above facts are quite sufficient to render approximately accurate quantitative estimates at the different periods impossible. From the results, it seems probable that the samples taken at the commencement of the experiments in 1875 were less comparable with those of 1878 and 1879, than were those of these later dates with those of 1881 and 1885. The losses indicated were, indeed, in most cases, much less over the second period of 4 years; a result which is, however, doubtless partly due to the fact that no manure was applied during that period. Another reason for concluding that the samples were less truly representative at the commencement in 1875, than afterwards, is that the percentage of nitrogen found at that date (0·204), is high for the depth stated, of arable soil in ordinary agricultural condition. Though, if the soil is naturally very rich, or if it had been treated otherwise than in ordinary agricultural practice, such a percentage is by no means impossible. The percentages of 0·15 and upwards, as afterwards found, are however quite as high as is usual in good, but long worked arable soil, which is only manured and cropped in the ordinary way.

Then as to the amounts of nitrogen estimated to be gained by the soil to the depth of 35 cm. (=13·8 inches) by the growth of sainfoin for 5 years, and of mixed grasses for 2 years. They were, both on the plot where farm-yard manure had previously been applied, and on that which had been unmanured over the 7 years from the commencement, more than was taken off in the crops of that period. This is certainly more than our own experience would lead us to expect.

From his determinations of the nitrogen in the subsoils at different periods, M. Dehérain concluded that the gains were not from that source. The percentage in the subsoils of the different plots varied however considerably; and on this point it may be stated that in the subsoils at Rothamsted, the variations which are quite independent of manuring and cropping, are so great on the same plot, that we have found it quite impracticable to make calculations as to loss or gain in the total nitrogen of the subsoils.

Upon the whole we conclude, that certainly the estimated losses of the surface soils, and probably also the estimated gains, are higher than can possibly happen in practice; and that the results are due to the method of taking the samples of soil not being such as to ensure strictly comparable estimates at the different periods. At the same time there can be no doubt that there would be losses beyond those due to the removal of the crops, under the conditions in which losses were found; that is, when the land was under arable culture. Nor can there be any doubt that there would be gains in the surface soil when the land was laid down in sainfoin and mixed grasses; and M. Dehérain points out the practical significance of such facts.

M. Dehérain concludes that the loss of nitrogen by arable soil, that is by soil that is mechanically worked, is due to the slow combustion of the nitrogenous organic matter of the soil; the nitrogen being either evolved as free nitrogen, or oxidated into nitric acid, and carried down into the subsoil, or into the drains. As to the gain by the surface soil, he considers that part is due to the action of deep-rooted plants, in taking up the nitric acid accumulated in the lower layers, and leaving a nitrogenous residue near the surface; a view in which we fully concur. As to gains not so to be accounted for, he considers it not yet settled whether they are due to the ammonia of the atmosphere, as supposed by M. SCHLŒSING, or to free nitrogen, as supposed by M. BERTHELOT.

In conclusion it may be remarked that, if the losses in ordinary agriculture were in amount anything like those which M. DEHÉRAIN'S figures show, even such large gains as are also indicated, would be far from sufficient to compensate them. It would indeed be necessary to seek for other sources of restoration, if our arable surface soils are not to lose their nitrogen much faster than we believe is the case in actual practice. That they do, however, slowly suffer reduction in their stock of nitrogen, when there is no restoration from without, there can we believe be no doubt. In other words, in actual practice, without such restoration from external sources, the losses are not fully compensated.

3. *The Experiments of* M. H. JOULIE.*

In this and subsequent sections, we have to consider evidence in regard to the fixation of free nitrogen, obtained, not in closed vessels, nor in the open field, but in vegetation experiments in which the soils and the plants were exposed to the free air, with known amounts of combined nitrogen supplied, and with more or less adequate precautions taken to exclude other sources than the free nitrogen of the atmosphere. M. JOULIE'S experiments are, in point of date, the earliest of those we have to notice.

M. JOULIE made two series of experiments on this subject. In the first he used a sandy-clay soil containing 0·104 per cent. of nitrogen, and in the second a sand containing only 0·0069 per cent. of nitrogen. In each series he used glass pots, with glass pans; in the first series he included 12 experiments, each in duplicate; and in the second series 10 experiments, also each in duplicate. Pounded glass was put at the bottom of each pot, and in each case 1500 grams of the matrix was used. Pure distilled water was supplied to the pans, which also received the drainage; and above the level of the water there were slits in the pots, for the aëration of the soil and the roots. The pots were placed on a bench under a glass roof, and were protected from birds and rain by means of wire gauze; the place of experiment being a court-yard of the Municipal Hospital, Rue Faubourg St. Denis, Paris. The different experiments represented so many different conditions as to manuring, those of Series 1, being as under:—

No. 1.—Without manure.

* 'Bulletin de la Société des Agriculteurs de France,' No. 1, Janvier 1, 1886, pp. 19-29.

No. 2.—A complete mineral manure, containing potassium sulphate and chloride, calcium sulphate, bicalcic-phosphate, and magnesium sulphate.

No. 3.—The complete mineral manure, and nitrate of soda = 0·3 gram nitrogen.

No. 4.—The complete mineral manure, and calcium carbonate.

No. 5.—The complete mineral manure, and caustic lime.

No. 6.—The mineral manure, excluding the bicalcic-phosphate, with nitrate of soda = 0·3 gram nitrogen.

No. 7.—The mineral manure, excluding the potash, with nitrate of soda = 0·3 gram nitrogen.

No. 8.—Farm-yard manure, containing 0·4 gram of nitrogen.

No. 9.—The same quantity of farm-yard manure, and calcium carbonate.

No. 10.—The complete mineral manure, and dried blood = 0·4 gram nitrogen.

No. 11.—The complete mineral manure, dried blood as in No. 10, and calcium carbonate.

No. 12.—Farm-yard manure as No. 8, and mineral constituents sufficient to bring the mineral supply up to that of the complete mineral manure.

On June 30, 1883, 6 germinated seeds of buckwheat were sown in each pot. There was great variation in the luxuriance of growth. On September 6, all the crops were cut at the level of the soil, leaving the roots in it. On September 15, after stirring the soils, each pot was re-sown with a mixture of rye-grass and hybrid trefoil. The rye-grass grew slowly through the winter, but the trefoil almost disappeared. In March, 1884, the herbage was cut, a little more rye-grass and trefoil was sown, and a second cutting was taken on June 18. On June 20 nitrate of soda = 0·1 gram nitrogen was supplied to Nos. 3, 6, and 7 ; and on August 21 the last cutting was taken.

The soils of the duplicate pots were mixed, as also were the crops, and the nitrogen was determined in the soils and in the crops separately. The following Table summarises the results obtained :—

Series 1.—Experiments with Sandy-clay Soil.

Experiments.	Manures.	Nitrogen.			Crops, dry.	Nitrogen.			Nitrogen, gain or loss.
		In soil.	In manure.	Total.		In soil.	In crops.	Total.	
		gr.	gr.	gr.	gr.	gr.	gr.	gr.	gr.
1	Without manure	1·56	..	1·56	11·00	1·685	0·3658	2·0508	+ 0·4908
2	Complete mineral manure	1·56	..	1·56	13·45	1·719	0·3540	2·0730	+ 0·5130
3	Mineral manure and nitrate	1·56	0·300	1·86	19·10	1·895	0·5163	2·4113	+ 0·5513
4	Mineral and calcium carbonate . . .	1·56	..	1·56	14·70	1·829	0·3390	2·1680	+ 0·6080
5	Mineral and caustic lime	1·56	..	1·56	13·80	2·042	0·3834	2·4254	+ 0·8654
6	Mineral without phosphate, and nitrate	1·56	0·300	1·86	14·42	1·599	0·4886	2·0876	+ 0·2276
7	Mineral without potash, and nitrate .	1·56	0·300	1·86	8·80	1·825	0·3110	2·1360	+ 0·2760
8	Farm-yard manure	1·56	0·400	1·96	14·35	1·752	0·3754	2·1274	+ 0·1674
9	Do., and calcium carbonate	1·56	0·400	1·96	14·85	1·857	0·3679	2·2249	+ 0·2649
10	Mineral, and dried blood	1·56	0·400	1·96	17·95	1·759	0·5564	2·3154	+ 0·3554
11	Mineral, dried blood, and calcium carbonate	1·56	0·400	1·96	12·80	1·523	0·4234	1·9464	− 0·0136
12	Farm-yard manure and mineral . . .	1·56	0·400	1·96	15·65	1·716	0·3814	2·0974	+ 0·1374

In the results given in the Table no account is taken of the nitrogen in the seed sown, which is estimated at not more than 2 to 3 millig. in the buckwheat, and the same in the rye-grass and trefoil, or in all not more than from 4 to 6 millig., which M. Joulie thinks was largely compensated by leaves of the buckwheat carried away by the wind. He considers that the results are as exact as possible in experiments of the kind. He concludes that, as in M. Berthelot's experiments, the results establish the reality of the fixation of free nitrogen in the presence of clay; and further, that the fixation takes place to a greater extent in the presence of vegetation, when the conditions are favourable for the development of the plants.

In the second series of experiments sand instead of soil was used. The percentage of nitrogen in it was only 0·0069, so that the actual amount of combined nitrogen supplied in the 1500 grams put into each pot, was only 0·1035 gram.

This series included 10 conditions as to manuring, each in duplicate. Each pot of experiments 1 to 8 received the complete mineral manure—No. 1 alone; Nos. 2, 3, 4, and 5, each with 0·3 gram of nitrogen as nitrate of soda; No. 6 with 0·2 gram nitrogen as nitrate of soda (half applied at the commencement and half a month later); No. 7 with 0·2 gram nitrogen as ammonium sulphate; No. 8 with 0·3 gram nitrogen as dried blood; No. 9 received farm-yard manure = 0·3 gram nitrogen, with mineral constituents sufficient to bring the mineral supply up to that by the complete mineral manure; lastly, No. 10 received at the commencement 0·3 gram nitrogen as powdered hay, and later 0·1 gram as nitrate of soda; the mineral composition of the manure being made up as in the case of the farm-yard manure.

On May 25, 1884, 10 germinated seeds of buckwheat were sown in each of the pots, excepting those of Experiment 2, which received only 5, and those of Experiment 3 which received 15. On September 16 the plants were cut, and they and the soils were analysed, the duplicates being mixed as before. The results are given in the following Table.

Series II.—Experiments with Sand as Soil.

Experiments.	Manures.	Nitrogen.			Crops, dry.	Nitrogen.			Nitrogen, gain or loss.
		In Soil.	In Manure.	Total.		In Soil.	In Crops.	Total.	
		gr.	gr.	gr.	gr.	gr.	gr.	gr.	gr.
1	Complete mineral manure	0·1035	. .	0·1035	0·970	0·1455	0·0290	0·1745	+ 0·0710
2	Do., and nitrate soda	0·1035	0·300	0·4035	6·825	0·3270	0·1405	0·4675	+ 0·0640
3	Do., do.	0·1035	0·300	0·4035	6·585	0·3060	0·1680	0·4740	+ 0·0705
4	Do., do.	0·1035	0·300	0·4035	5·890	0·4080	0·1375	0·5455	+ 0·1420
5	Do., do.	0·1035	0·300	0·4035	7·850	0·2280	0·2525	0·4805	+ 0·0770
6	Do., do.	0·1035	0·200	0·3035	7·612	0·2280	0·2420	0·4700	+ 0·1665
7	Do., and ammonium sulphate . .	0·1035	0·200	0·3035	6·425	0·1850	0·2315	0·4165	+ 0·1130
8	Do., and dried blood	0·1035	0·300	0·4035	5·600	0·2685	0·1375	0·4060	+ 0·0025
9	Do., and farm-yard manure . . .	0·1035	0·300	0·4035	4·572	0·4350	0·1205	0·5555	+ 0·1520
10	Do., powdered hay, and nitrate soda	0·1035	0·400	0·5035	1·225	0·3570	0·0390	0·3960	− 0·1075

In the experiments of the first series, with a range of from 1·56 to 1·96 gram of combined nitrogen supplied, there were, in several cases, indicated gains of 0·5 gram, or more; and in one case the gain amounted to 0·865 gram. In the experiments of the second series, with a total supply in sand and manure generally ranging from 0·3 to 0·5 gram, there was, in one case a loss, in five cases the gain was less than 0·1 gram, and in no instance did it reach 0·2 gram. M. JOULIE attributes the less amount of gain in the second series, to the much shorter period of vegetation involved.

Reviewing the results of the two series of experiments, M. JOULIE says the variable quantities of nitrogen gained cannot be attributed to dust, ammonia, or other compounds of nitrogen, in the air, as all the pots were equally exposed to these; whilst there is a range from 0·1075 gram loss, to 0·8654 gram gain of nitrogen, the difference amounting to nearly 1 gram. The result must be due, therefore, to the fixation of the free nitrogen of the air, either in the soil or by the plant. M. BERTHELOT attributed the result in his experiments to the clay soil, under the influence of microbes; but M. JOULIE cannot go so far. It was, however, true that, in his experiments, the surface of the water in the pots and the surface of the soils showed myriads of microbes. He asks—if such bodies can cause the fixation of free nitrogen, why should not grouped cells, as in the case of the higher plants, have the same power?

He further says—as plants have the power of causing the combination of carbon with the elements of water, after having decomposed carbonic acid, whilst chemists can only reduce it to carbonic oxide; as MM. THENARD have succeeded in bringing nitrogen into combination with the elements of water; and as M. BERTHELOT has shown that free nitrogen is brought into combination with dextrine and cellulose under the influence of the silent electric discharge—it is only a logical consequence that free nitrogen should be brought into combination within the plant. In reference to this argument it may be oberved that the parallelism of the action by which free nitrogen combines with the elements of water in the laboratory, with that by which carbon and the elements of water combine within the plant, only holds good on the assumption that the carbon of the carbonic acid is first reduced to the free state, and so combines with the elements of water, without the intervention of its own oxygen.

M. JOULIE compares the amounts of nitrogen fixed in his various experiments with the amounts of crop produced, and observes that the gains have no relation to the amount of vegetation. He next comments on the connection between the condition of manuring of the various soils, and the amounts of nitrogen gained. Referring to the results of the first series of experiments, he points out that whilst without manure the gain was 0·491 gram, it was raised to 0·513 gram by purely mineral manure. Again, whilst the addition to the mineral manure of 0·3 gram nitrogen as nitrate of soda only gave a further gain of 0·038 gram, and the addition of calcium carbonate increased the fixation by only 0·0950 gram, the addition of caustic lime increased the fixation by 0·352 gram. It appeared, therefore, that lime exercised a very favourable influence on the phenomenon.

On the other hand, the use of organic matter, as farm-yard manure, or dried blood, much reduced the amount of fixation. The addition of calcium carbonate to the farm-yard manure increased the fixation, whilst the same addition to the dried blood reduced it.

When in the first series of experiments either phosphoric acid or potash was excluded from the mineral manure, there was a most remarkable decline in the amount of fixation, indicating, M. JOULIE thinks, how necessary is a due balance of the mineral supplies for the full development of the action.

In the second series, as in the first, the unfavourable influence of organic manures was obvious.

M. JOULIE concludes that his own results, like those of M. BERTHELOT, show that the fixation of nitrogen is due to a physiological action. Microbes play an important part; and his own experiments show that the action is developed in the absence of clay. His results were not very favourable to the supposition that the plants themselves effected the fixation; but he considers that further comparative experiments, with and without vegetation, are necessary to settle the point. For the present he limits himself to the establishment of the great fact of the fixation of the free nitrogen of the atmosphere, leaving to the future the exact explanation.

In order to show the practical importance of the fixation of free nitrogen, M. JOULIE takes for illustration the results of the experiment No. 5, in the first series, in which the largest amount of gain was indicated. In that experiment the complete mineral manure, with caustic lime in addition, was used, without any artificial supply of nitrogen. At the commencement the soil contained 1·56 gram, and at the conclusion 2·042 grams of nitrogen, and the crops contained 0·3834 gram, showing a total gain, therefore, of 0·8654 gram nitrogen. As the soil in the pots was 10 cm. deep, he calculates that this would correspond to 1144 kilograms of nitrogen fixed per hectare weighing 2000 tonnes (= 1021 lbs. nitrogen per acre). Or, reckoning only according to the relative superficies of the soil in the pots, and of a hectare, the gain of nitrogen would be 432 kilograms per hectare (= 386 lbs. per acre). He further reckons, that the value of the nitrogen gained, at 1 franc 50 per kilogram as in manures would be 1716 francs, or 650 francs per hectare (= 555 or 210 shillings per acre), according to the mode of calculation adopted. He admits, however, that it cannot be estimated so high, because the nitrogen fixed in the soil is in a form not at once assimilable by plants.

In reference to the above results, M. JOULIE says that our own at Rothamsted, and those of M. DEHÉRAIN in France, obtained in field experiments, cannot be relied upon as the basis of conclusions on this subject; because the samples of soil taken at different times cannot exactly represent the mean composition of the soil, and because the layer of soil sampled may have lost combined nitrogen by drainage, or gained it from the subsoil. M. JOULIE, on a former occasion, indicated in more detail his objection to our mode of experimenting; but he did so in a way which showed entire ignorance or misconception of our method.

We have already given our reasons for believing that certainly the losses, and probably the gains also, shown in M. DEHÉRAIN's experiments were too high. We nevertheless, quite agree that there would be losses where he found losses, and that there would be gains where he found gains. It is to be observed, however, that it was under the conditions of arable culture, that is of artificially aërated soil, and with vegetation, that M. DEHÉRAIN found great losses, whilst it is in well aërated soils, also with vegetation, that M. JOULIE finds such enormous gains.

It is further to be observed that the large gains shown in M. JOULIE's results were obtained chiefly in the growth of buckwheat, and not with plants of the Leguminous family which are reputed to be "*Nitrogen collectors.*" From our own results, taken together with known facts as to agricultural production, and the fertility of soils, it may be confidently affirmed that such gains as M. JOULIE finds within a period of about 14 months, do not take place, either with or without vegetation, in ordinary soils, in ordinary practice.

4. *The Experiments of* Dr. B. E. DIETZELL.

At the meeting of the *Naturforscher-Versammlung* at Magdeburg in 1884, Dr. DIETZELL gave the results of experiments, the primary object of which was to determine whether plants absorb combined nitrogen from the atmosphere by their leaves; but they equally afford evidence on the question whether they assimilate the free nitrogen of the air. The plants selected were peas and clover, each of which he grew under four conditions as to manuring. A garden soil, containing 0·415 per cent. of nitrogen was used, and the experimental pots were made of hard burnt clay. The plants were watered with distilled water, and the drainage was returned to the soils. The pots and their contents were exposed to free air, but protected by a linen roof.

The conditions of the different experiments were as follows:—No. 1, without manure; No. 2, manured with kainite; No. 3, with kainite and superphosphate; No. 4, with kainite, superphosphate, and calcium carbonate; No. 5, with kainite, superphosphate, and calcium carbonate, but without a plant; and No. 6, without either manure or plant.

The nitrogen was determined in the original soil, and in the seed; also in the soil at the conclusion of the experiment, and in the plants grown. The following figures show the losses or gains of nitrogen, represented in percentage of the original nitrogen in soil and seed:—

	Loss or gain of nitrogen.		
	Peas.	Clover.	Without plant.
	per cent.	per cent.	per cent.
Without manure	− 10·69	− 5·10	+ 0·26
Kainite	− 15·32	− 14·76	
Kainite and superphosphate	0·00	− 7·37	
Kainite, superphosphate, and calcium carbonate	− 12·72	− 10·38	− 10·24

Thus, there was, in every case but one with the peas, and in every case with the clover, a loss, not a gain, of nitrogen. There was also a loss where the soil was manured, but left without a plant.

On the other hand, in the experiment without either manure or plant, the figures show a gain of nitrogen. In reference to this last result, it should, however, be stated, that whilst in one German account it is, as in the Table, given as a gain of only 0·26 per cent. of the original nitrogen, in another German account, as well as in an English one, it is represented as a gain of 0·26 gram. In the first case the gain would be immaterial, whilst in the other it would be considerable, though still but small compared with the results obtained by M. Joulie.

It is to be observed that whilst with almost exclusively non-leguminous growth, M. Joulie found gains of nitrogen in all cases, and in some very large gains, Dr. Dietzell, experimenting exclusively with leguminous plants, which are credited with being beyond all others atmospheric nitrogen accumulators, in all cases found losses instead of gains. How is this discrepancy to be explained? It may be answered, that with a garden soil containing so much as 0·415 per cent. of nitrogen, it is not at all surprising that there should be some loss. Indeed loss would seem to be a perfectly natural result; and it is obvious that, neither from the combined nitrogen of the atmosphere, or that due to accidental sources, nor from free nitrogen, either directly or indirectly, did these reputed nitrogen-collectors gain nitrogen enough to compensate the losses from the rich soil. It is, indeed, recorded gains that require confirmation, with very careful methods of experimenting, before they can be accepted as conclusive evidence of the fixation of free nitrogen, and not as due merely to accidental sources of combined nitrogen, or to other experimental errors almost inevitable in experiments in which the soils and the plants are not enclosed, but exposed to the free air.

In conclusion, all the results of Dr. Dietzell, excepting the one in which he found a gain, seem quite accordant with well established facts. On the other hand, if free nitrogen is really fixed in the soil under the influence of microbes, it certainly might be supposed that the result would be developed in a soil so rich in organic matter, and doubtless, therefore, in micro-organisms also, as a garden soil containing 0·415 per cent. of nitrogen; and especially might it be supposed that it would be developed in the presence of leguminous growth, in connection with which, if at all, the establishment of the reality of such an action would serve to explain facts as yet not otherwise fully explained.

5. *The Experiments of Professor* B. Frank.

In the number of the 'Berichte der Deutschen Botanischen Gesellschaft' for August, 1886, Dr. Frank gave a paper, "Ueber die Quellen der Stickstoffnahrung der Pflanzen." At the meeting of the *Naturforscher-Versammlung*, held at Berlin, in September, 1886, he gave a further communication on the subject; and he has since published a paper on the position of the question, before, at, and after that meeting.

He admits the probability of the conclusion of Boussingault and others, that plants do not directly assimilate free nitrogen. He states, however, that in practical agriculture it is assumed that some plants do fix the free nitrogen of the air; and he refers to the experience and writings of Schultz-Lupitz, and others, on the point, especially during the last 10 years. Thus, Schultz-Lupitz found that certain Leguminosæ, especially lupins, grew well in a poor soil, under the influence of mineral manures; and so far from appearing to exhaust the soil, cereals, roots, and potatoes, grew well after them, as they would if nitrogenous manures had been applied.

Frank refers to the amount of combined nitrogen coming down in rain, &c., as about 3 kilograms per hectare (= 2·7 lbs. per acre), per annum, and to the average amount of nitrogen removed in crops as 51 kilograms per hectare (= 45·5 lbs. per acre), apparently obtained from the air by the nitrogen-gathering plants, which are considered more effective than manure and cattle feeding. He points out that the evidence is not conclusive, and he recognises that the question is, whether this is only "*Raub-bau*" after all? This can only be settled by direct experiments.

He had been working at the subject for three years, and now gives the results of the last completed experiments, those of 1885. The first question to be decided was—do the so-called nitrogen-gathering plants enrich the soil, whilst the same soil, with the same exposure, but without a plant, does not gain combined nitrogen?

He experimented with a humus-sand soil, finely sifted. In some cases he used cylinders of pottery glazed inside, 80 cm. (= 31·5 inches) deep, and 17·5 cm. (= 6·9 inches) wide; in others glass cylinders, also of 80 cm. deep, but only 11 cm. (= 4·3 inches) wide. To the rim of each cylinder a cap of wire gauze was fixed, to exclude insects; and the vessels were exposed to free air. The soils were watered with distilled water. One of the wide earthen cylinders, and two of the narrow glass cylinders, were left without a plant. In one wide earthen cylinder three lupin seeds were sown; in one narrow glass cylinder two lupin seeds, in another one lupin seed only, and in another one lupin seed and 20 incarnate clover seeds were sown. If weeds grew where there was no experimental plant, they were stocked up, but if in the vessels with the experimental plants, they were undisturbed.

The nitrogen was determined in seed, in the products of growth, and in the soils before and after growth. The seeds and plants were dried at 50° C. for analysis, and it was found that the lupin seeds contained an average of 0·009 gram. nitrogen per seed, and that 20 incarnate clover seeds contained 0·003 gram of nitrogen.

Summary of Dr. FRANK's results in the summer of 1885.

	Nitrogen.						
	Per cent. in soils.		Total in soil and seed at commencement.	In the plants.	Total in soil and plants at conclusion.	Gain or loss on the original quantity.	
	Before experiment.	After experiment.				Actual.	Per cent.
	Per cent.	Per cent.	Grams.	Grams.	Grams.	Grams.	Per cent.
1. Earthen cylinder without plant	0·0957	0·0907	20·5755	—	19·5112	−1·0643	− 5·1
2. Glass cylinder without plant	0·0957	0·0837	9·2589	—	8·0979	−1·1610	−12·5
3. Glass cylinder without plant	0·0957	0·0832	7·1775	—	6·2400	−0·9375	− 8·69
4. Earthen cylinder with 3 lupin seeds	0·0957	0·1065	20·5547	0·8208	23·6758	+3·1211	+15·2
5. Glass cylinder with 1 lupin seed	0·0957	0·0992	9·0373	0·1138	9·4781	+0·4408	+ 4·87
6. Glass cylinder with 1 lupin, and 20 incarnate clover seeds	0·0957	0·0854	8·29039	0·2295	7·6169	−0·67349	− 8·08
7. Glass cylinder with 2 lupin seeds	0·0957	0·0893	8·8032	0·0274	8·2251	−0·5781	− 6·56

It is seen that the actual amounts of nitrogen involved were large, being about 20 grams in the experiments in the wider vessels, and nearly half as much in those in the narrower vessels. The losses were in some cases about, or more than, a gram; and one of the two gains amounted to more than 3 grams.

In each of the three experiments without a plant (Nos. 1, 2, and 3), there was a loss of nitrogen. FRANK states that it was not as nitric acid, which either diminished but little, or increased, there being no plant to take it up. Nor was it as ammonia, as a direct experiment with a rich peaty soil, enclosed under a luted bell jar, and ammonia free air passed through it, showed very little ammonia evolved. He points out that the more imperfect the ventilation of the soil, the less was the gain; and he considers it probable that in the absence of ventilation the evolution of free nitrogen would be enhanced. In fact the losses were greater in experiments 2 and 3 with vessels 80 cm. (= 31·5 in.) deep, and only 11 cm. (= 4·3 in.) wide, than in No. 1 with a width of 17·5 cm. (= 6·9 in.).

It is indeed obvious that, with vessels so narrow and deep, and closed at the bottom, as according to the description we conclude they were,* and with no plant to cause evaporation, and with consequently very little aëration, the conditions would be favourable for the evolution of free nitrogen, and the more so in the narrower vessels. In fact it may be doubted whether, if there had been holes at the bottom of the

* We have since ascertained that the vessels were closed at the bottom.

vessels, and free aëration had been kept up, there would have been any loss at all from a soil containing, as this soil did, less than 0·1 per cent. of nitrogen.

It is further to be observed that the losses with growth were, in No. 6, where the one lupin plant died before blooming, and where only 7 of the 20 incarnate clover seeds grew, and in No. 7 where only one of the two lupin seeds grew, and it gave very small produce. In both cases, therefore, there would, independently of the soil itself, be decomposing organic matter, conditions under which, in the experiments of BOUSSINGAULT, and also in those at Rothamsted, there was more or less loss, supposed to be as free nitrogen.

Again, in the experiment above referred to, made to determine whether there was any material loss as ammonia, FRANK used a very unusually rich soil, containing 1·1836 per cent. of nitrogen, which after exposure for 180 days to a current of air in shallow vessels, only contained 1·0976 per cent. It had lost therefore 0·0860 per cent., corresponding to 7·27 per cent. of its total nitrogen, of which only 0·0004 was as ammonia. In reference to this loss FRANK says that if such loss is always going on in the soil, we must suppose that there is restoration in some way. But it is to be observed that the soil in which he found such loss was not only about 12 times as rich as the one used in his other experiments, but probably 8 or 10 times as rich as the majority of ordinary arable soils. Hence it is obvious that the amount of loss it sustained, cannot be taken as any indication of what happens in actual practice. Nor can the conditions of the experiments in the narrow and deep vessels without ventilation, be considered comparable with those of ordinary arable surface soils, or even of subsoils with fairly good natural, or with artificial drainage.

That soils do lose nitrogen, not only by the removal of crops, but also by drainage of nitric acid, is certain; and if there is no return of nitrogenous manures from without, the result is a gradual diminution of the fertility, so far as the nitrogen is concerned. But the balance of evidence is against the supposition that there is a constant and considerable loss by the evolution of free nitrogen, in the case of arable soils which are only moderately rich in nitrogenous organic matter, and which are fairly drained, either naturally or artificially.

On this point it may be mentioned that, in those of the field experiments at Rothamsted in which the unusual practice of applying farm-yard manure every year is adopted, it is found that there is considerable loss of nitrogen from the soil, beyond that known to be removed in the crops, and estimated to be lost in the drainage. On the other hand, where no nitrogen has been applied for many years, and the amount of nitrogen in the surface soil is only about, or little more than, 0·1 per cent., the loss of nitrogen by the soil over a long series of years corresponded approximately with the amounts removed in the crops, together with those estimated to be lost in the drainage. Again, when ammonium-salts are applied, even so late in the season as October or November, and drainage takes place soon afterwards, the drainage-waters will contain amounts of nitrogen showing a very direct relation to the different amounts of ammonia applied in the manure; but scarcely any of it as ammonia, nearly the whole existing as nitric acid; and this is the case although the drainage passes through 20 inches or more of raw clay subsoil. Lastly, direct experiments have shown that there is a diminution in the amount of nitric acid in the soil down to a certain depth, varying according to the root-range of the crop grown, and to the season, but that in the depths of the subsoil below this point, the amount is again greater.

Upon the whole, then, we are disposed to think that, in most arable soils which are only manured and cropped as in ordinary practice, and which have fair natural or artificial drainage, there is little if any loss by the evolution of free nitrogen.

We would indeed submit, that the losses found by Dr. FRANK in his series of 7 experiments, are in all probability largely, if not entirely, accounted for by the special conditions of the experiments themselves to which attention has been called, and that those found in the rich soil, in the closed vessel, depended greatly if not wholly on the abnormal character of the soil itself.

The gains (as in Experiments 4 and 5) are, however, by no means so easy to explain. Indeed, if there were no accidental source of error, such as all vegetation experiments in free air must be more or less liable to, the explanation obviously would be, that the free nitrogen of the air had come into play in some way.

Dr. FRANK supposes that, even in Experiments 6 and 7, where a loss was indicated, there had nevertheless been a gain under the influence of the plant growth, but not sufficient to counterbalance the loss. We would suggest that, in Experiments 4 and 5, where a gain was indicated, there may have been no loss at all; especially in No. 4, with the wider vessel, and where the growth of the lupins was the most luxuriant, and the seed ripened; for, under such conditions, there would be much more evaporation, and therefore much more movement within the soil, and aëration of it.

But, apparently giving full force to the evidence in his experiments of loss by the evolution of free nitrogen, and taking it as confirmation of the supposition that in actual practice soils suffer to a very material extent in this way, Dr. FRANK says that all that can be concluded with certainty is—that two opposite actions are at work in the soil—one setting free nitrogen, and the other bringing it into combination—the latter being favoured by the presence of vegetation. He admits that neither his own results, nor those of others, afford decisive evidence as to how this takes place; nor does he think that it follows from his results, that the plant itself effects the combination.

Independently of direct experimental evidence on the point, he considers it unlikely that the gain of nitrogen can be due to the ammonia of the air, because it is so small in amount, because the gain is by the soil rather than by the plant, and lastly, because, as the ammonia of the air is largely due to emanations, if it were the source we should be without explanation of the circulation of nitrogen in nature; that is, of the return of free nitrogen into combination, to compensate for the losses by its evolution from combination.

Indeed, with Dr. FRANK, as with other investigators of this subject, a prevailing idea seems to be, that there must exist a source of compensation for the loss of combined nitrogen by the removal of crops, by drainage, and above all by the evolution of free nitrogen from the soil, and in other ways. We believe, however, that the losses by the removal of crops are much exaggerated, due account not being taken of the return by the manures of the farm; also that the loss by the evolution of free nitrogen by the soil is exaggerated, the results obtained in the laboratory not being comparable with the conditions in the field. At the same time we believe, that such losses as do in reality take place in ordinary agriculture, are not fully compensated; but that arable soils yielding products for sale, and not receiving nitrogenous manures from without, do gradually reduce in fertility, so far as their nitrogen is concerned.

6. *The Experiments of Professor* HELLRIEGEL *and Dr.* WILFARTH.

At the Berlin meeting of the *Naturforscher-Versammlung,* held in September, 1886, in the Section *Landwirthschaftliches Versuchswesen,* Professor H. HELLRIEGEL gave a paper entitled, "Welche Stickstoffquellen stehen der Pflanze zu Gebote?" One of ourselves was presiding at the time, and the communication was obviously considered by the numerous agricultural chemists present to be one of great interest and importance. We have vainly tried to get the paper *in extenso*;* but we have now two accounts of the results, one by HELLRIEGEL himself, in the 'Zeitschrift des Vereins für die Rübenzucker-Industrie des Deutschen Reiches,' and another in a very comprehensive summary of the evidence relating to the sources of the nitrogen of vegetation, published by Professor KÖNIG.†

HELLRIEGEL first gave results of experiments with barley, oats, and peas, made in pure washed sand, in pots 20 cm. deep, each containing 4 kilograms of the material. Nutritive solutions containing no nitrogen were added to all. One series of pots received besides, a fixed quantity of nitrogen as nitrate of soda, a second twice as much, and a third four times as much. The results showed that in the case of the gramineous plants the amount of produce grown had a direct relation to the quantity of nitric nitrogen supplied. It was very different with the peas.

In many comparative experiments he got astonishing growth with peas in the sand with all other food substances, but without nitrogen, whilst under exactly similar conditions the Gramineæ showed nitrogen-hunger and failed. He gives the following results with peas:—

* In an interview with Professor HELLRIEGEL, at the meeting of the *Naturforscher-Versammlung,* held at Cologne in September, 1888, we learnt that the details of his experiments have not yet been published, but that a full paper is in course of preparation. [*October,* 1888.]

† 'Wie kann der Landwirt den Stickstoff-Vorrat in seiner Wirtschaft erhalten und vermehren?' Berlin, 1887.

	Total above-ground dry substance.	Seed produced.
	grams.	grams.
1884	28·483	13·947
1885	27·816	11·710
1885	33·147	12·426
1886	20·372	8·956

To convey an idea of what 33 grams of dry produce means, he states that with barley that amount was never obtained even with the addition of nitrates. So far as we know, he has not estimated the amount of nitrogen in the produce,* which, however, would be large ; and as to the source of it, he says it is obviously from the air.

The quartz sand was washed many times, the nutritive mixture given contained no compound of nitrogen, and the plants were watered with distilled water (the first third of the distillate not being used). Hence he considered the supposition that accidental impurity was the source of the nitrogen to be out of the question, especially when the amount of the produce is considered. Further, the constant failure of the Gramineæ under exactly the same conditions, afforded direct proof that the soil was not the source of the nitrogen. He concludes :—

The Papilionaceæ are distinguished from the Gramineæ in not being dependent on the soil for their nitrogenous food. The sources of nitrogen which the atmosphere affords have, for these plants, the highest importance. They alone can suffice to bring them to a normal or full development.

To determine how far the combined nitrogen in the atmosphere was the source, he made a series of 4 experiments, in each of which a pot of peas was enclosed under a glass shade, and a constant stream of air was passed through ; in No. 1, without first washing the air, but in Nos. 2, 3, and 4, the air was washed to remove all nitric acid and ammonia. The result was, that the growth was as good with the washed as with the unwashed air. There only remained, therefore, the supposition that the Papilionaceæ have the power of utilising free nitrogen. He accepts the conclusion of Boussingault that the Papilionaceæ cannot directly assimilate free nitrogen ; but the possibility of an indirect action is not thus excluded. In the first place Berthelot had shown that bacteria abound in the soil, and possess the power of bringing free nitrogen into organic combination ; and secondly the nodules found on the roots of normally growing Papilionaceæ are full of bacteria.

The author has often observed that when peas are grown in a nitrogen-free soil, the growth is quite normal, and the colour of the leaves quite healthy, until the reserve material of the seed is used up. Growth is then arrested, and the leaves become pale or yellow ; but after a shorter or longer time, they regain their green colour, a second period of growth begins, and it continues to the end. In a series of parallel experiments, however, some plants will develop full, and others only a very limited growth.

* See foot-note, p. 64.

Examination showed that the plants which did not develop beyond the first period, had either no nodules on their roots, or only weak indications of them; whilst the roots of the plants which developed favourably, had the nodules, and the more, or the older and stronger, the nodules, the better was the development of the plants. He, therefore, instituted experiments to determine whether by the supply of the organisms the formation of the root nodules and favourable growth could be induced; and on the other hand, whether by their exclusion the result could be prevented.

To each of ten out of 40 experimental pots, with nitrogen-free soil, 25 c.c. of an extract of fertile soil, made with five times its weight of distilled water, was added. After a time the plants in each of the ten pots regained their green colour, and grew vigorously; whilst in only two of the thirty pots without the addition of the micro-organisms did the plants develop favourably, all the rest showing more or less nitrogen-hunger, and some were quite yellow. After a time the plants from two of the pots with bacteria, and from five without, were taken up, and examination showed very strikingly the connection between the amount of above-ground growth, and the development of the root nodules. In the 8 remaining pots with bacteria, the growth was very uniform; whilst in only 4 of the remaining 25 without bacteria was there fair development.

Hellriegel states that the quantity of soil extract added contained only 1 millig. nitrogen. In two other experiments everything was sterilised. The peas germinated healthily, developed their first 6 leaves, but did not go further, and died, not a trace of nodules being found on their roots. He concludes:—

To the nourishment of the Papilionaceæ, especially their assimilation of nitrogen, the so-called leguminous nodules, and the micro-organisms they contain, stand in close and active relation.

It was remarkable, however, that in numerous trials with lupins, under exactly similar conditions, successful second growth could not be obtained. The conclusion was, that the organisms in the root nodules of lupins were of another kind, which were less generally distributed than those found in the nodules of peas. Further, it is known that lupins do not grow well on a heavy, or even on a rich humus soil.

Three rows of the experimental pots were filled with quartz sand, a nitrogen-free solution was added, and lupins were sown. A portion of diluvial sandy soil, where lupins were growing well, was treated with 5 times its weight of distilled water, and a turbid extract was obtained, in which it might be assumed that there would be a sufficient quantity of the micro-organisms peculiar to the sandy soil. To each experimental pot of the first row, 25 c.c. of this fluid was added; those of the second row were left without any such addition; and those of the third row received the extract from the loamy humus soil, the same as was used in the case of the experiments with the peas.

The germination and early growth were favourable in all cases; then followed the hunger-condition, and after 30 days from the sowing, all the rows showed equal poverty. Then the lupins of the first row began to show fresh green colour, assumed a healthy aspect, and grew well. The plants of both the second and third rows maintained a sickly, brown-red colour.

The roots of the plants of the first row, which received the sandy-soil extract, were thick with large root-nodules, such as are found in the field with normal vegetation. On the roots of the second series, without any soil extract, no trace of the nodules could be found; and on the roots of the third series, which received the extract from the rich soil, on only one plant was a single weak nodule observed.

Other sandy-soil Papilionaceæ, such as serradella (*Ornithopus sativa*), behaved as the lupins; whilst peas, vetches, and beans, grew best in the third row, and red clover gave no special result.

HELLRIEGEL observes that, although the Papilionaceæ have the property of turning to account atmospheric nitrogen, they nevertheless do take up nitrogen from the soil, especially as nitrates; but he considers it doubtful whether such plants can attain to a normal development with nitrates alone, and with the exclusion of micro-organisms.

Finally, he admits that his observations require control and perfecting in various directions; and he confines himself for the present, to the simple statement of his experimental results.

In reference to the foregoing results of HELLRIEGEL, we have already said that we have not been able to find any record of the experimental details; and indeed it seems doubtful whether determinations of nitrogen were made, either in soils, seed, or products of growth.* Nevertheless, such particulars as are given, can leave no doubt whatever that the products of growth, both of the peas and of the lupins, where favourable conditions were provided, would contain very much more nitrogen than the seed sown. If, therefore, the washed sand, and the nutritive solutions, were free from combined nitrogen, and the conditions of exposure of the experimental pots in free air were such as to exclude the possibility of the access of accidental sources of combined nitrogen, the obvious explanation is that the nitrogen gained had its source in the free nitrogen of the air.

Then, as to the conditions under which the free nitrogen has been brought into combination? The negative result with the Gramineæ, the negative result with the peas when everything was sterilised, or when the sand was not seeded by the soil-extract, the positive result with the peas when the sand was seeded by the humus soil extract, the negative result with the lupins when their soils were not seeded, or when they were seeded with the same extract as the peas, and the positive result when seeded with the extract from the sandy soil where lupins were growing, seem to exclude any other conclusion than that the micro-organisms supplied by the soil extracts were essential agents in the process of fixation. Further, the development of nodules on the roots was, to say the least, a coincident of the fixation. To HELLRIEGEL'S conclusions on this point, the objections have been raised,—first that the nodules are a result and not a cause of active growth, and that in fact they con-

* See foot-note, p. 64.

stitute a supply of reserve material for growth; and secondly that the investigations of TSCHIRCH and BRUNCHORST, prove that the nodules have no external communication with the soil. As to the latter objection, it may be observed, that Professor MARSHALL WARD has recently shown that, on the death of the nodules, the micro-organisms become distributed within the soil; and further, that in the case of HELLRIEGEL's experiments it was the organisms themselves, or their germs, that he supplied to the soil.

It must be admitted that HELLRIEGEL's results, taken together with those of BERTHELOT and others, do suggest the possibility that although the higher plants may not possess the power of directly fixing the free nitrogen of the air, lower organisms, which abound within the soil, may have that power, and may thus bring free nitrogen into a state of combination within the soil in which it is available to the higher plants —at any rate to members of the Papilionaceous family. At the same time it will be granted, that further confirmation is essential, before such a conclusion can be accepted as fully established.

Since the above was written, Dr. WILFARTH, who was associated with Professor HELLRIEGEL in the experiments which have been described, has given an account of a subsequent series of experiments which were made in 1887. Under the title of "Ueber Stickstoffaufnahme der Pflanzen," he gave the results at the meeting of the *Naturforscher-Versammlung*, at Wiesbaden, in September, 1887, and a short account of them was published in the 'Tageblatt,' pp. 362–63, and also in 'Versuchs-Stationen,' vol. 34, 1887, pp. 460–61, of which the following is a pretty full summary :—

He states that the previous experiments had shown, that the Gramineæ, the Chenopodiaceæ, the Polygoneæ, and the Cruciferæ, take their nitrogen from the soil, and that their growth was proportional to the available supply of nitrogen. The Papilionaceæ on the other hand take their nitrogen from the air, and grow quite normally in an absolutely nitrogen-free soil, provided a very small quantity of cultivated soil be added. He further states that they have now repeated the experiments in the same way, and that the results fully confirm the conclusions before arrived at as above stated.

The new experiments were made with oats, buckwheat, rape, peas, serradella, and lupins. The experimental soil was a pure sand, entirely free from nitrogen. Each pot contained 4 kilog. of this sand, to which were added the necessary mineral constituents. All the plants grew until the nitrogen of the seed was used up. Then to each pot a small quantity of the turbid watery extract of a surface soil was added, the quantity representing 5 c.c. of the soil, and containing from 0·3 to 0·7 millig. nitrogen. After this the different plants exhibited very great differences in growth. Neither the oats, rape, nor buckwheat showed any effect from the addition of the soil-extract, but remained in the condition of "nitrogen-hunger." On the other hand, the Papilionaceæ after a time recovered from their nitrogen-hunger, suddenly became dark green, and then grew luxuriantly up to ripeness. In experiments in which the soil-extract was sterilised by boiling, there was no such result. Peas grew well under the influence of extract from any cultivated soil, but lupins and serradella only when extract from a soil where these plants were growing was used. The series comprised 178 pots, and the results were so accordant, as was shown by photographs exhibited, that the possibility of accident, or nitrogenous impurities, was out of the question.

Thus, it may be considered established that the Papilionaceæ can take the whole of their nitrogen from the air.

The experiments of the preceding year had shown that the peas did not derive their nitrogen from the small quantity of combined nitrogen in the air, and new experiments fully confirmed this. Following the plan of BOUSSINGAULT, they put 4 kilog. of ignited sand in a large glass balloon, added mineral constituents and a small quantity of the soil-extract, and then sowed one seed of oats, one of buckwheat, and one of peas. The vessel was then perfectly closed by a well-ground glass stopper; but carbonic acid was occasionally supplied. The oats and buckwheat only grew so long as the supply of nitrogen of the seed lasted ; but the peas continued to grow luxuriantly and quite normally. A large part of the produce was found to contain 6·55 grams dry substance, and 0·137 gram nitrogen.

The author says that it cannot yet be with certainty explained in what way the soil-extract enables the Papilionaceæ to assimilate the nitrogen, and that it is even doubtful whether the root-nodules have any connection with the taking up of the nitrogen. It is, however, proved that the soil-extract favours the development of the nodules, whilst the sterilised extract has no such effect. It seems natural to attribute the action to bacteria, and to connect it with the organisms in the nodules, but the experiments do not as yet settle the question.

The amount of nitrogen in the seed is not given, but to show how considerable the assimilation of nitrogen may be, the following results, showing the amounts of dry substance, and of nitrogen, in the produce of a number of the pots of lupins, are quoted :—

	Without soil-extract.			With soil-extract.	
	Dry matter.	Nitrogen.		Dry matter.	Nitrogen.
Nos.	grams.	grams.	Nos.	grams.	grams.
9	0·918	0·0146	3	44·73	1·099
10	0·800	0·0136	4	45·62	1·156
11	0·921	0·0132	5	44·48	1·194
12	1·021	0·0133	6	42·45	1·337

Such is the brief account of the experiments as yet published by Dr. WILFARTH ; and that full confidence was placed in the results by those present may be inferred, since the report states that the communication was received with great applause ; whilst in the discussion which followed, Drs. NOBBE, HEIDEN, LIEBSCHER, FLEISCHER, and EMIL VON WOLFF, took part.

It will be seen that the results are not only confirmatory of those given by HELLRIEGEL the year before, but that they are even much more definite and striking. Thus, taking no account of the fraction of a milligram of combined nitrogen supplied in the soil-extract, the amount of dry matter produced is nearly 50 times, and the amount of nitrogen assimilated is nearly 100 times, as much with, as without, the soil-extract!

The negative result with Gramineæ, Chenopodiaceæ, Cruciferæ, and Polygoneæ, is certainly just what would be expected from all that is known of the influence of soil-supplies of nitrogen on the growth of the agricultural representatives of those families. It will be observed, however, that whilst with oats and buckwheat as representatives of the Gramineæ and the Polygoneæ, HELLRIEGEL and WILFARTH got negative results, it was chiefly with rye-grass and buckwheat that M. JOULIE obtained such great gains, though it is true under very different conditions as to soil-supplies of nitrogen, whilst some of his greater gains were largely in the soil as well as in the plants.

But whilst experience, whether practical or experimental, does not point to an unsolved problem in the matter of the sources of the nitrogen of the agricultural plants of the families above enumerated, it is far otherwise so far as the Papilionaceæ are concerned. It is true that, besides other evidence, our own results, recorded in this and former papers, show that even these plants do avail themselves of nitrogen existing as nitrates within the soil; and HELLRIEGEL also distinctly recognises that such is the case. At the same time, in reference to our own experiments we have admitted that the evidence adduced does not justify the conclusion that nitrates within the soil were an adequate source of the whole of the nitrogen that was taken up in some of the cases cited. Indeed, although the question of the sources of the nitrogen of the Leguminosæ has been the subject of experiment and of controversy for about half a century, it is generally admitted that all the evidence that has been acquired on lines of inquiry until recently followed have failed to solve the problem conclusively. It should not, therefore, excite surprise that any new light should come from a new line of inquiry. Hence should be recognised, whether as real advance in knowledge, or as only incentive to further investigation, the importance of the cumulative evidence of the last few years—of which that furnished by the experiments of HELLRIEGEL and WILFARTH is certainly the most definite and the most striking, pointing to the conclusion that although chlorophyllous plants may not directly utilise the free nitrogen of the air, some of them, at any rate, may acquire nitrogen brought into combination under the influence of lower organisms, the development of which is, apparently, in some cases a coincident of the growth of the higher plant whose nutrition they are to serve.

Such a conclusion is, however, of such fundamental, and of such far-reaching importance, that further proof must yet be demanded, before it can be accepted as beyond question. Should it be eventually fully established, it would certainly suffice to explain facts hitherto not fully explained. On the other hand, should it not be established, and a soil source of the whole of the nitrogen of the Leguminosæ be conclusively proved, the facts of agricultural production would, it seems to us, be equally well explained. To this point we shall refer again in our general concluding observations.

7. *The Experiments of Professor* EMIL VON WOLFF.

It was also at the Berlin meeting in 1886, at which one of ourselves was present, that Professor VON WOLFF distributed a page of tabulated results of vegetation experiments, made at Hohenheim, and gave some account of them. A preliminary series had been made in 1883, and more careful series were conducted in 1884, 1885, and 1886. Three sets of experiments, A., B., and C., were made, as follows :—

A. In wooden boxes, 14·5 cm. (= 5·7 inches) diameter, and 28 cm. (= 11·0 inches) deep. Into each was put 8 kilog. (= 17·6 lbs.) of calcareous coarse-grained river-sand, from which the finest part had been removed by washing. The experimental plants were—oats, sand-peas, field-beans, and red clover; and each of these was grown under the following conditions :—

1.—Without manure.

2.—With mineral manure, comprising—superphosphate, magnesium sulphate, calcium carbonate, and potassium bicarbonate.

3.—With the mineral manure, and potassium nitrate = 0·208 gram nitrogen.

4.—With the mineral manure, and potassium nitrate = 0·832 gram nitrogen.

B. In sheet zinc vessels, 25 cm. (= 9·8 inches) diameter, and 35 cm. (= 13·8 inches) deep; into each of which was put 24 kilog. (= 52·9 lbs.) of the washed river-sand. These experiments were only made in 1885 and 1886. The plants were oats, and sand-peas; and the same four conditions as to manuring, as above described, were adopted; but the quantities used were larger, the amount of nitrogen supplied in Experiment 3 being 0·416 gram, and Experiment 4—1·664 gram.

C. In cement vessels, 50 cm. (= 19·7 inches) diameter and 60 cm. (= 23·6 inches) deep. Into each was put 210 kilog. (= 463 lbs.), of the raw unwashed river-sand. The conditions of manuring were in kind the same as in A. and B.; but the quantities of nitrogen supplied were, in Experiment 3—0·832 gram, and in Experiment 4—3·328 grams. These experiments were made in 1884, 1885, and 1886; and the plants were—oats, field-beans, clover, and potatoes.

At the Berlin meeting in 1886, Professor VON WOLFF gave only the amounts of air-dried above-ground produce; but he has since published, in conjunction with Dr. C. KREUZHAGE, a long paper, giving a great deal of analytical detail.* It is there explained that, for the experiments in wooden boxes (A.), and in zinc vessels (B.), fresh washed river-sand was put in each year; but that, for those in the cement vessels (C.), the unwashed river-sand first put in was not renewed.

* "Vegetationsversuche in Sandkultur über das Verhalten verschiedener Pflanzen gegen die Zufuhr von Salpeterstickstoff." 'Landwirthschaftliche Jahrbücher,' vol. 16, Heft 4, 1887, pp. 659 *et seq.*

As to the results, WOLFF called attention to the fact that with the oats and the potatoes comparatively little increase was obtained by the use of mineral manure without nitrogen, but that where nitrogenous manure was added the increase bore a direct relation to the amount of nitric-nitrogen supplied. The behaviour of the Leguminosæ was, however, quite different. With these plants, the mineral manures as a rule gave considerable increase, whilst the addition of the nitrate generally gave little or no further increase. He remarks that these results are consistent with those obtained in ordinary agriculture; and that it is a question whether the so-called "nitrogen collectors" obtain all their nitrogen by means of their widely and deeply penetrating roots, or whether they draw some of it from the air; and if so, whether they can only take it as combined nitrogen, or also as free nitrogen? He considers that the results of BOUSSINGAULT and ourselves are against the supposition that they assimilate free nitrogen. At the same time he thinks the results of HELLRIEGEL show that the Papilionaceæ are not dependent on soil sources of nitrogen alone; though further evidence is required to determine whether or not the free nitrogen of the air comes into play.

As to the connection of the root-nodules with the development of the plants, WOLFF considers that they may be equally well supposed to be a consequence as a cause of active growth. Observations by SCHULTZ-LUPITZ, and at Hohenheim, have shown that the nodules may be very little developed in a soil rich in nitrogen. He refers to the results of FRANK and BRUNCHORST, as indicating, that the contents of the nodules do not consist either of bacteria or of fungoid forms,* but rather of nitrogenous matters which are re-absorbed by the plant when forming fruit.

As bearing on the subject, he quotes the following results of TROSCHKE at Regenwalde, showing the comparative composition of the nodules and of the roots of blue lupins, at the time of pod formation:—

	Pure ash.	Crude fat.	Crude fibre.	Crude protein.	Non-nitrogenous extract.	Nitrogen.	Of total nitrogen albuminoid.
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
Nodules . . .	7·51	5·33	9·43	45·31	32·42	7·25	69·7
Roots	4·07	1·31	52·25	7·06	34·61	1·13	73·7

At Hohenheim, the nodules of yellow lupins were found to be less nitrogenous at the conclusion of pod-formation.

WOLFF considers that numerous results show that atmospheric sources of nitrogen do come into play in the growth of the Papilionaceæ. He quotes from BOUSSINGAULT in reference to experiments with clover and peas grown in a nitrogen-free soil that

* It may be observed that the recent results of Professor MARSHALL WARD ('Phil. Trans.,' B, vol. 178, 1887, pp. 539–562) are at variance with the views of FRANK, BRUNCHORST, and TSCHIRCH on this point.

the plants had gained nitrogen. It is to be observed, however, that the results in question were those of BOUSSINGAULT's earliest experiments, made in 1837 and 1838, whilst his later results with Leguminosæ obtained under similar conditions, either show slight loss or scarcely appreciable gain. WOLFF also quotes experiments of his own with clover, made in 1853, in one case in good soil, and in another in the same soil previously ignited, when the dry substance of 4 cuttings was, from the natural soil 3·396 grams, but from the ignited soil 20·314 grams; these quantities being exclusive of stubble and roots.

Referring to his more recent experiments, considerable detail is given as to the preliminary series made in 1883, as well as to the more complete series conducted in 1884, 1885, and 1886, the general conditions of which have been described above, and we now confine attention to some further account of the results obtained in the later years.

WOLFF states that the washing of the river-sand removed 1·46 per cent. of it, which consisted of sandy clayey matter. He gives a complete mineral analysis of both the crude sand, and the separated fine matter. He states that the washed sand contained only traces of nitrogen. The separated fine matter contained, however, according to a direct determination, 0·304 per cent. of nitrogen, which seemed to exist in humus compounds. It may be observed, that it was, therefore, about twice as rich in nitrogen as most ordinary arable surface soils. The results obtained in the first year 1884, in the cement vessels, with the unwashed river-sand, show the influence of this supply; but it was concluded that in 1885, and 1886, but little remained in a condition available to the plants.

All the experiments were equally exposed to the influences of air and light, excepting that the wooden boxes were placed on a low truck, which was pushed under shelter when there was violent or continued rain. All the plants were watered with distilled water when there was not sufficient rain; and in all cases the drainage was collected and used for re-watering. The sowing of the seeds was always at the end of April; the gathering of the oats, beans, and lupins, at the end of July; and the last cuttings of the clover in August and September.

To the amounts of above-ground produce (air dried), WOLFF now adds the quantity of roots, and the total weight including them. He also gives, for the produce of 1886, the amounts of dry matter, and both the percentages of nitrogen in the dry matter, and the actual quantities of nitrogen in the crops, in the case of the "C" series of experiments, that is, those made in the cement vessels, with the unwashed river-sand. In the case of the sand-peas, however, the results of the "B" series, that is, those made in zinc vessels, with the washed river-sand, are given. The following is a summary of the results, so far as the nitrogen is concerned:—

Total Nitrogen in the Crops of 1886.

	"C" series, unwashed sand.				"B" series, washed sand.
	Oats.	Lupins.	Field-beans.	Red clover.	Sand-peas.
	grams.	grams.	grams.	grams.	grams.
1. Unmanured	0·371	2·797	2·478	3·396	0·488
2. Mineral manure	0·388	9·572	4·689	8·352	2·389
3. Mineral manure, and 0·832 gram N .	1·136	5·392	3·232	8·803	3·698
4. Mineral manure, and 3·328 grams N	3·486	8·816	4·104	9·403	5·069

It should be observed that the quantities of nitrogen in manure, as stated in the table, relate to the "C" Series in unwashed sand, but the quantities supplied to the smaller vessels of the "B" Series for the sand-peas were for Experiment 3, 0·416 gram, and for Experiment 4, 1·664 gram. On the results, WOLFF remarks that without exception the cereal crop (oats) only flourished when there was a sufficient quantity of nitrogenous nutriment (nitric-nitrogen) available; whilst the Leguminosæ for the most part grew quite as luxuriantly without, or with very little, nitrogen in the soil, provided the ash constituents were in abundance. Potatoes, on the other hand, like the oats, required combined nitrogen to be provided within the soil.

The mineral composition of the crops, and the influence of the varying climatal conditions of the three seasons, are discussed in some detail. Estimates are given of the amounts of phosphoric acid, potash, &c., in the crops, calculated per hectare; and the quantity of nitrogen in the oat crop is estimated to correspond, in one case, to 139·4 kilog. per hectare (= 124·5 lbs. per acre), which is very large.

Excluding the experiments in the unwashed sand, the mean of 7 experiments under each condition of manuring with oats, and of 22 under each condition with different Leguminosæ, shows the following amounts of air-dried produce:—

	Oats. Mean of 7 experiments in each case.	Leguminosæ. Mean of 22 experiments in each case.
	grams.	grams.
1. Unmanured	16·87	22·71
2. Mineral manure	18·31	68·53
3. Mineral manure, and 0·238 gram nitrogen for the oats, and 0·180 gram for the Leguminosæ	47·42	73·39
4. Mineral manure, and 0·952 gram nitrogen for the oats, and 0·72 gram for the Leguminosæ	110·74	74·95

The little influence of the mineral, and the great influence of the nitrogenous manure, on the oats, and again the marked influence of the mineral, and the little effect of the nitrogenous manure, on the Leguminosæ, are here very strikingly illustrated.

Referring to the amounts of nitrogen in the crops of sand-peas grown in the washed sand, as given in the table at the top of page 74, WOLFF states that the 19 peas sown contained 1·542 gram dry substance, and he estimates the amount of nitrogen in the seed sown at only 0·0647 gram; the quantity of nitrogen so provided was, therefore, quite immaterial in proportion to that in the crops.

The amounts of nitrogen in the clover crops of Experiments 2 and 4 are estimated to correspond to 334 and 376 kilog. per hectare, to which one-third should be added for stubble and roots. The beans appear not to have collected so large a quantity of nitrogen in 1886 as the clover and the lupins; and it may be mentioned that whilst the 54 bean seeds sown would contain 1·0353 gram of nitrogen, the same number of lupin seeds would contain only 0·5912 gram.

Some experiments by STRECKER with yellow lupins are quoted as showing considerable gains of nitrogen.

With regard to the numerous results recorded in his paper, WOLFF admits that they are not satisfactory in all respects, those of different years not being always accordant. But the special object was to compare the growth of cereals with that of Papilionaceæ, over a series of years, in a soil poor in combined nitrogen, or containing known amounts of it; and the influence of the various seasons was different on the different crops.

The following table gives estimates of the amounts of nitrogen in the crops, more (+) or less (−) than supplied in the seed and manure, calculated per hectare; the selection of experiments being the same as in the case of the results given in the table at the top of p. 74 :—

Estimated Nitrogen, in kilograms per hectare, in crops more (+) or less (−) than in seed and manure.

	"C" series, unwashed sand.				"B" series washed sand
	Oats.	Lupins.	Field-beans.	Red clover.	Sand-peas.
	Kilog.	Kilog.	Kilog.	Kilog.	Kilog.
1. Unmanured	− 9·0	+ 88·3	+ 57·7	+ 135·8	+ 84·7
2. Mineral manure	− 8·3	+ 359·3	+ 146·2	+ 334·1	+ 464·9
3. Mineral manure, and nitrate	− 11·7	+ 158·8	+ 54·6	+ 318·8	+ 643·5
4. Mineral manure, and nitrate	− 17·6	+ 195·8	− 10·4*	+ 242·9	+ 668·1

* Given in error as + 89·5 by WOLFF, the nitrogen of the manure of Experiment 3 instead of that of Experiment 4 having been deducted.

It is added that these amounts do not include the nitrogen in the stubble and roots, which in the case of the oats would be very little, in that of the beans and lupins not much, but in the sand-peas considerable, as also in the clover. It is to be observed, moreover, that no account of the nitrogen in the unwashed sand is here taken. But WOLFF points out that the gain was the largest in the case of the peas grown in the washed sand, which showed only a trace of combined nitrogen. Again the cement cases and their contents were exposed to the weather from year to year whilst there was no crop; but WOLFF points out, as is doubtless true, that the amount of combined nitrogen brought down in the rain and dew would be quite immaterial.

Admitting it to be established that plants do not assimilate the free nitrogen of the air, he thinks the only remaining hypothesis is that certain plants are enabled to appropriate the combined nitrogen of the air, either directly through their leaves, or by absorption in the soil; and the latter he considers by far the most probable. In reference to this point he refers to the results of experiments made to determine the amount of ammonia absorbed from the air by dilute acids exposed in shallow vessels. In this way A. MÜLLER estimated that 12 kilog. of ammonia were absorbed per hectare (= 10·7 lbs. per acre), per annum, in Sweden; whilst O. KELLNER's estimate in Japan, was 14 kilog. per hectare (= 12·5 lbs. per acre). O. KELLNER also determined the amounts of nitric and nitrous acids absorbed by solutions of potassium carbonate. The quantities of nitrogen so absorbed corresponded to 11·78 kilog. per hectare as ammonia, and to 1·30 kilog. as nitric and nitrous acids; giving a total of 13·08 kilog. of nitrogen per hectare (= 11·68 lbs. per acre), per annum. Other experimenters have, however, found much more. Thus, reckoning according to SCHLŒSING's experiments, in one of which a quantity of soil gained at the rate of 2·59 kilog. ammonia per hectare in 14 days, and in another 4·097 kilog. in 28 days, the amounts absorbed would be in the one case 68, and in the other 53 kilog. per hectare (= 60·7 and 47·3 lbs. per acre) per annum. WOLFF points out that even these amounts are small compared with the quantities of nitrogen assimilated in the experimental crops. He further remarks that his porous sand would probably present more than a hundred times the absorbing surface of an acid or alkaline solution of the same area.

Besides ammonia absorption, he thinks there is probably another way in which a humus-free soil may become a source of nitrogen to plants; viz., by the combination of free nitrogen, under the influence of calcium carbonate. He quotes B. FRANK ('Landw. Presse,' March 2, 1887), as having shown that a marl rich in lime, with which SCHULTZ-LUPITZ marled his soil, constantly yielded nitric acid after being boiled out with water, by which it is supposed it would be sterilised; and this was the case when the mass was exposed to ammonia-free air. According to the results obtained, a kilogram of the mass would acquire nearly a gram of nitric acid per annum. Pure calcium carbonate acted in a similar manner. After being washed out with hot water on a large filter, and kept moist, but protected from dust, nitrification took place. FRANK considers the calcium carbonate of the soil as a nitrogen combiner; and that, in presence of water and atmospheric air, nitrite and nitrate of lime are gradually formed, the nitrogen and oxygen of the air uniting in contact with the porous body, and the acid uniting with the lime and expelling carbonic acid.*

WOLFF further quotes the observations of CLOËZ, made more than 30 years ago, in which he passed air, first through a solution of potassium carbonate, then through sulphuric acid and over pumice moistened with sulphuric acid, for six months over various porous substances. He found a formation of nitric acid when the air was passed over pieces of brick or pumice moistened with potassium carbonate; and also traces with chalk, chalk marl, and a mixture of kaolin and calcium carbonate. On the other hand he found no formation of nitric acid by burnt bones moistened with potassium carbonate, or by clay.

WOLFF considers that the conditions of his experiments involved those found by CLOËZ to favour such formation of nitric acid. He admits, however, that it is difficult to explain why the action should take place when the Leguminosæ are present, and that the growth of the cereals is not benefited thereby. He suggests whether the greater pumping action of the leaves of the Leguminosæ causes a more active aëration of the soil, and so it may be that with their increased development the greater is the amount of nitrogenons nutriment accumulated from the atmosphere by the moist soil, whilst it is well known that these plants leave an efficient nitrogenous residue for succeeding crops.

In conclusion, WOLFF admits that the amounts of absorption indicated in the experiments with particular plants cannot be expected on a large scale. In practice, soils are not kept so porous, and so constantly moist; nor are the mineral conditions of the soil always so favourable. Indeed the variations of result in the different experiments illustrate the influence of varying conditions.

Perhaps the most striking of WOLFF's results were those obtained in the experiments made in 1853, in which clover yielded about six times as much dry produce grown in an ignited rich meadow soil, as in the same soil in its natural state. The ignited soil would not only be nitrogen free, but sterilised; so that, unless it acquired and developed micro-organisms during the growth, the supposition of the intervention of such agents in bringing free nitrogen into combination within the soil would be excluded. In reference to this point it may be remarked, that in the case of HELLRIEGEL'S experiments, in which he added the watery extract from various soils to his quartz sand, he states that red clover showed no special result.

Next as to WOLFF's more recent results, in which river-sand was used as soil, in some cases unwashed, but in others washed free from the fine matter which contained nitrogen. It is, as he says, quite consistent with experience in agriculture, that oats and potatoes should yield little increase by mineral manure without nitrogen, but give increase much in proportion to the nitrogen supplied to the soil; and that, on the

* The above results of FRANK have since been called in question by H. PLATH ('Jahrbücher,' vol. 16, 1887, p. 891, and vol. 17, 1888, p. 725); also by Professor LANDOLT, 'Landw. Presse,' Jahrgang 15, no. 30.

other hand, the Papilionaceæ should give marked increase with mineral manure, and but little with nitrogenous manure. Such is certainly the result in ordinary soils containing nitrogen; but if we are to assume that in his experiments the sand was not the source of the nitrogen, both the amounts of dry produce, and those of nitrogen, in the different crops, as shown in the tables which have been given, are such as seem to exclude any other explanation than that the air had contributed nitrogen in some way.

At the same time, the conditions of experiments conducted in a not absolutely nitrogen-free soil, and with free exposure to the weather, and so subject to accidental sources of more or less combined nitrogen which such conditions necessarily imply, however appropriate for obtaining initiative results and general indications, seem scarcely suitable for the settlement of so delicate a question as that of the source of the nitrogen of vegetation. On this point it may be remarked that, according to the data given, the unwashed sand put into each cement vessel would contain about 9 grams of combined nitrogen, whilst, as shown in the table at page 74, the largest crops of the lupins and red clover, with mineral but without nitrogenous manure, only contained about that amount of nitrogen, and the beans only about half as much. It is true that this was the result in the third year, 1886, after somewhat similar amounts had already been grown for two years.

Again, so far as the sand did contain nitrogen, the great difference of result with the Gramineæ and the Leguminosæ, under the influence of mineral manure without nitrogen, is not absolutely conclusive evidence that the Leguminosæ had acquired nitrogen from some other source than the soil; for there can be little doubt that Leguminosæ do utilise nitrogen existing in the soil in a condition in which it is not available to the Gramineæ. The results obtained in the washed sand, must, however, be admitted to have much greater significance.

As to the explanation of the results, WOLFF is disposed to attribute the gains of nitrogen to the absorption of combined nitrogen from the air by the soils, and to the fixation of free nitrogen within the soil under the influence of porous and alkaline bodies, as supposed by CLOËZ and FRANK, rather than to the fixation of free nitrogen either under the influence of micro-organisms, or directly by the plants themselves. In fact, neither were the conditions of his experiment with the clover in burnt soil, nor those of his later experiments in washed sand, such as to favour the supposition of the intervention of micro-organisms. On the other hand, he considers they were favourable for the absorption of combined nitrogen from the air, and for the supposed fixation of free nitrogen within the soil under the influence of porous and alkaline bodies. At the same time, he admits that it is not easy to explain why Gramineæ do not, equally with the Leguminosæ, benefit by such absorption, and by such fixation.

For our part we believe that a careful consideration of all that is involved in this undoubted fact, points to the exclusion of the supposition that the gain is either by the absorption of ammonia from the air, or by the fixation of free nitrogen within the soil under the influence of porous and alkaline bodies. We shall refer to the point again in our concluding remarks, but we may here say in passing, that the results of our experiments on the growth of wheat for many years in succession on the same land without nitrogenous manure, show that with much less nitrogen annually removed in the crops, and estimated to be lost by drainage, than would be required for the growth of the Leguminosæ in WOLFF's experiments, the soil has nevertheless lost much nitrogen. Again, taking the average of ten years, the amount of nitrogen as nitric acid which has passed through 60 inches depth of soil and subsoil in the Rothamsted drain-gauges, exposed to the air and rain, with aëration from below also, but without vegetation, has been somewhat less than 40 lbs. per acre (= 44·8 kilog. per hectare) annually; and it cannot be doubted that at least the greater part of this has been derived from the organic nitrogen of the soil and subsoil. It is obvious, therefore, that the amount due to absorption and to such fixation together, must be much less than this.

If, therefore, neither nitrogenous impurity in the sand, nor accidental sources of combined nitrogen, can be supposed to account for the gains by the Leguminosæ in WOLFF's experiments, it would seem that the explanation must be sought, either in the agency of micro-organisms, or in direct assimilation by the plants themselves.

8. *The Experiments of Professor* W. O. ATWATER.

Professor ATWATER, of the Wesleyan University, Middletown, Conn., U.S.A., has published three papers in the 'American Chemical Journal':—1. "On the Acquisition of Atmospheric Nitrogen by Plants" (vol. 6, No. 6); 2 (with E. W. ROCKWOOD). "On the Loss of Nitrogen by Plants during Germination and Growth" (vol. 8, No. 5); 3. "On the Liberation of Nitrogen from its Compounds and the Acquisition of Atmospheric Nitrogen by Plants" (vol. 8, No. 5). In these papers he gives the results of experiments of his own, and discusses the results of others also, on various aspects of the question. In what years his experiments were made is not stated, but we assume in 1883 and 1884, as they were undertaken after a visit he paid to Europe in 1882, and in the autumn of 1884 he gave a paper on the subject, at the meeting of the British Association at Montreal, and also at the meeting of the American Association at Philadelphia.

It is stated that the question was :—"*May plants, grown under normal conditions, acquire any considerable amount of nitrogen, free or combined, from the ambient atmosphere?*" It is further stated, that after a series of trials had shown a not inconsiderable acquisition of atmospheric nitrogen, a second series was planned to verify the results of the first, and to include a collateral inquiry, namely :—

"*How is the acquisition of nitrogen from the atmosphere affected by abnormal conditions of growth, and what bearing may the results obtained have upon the interpretation of those obtained by other experimenters, and upon the general question of the assimilation of atmospheric nitrogen by plants?*"

Professor ATWATER first made two series of experiments in which he grew peas in sand, to which he supplied nutritive solutions, containing mineral matters, and known quantities of nitrogen as nitrate. The plants were grown in free air, but protected from rain and dew.

The following are the actual amounts of nitrogen supplied in seed and nutritive solution, the actual gains, and the gains per cent. on the total amounts supplied, in the first series of 3 experiments :—

Supplied . .	millig. .	103·7	120·6	95·7
Gains . . .	millig. .	+ 63·5	+ 13·2	+ 13·0
Gains . . .	per cent.	+ 61·2	+ 10·9	+ 13·6

The second series included 12 experiments, in six of which generally less, and in the other six generally much more, nitrogen was supplied in the solution than in the seed.

With the smaller quantities of total nitrogen the results were :—

Supplied .	millig. .	94·7	128·2	93·6	130·9	93·3	129·2
Gains . .	millig. .	+ 21·7	+ 30·7	+ 62·5	+ 27·2	+ 93·2	+ 81·7
Gains . .	per cent.	+ 22·9	+ 23·9	+ 66·8	+ 20·8	+ 99·9	+ 63·2

With the larger quantities of nitrogen the results were :—

Supplied . .	millig. .	169·3	199·3	170·5	194·5	135·8	161·0
Gains (or loss)	millig. .	+ 9·6	+ 1·3	− 20·9	+ 3·0	+ 142·0	+ 99·2
Gains (or loss)	per cent.	+ 5·6	+ 5·7	− 12·3	+ 1·5	+ 104·5	+ 62·0

With regard to the variations of result, it is concluded that where the gains were less, or there was loss, the conditions were abnormal, and where the conditions were normal, the gains were the higher.

Some of the results are calculated per hectare and per acre, showing very large gains compared with the amounts of nitrogen in ordinary crops in the field.

Numerous experiments were also made to determine the loss of nitrogen in the germination of peas ; five by the water-culture method, and eight in sand.

Three of the water-culture experiments were conducted in open air, when the results were :—

Nitrogen in seed .	millig. .	215·6	67·5	41·4
Nitrogen lost . .	millig. .	− 18·1	− 6·6	− 0·6
Loss	per cent.	− 8·4	− 9·8	− 1·5

In two water-culture experiments in the air of the laboratory, there was gain not loss of nitrogen, which was attributed to ammonia in the air. The results were :—

In seed . .	millig. .	290·2	119·7
Gains . . .	millig. .	+ 19·2	+ 10·2
Gains . . .	per cent.	+ 6·6	+ 8·5

Of the 8 germination experiments in sand, 3 were conducted in the open air. The results were :—

In seed . .	millig. .	100·3	102·9	109·7
Loss . . .	millig. .	— 6·0	— 10·9	— 16·6
Loss . . .	per cent.	— 5·9	— 10·6	— 15·1

Three experiments in a greenhouse showed :—

In seed . .	millig. .	115·6	103·4	101·4
Loss . . .	millig. .	— 8·4	— 12·7	— 16·5
Loss . . .	per cent.	— 7·3	— 12·4	— 16·3

Lastly, two experiments in a room in a dwelling-house, showed :—

In seed . .	millig. .	88·3	88·2
Loss . . .	millig. .	— 7·2	— 9·7
Loss . . .	per cent.	— 8·2	— 11·0

Atwater discusses the results of other experimenters, including those of Boussingault, Schlœsing, and ourselves and the late Dr. Pugh, on the evolution of free nitrogen under various conditions. On the whole he concludes, that germination without the liberation of nitrogen is the normal process; that losses, whether during germination, or in later periods of growth, are due to forms of decay; and that they would thus be not essential to germination and growth, but accessory phenomena. He, nevertheless, gives a table showing, for 8 of his experiments, the actual gains found, the greater gains supposing there had been a loss of 15 per cent. of nitrogen—as in some of his own germination experiments, and the still greater gains supposing a loss of 45 per cent., as shown in experiments of Boussingault in the germination of beans and maize, under special conditions, in contact with nitrate. He admits, however, that there is no evidence to show what was the loss in his own vegetation experiments.

We may here remark, that, in full recognition of the loss of nitrogen under conditions of decay, it was concluded, both by Boussingault and at Rothamsted, that excepting in some cases in which there was obviously some decay, the results were not vitiated by any such loss, and in those cases the losses found were so explained.

Atwater quotes Boussingault's earliest experiments, made in 1837 and 1838, to show that with clover and peas nitrogen must have been gained from the air. It should be remarked, however, that Boussingault himself, after he had acquired much more experience, both in the conduct of vegetation experiments, and in analytical method, entirely disallowed that the results of those early experiments were evidence of the fixation of free nitrogen. He also quotes Boussingault's later experiments; made in 1851, 1853, and 1854, and says in regard to them that—"even the cereals, oats and wheat, contain 17 to 32 per cent. more than was supplied in the seed. The results differ from those of the previous series in that the cereals here show more, and the legumes less, gain of nitrogen." Now, in the 6 cases with haricots and lupins referred to, the amounts of nitrogen supplied in the seed ranged from 19·9 to 36·7 millig., and they show, respectively—2·3 millig. actual loss, and 3·2, 2·5, 4·2, 3·0 and 2·0 millig. actual gain. In the case of the cereals, on the other hand, the total amount of nitrogen supplied in the seed was, with the oats only 3·1, and with the wheat only 6·4 millig., and the actual gains shown were only 1·0 and 1·1 millig. Yet, Atwater shows that these last results, calculated into *percentage*, represent gains of 32 and 17 per cent. of the original nitrogen, and larger gains even than with the legumes! It need hardly be said, that Boussingault interpreted these later results as not indicating any fixation of nitrogen.

Briefly summarised, Atwater's conclusions are:—

1.—That in some of his experiments with peas, half or more of the total nitrogen of the plants was acquired from the air. Where the gains were small, or there was a loss, the conditions were abnormal, and it is to be assumed that there was loss, either from the nitrate of the nutritive solutions, from the seeds during germination, or from the growing plants.

2.—An actually observed gain is positive proof that nitrogen has been assimilated, either directly by the plants, or indirectly through the medium in which the roots have developed. The failure of an experiment to show gain only proves non-assimilation, if it is also proved that there was no liberation of nitrogen. The conflicting results of various experimenters may probably be explained by the fact of such liberation.

3.—The experiments do not show in what way the nitrogen is acquired. It must have been taken up, either as free or combined nitrogen, either directly through the foliage, or indirectly through the soil and nutritive solutions, and the roots.

4.—It is possible that the negative results of Boussingault, and ourselves, are due to the liberation of free nitrogen. The conditions were, moreover, such as to exclude the action of electricity and of microbes.

5.—Since Berthelot has shown that nitrogen may be fixed in organic matter by the agency of electricity, and in soils by the agency of micro-organisms, some cases of gain may be so explained; but it is considered that the conditions for such actions did not exist in his own experiments. The balance of evidence favours the assumption that the plants themselves were the agents.

6.—The conclusion that plants acquire atmospheric nitrogen accords with, and explains, facts of vegetable production otherwise unexplained; and the fact of its acquisition in considerable quantities seems well established.

We have pointed out that in BERTHELOT'S later papers he seems to rely much more on the agency of micro-organisms, than on that of electricity, in explanation of the phenomena of the fixation of free nitrogen; whilst ATWATER does not consider that the conditions of his own experiments are favourable to the supposition that either of these agencies was the cause of the fixation which his results show.

It need only be added, that the assumption that the real gains are generally greater than the experimental results indicate, on account of the losses that have taken place, is a very old one, it having been brought against the negative results obtained by BOUSSINGAULT, and at Rothamsted, thirty or more years ago. It is still, however, as has been seen, a favourite argument with others as well as Professor ATWATER. We have already said, that neither the conclusions of BOUSSINGAULT, nor those drawn from the Rothamsted experiments, were vitiated by virtue of such loss. Further, the supposition that the assimilation of free nitrogen is the greater when luxuriance is favoured to a certain degree by artificial supplies of combined nitrogen, owes its origin to about the same date. The result was, however, as distinctly negative in the experiments at Rothamsted when luxuriance was so favoured, as when it was not. It is freely admitted, however, that in many of the experiments of BOUSSINGAULT, as in those at Rothamsted, the arrangements were such as to exclude the agency, either of electricity or of micro-organisms. To this point we shall refer again presently.

9. *Recent results and conclusions of* M. BOUSSINGAULT.

We have frequently discussed the results of M. BOUSSINGAULT, obtained from 1837 to 1854, and expressed entire agreement with the conclusions he drew from them under the conditions provided; and it is not the object of the present comments to reconsider them in any detail. The question of the fixation of free nitrogen has, however, assumed a new aspect in recent years. It is now supposed that fixation, either by the plant, or within the soil, takes place, if at all, by the agency of electricity, or of micro-organisms, or of both; and there can be no doubt, that the earlier vegetation experiments of BOUSSINGAULT above referred to, as well as those conducted at Rothamsted about thirty years ago, were so arranged as to exclude the influence of either of those agencies. If, therefore, it should be established, that fixation does take place under their influence, and that such influence is essential for the development of the action, the conclusions, both of BOUSSINGAULT and ourselves, from the results in question, are so far vitiated. It is to some of BOUSSINGAULT'S more recent results, which have a bearing on this new aspect of the question, that we propose now to refer.

In BOUSSINGAULT'S previous vegetation experiments he had used sterilised materials as soils. But, in 1858,* he commenced a series in which he employed more or less of a

* 'Comptes Rendus,' vol. 48, 1859, p. 303. 'Agronomie, &c.,' 2e édit., vol. 1, 1860, p. 283.

rich garden surface soil, mixed with more or less sand, or quartz, or both. The plants grown were lupins, hemp, and haricots; in some cases in free, and in others in confined air. In the latter cases, the materials were put into a large glass balloon, or carboy, moistened with pure distilled water, the seed sown, and then the whole perfectly closed from the outer air by means of caoutchouc, arrangement being made, however, for the supply of carbonic acid.

In the experiment with lupins in confined air, the largest amount of the rich soil was used, and the result was so striking, that BOUSSINGAULT repeated it the next year, 1859,* when he obtained an almost identical result. In no other case was there anything like the same amount of gain of nitrogen, and we must only refer in any detail to the conditions and the results of these two experiments. They were as follows:—

Date.	Plant.	Air.	Soil, &c.				Nitrogen.						
			Soil.	Sand.	Quartz.	Ash.	At commencement.			At conclusion.			
							In soil.	In seed.	Total.	In soil.	In plant.	Total.	Gain.
			grams.	grams.	grams.	grams.	grams.	grams.	grams.	grams.	grams.	grams.	grams.
1858	Lupin	Confined	130	1000	500	0·2	·3393	·0204	·3597	·4065	·0246	·4311	+ ·0714
1859	Lupin	Confined	130	720	150	0·1	·3380	·0200	·3580	·3834	·0417	·4251	+ ·0671

It should be stated that, taking the mean of 7 determinations by the soda-lime method, the rich garden soil used contained 0·261 per cent. of nitrogen. This BOUSSINGAULT calculated would correspond to 11,310 kilog. nitrogen per hectare (= 10,098 lbs. per acre), one-third of a metre deep. He also determined the amounts of ammonia, nitric acid, and carbon, in the soil; and he concluded that the nitrogen, beyond the small amount existing as ammonia or nitric acid, was in combination as organic matter. In fact it existed in organic detritus, and especially in a black substance which he observed by the microscope.

Referring to the figures, it is seen that in the experiment in 1858 there was a gain of 0·0714 gram nitrogen, upon a total of 0·3597 supplied in soil and seed. Further, calculation shows that, of the total gain 0·0672 gram was in the soil, and only 0·0042 gram in the plant, notwithstanding that the original soil contained 0·3393 gram nitrogen. BOUSSINGAULT remarks that the fertilising matters in the soil had thus scarcely taken any part in the growth; the conclusion being that it was only the nitrogen that existed as, or was transformed into, ammonia or nitric acid, that was available. He further remarks, that it was impossible, that anything like the amount of excess of nitrogen in the soil could be due to the débris of the vegetable matter of the lupin, roots, &c. He adds:—*It is the soil and not the plant which has fixed the nitrogen; and one such result can only be admitted if confirmed by future experiments.*

* 'Agronomie, &c.,' p. 329.

In the same year, 1858, another experiment was made with lupins, but in free air, one with hemp in free air, two with haricots, one in confined and one in free air, and one with 120 grams of the rich soil placed in a shallow vessel, kept moist with distilled water, and exposed to the free air as an experiment on fallow. In this last experiment, whilst there was a loss of about one-third of the carbon, the nitrogen increased from 0·31320 gram to 0·32184 gram, showing a gain therefore of 0·00864 gram.

Boussingault remarks that whilst the soil has lost a considerable quantity of its carbon by slow combustion, the nitrogen instead of diminishing has even increased, and that it remains to decide whether there has been nitrification, production, or simple absorption of ammonia.

In introducing the report of the second series of experiments, those made in 1859, Boussingault says that he could not accept the gain of nitrogen by vegetable mould as sufficiently established, without repeating the experiments. It further remained to examine whether, in case there really were a fixation of nitrogen, it was as nitric acid, as ammonia, or as organic compounds.

In reference to the result of the experiment made in 1859, as given in the table, Boussingault says that during the growth of a lupin in a confined atmosphere, in 130 grams of very rich soil, mixed with sand to favour the access of air, the plant, during 97 days, assimilated 0·0217 gram nitrogen from the soil, and yet the soil gained 0·0454 gram nitrogen, only one-ninth of which pre-existed as nitric acid or ammonia. The total gain of nitrogen in plant and soil was 0·0671 gram, a result which is almost identical with that found in 1858. He adds, that there is this curious coincidence, that in both cases it is by the soil, and not by the plant, that the gain has been effected.

In the case of none of the other vegetation experiments in 1859 are the gains or losses by the soils given, so that the total gain or loss cannot be estimated. Boussingault points out, however, that in 1859, there was about twice as much nitrogen taken up by a haricot growing in 100 grams of the soil, as by one growing in only 50 grams in 1858. It may be added that haricots took up much more nitrogen in proportion to a given amount of soil than lupins.

Referring to the main results, Boussingault says the singular fact appears, that the soil not only gained ammonia and nitrates, but organic matter also, possibly the remains of living organisms. On careful examination, he has observed that vegetable earth contains, not only dead organised matter, but living organisms, germs, the vitality of which is suspended by drying, and re-established under favourable conditions as to moisture and temperature. This mycodermic vegetation is not always visible to the naked eye, and its progress must be followed by the aid of the microscope. The mycoderms have only an ephemeral existence, and they leave their detritus in the soil, which in time may give rise to ammonia and nitric acid. Even if the nitrogen of the air takes part in nitrification, a part of the nitrogen will exist in mycoderms, or their remains.

However this may be, considering only the numerical results which have been obtained, he is forced to believe that the soil of Liebfrauenberg has fixed nitrogen; nitric acid and ammonia being at the same time developed. The experiment on fallow in confined air seems to indicate that the vegetation has but little to do with the result.

Having given the details of his experiments, he submits them to the criticism of others, thus enabling them to judge whether the intervention of the nitrogen of the air in the production of nitrates is really established. In his opinion, if there is not absolute proof, there is certainly strong presumption, in favour of the reality of the phenomenon.

These very remarkable results seem to have instigated new experiments to test the validity of the obvious conclusion from them. In the discussion of the previous experiments BOUSSINGAULT had constantly compared the results obtained in a vegetable soil with those in a nitre bed. In reference to these new experiments he says that in the nitrification of vegetable earth, and in the materials of an artificial nitre bed, everything leads to the conclusion that the nitric acid is developed especially at the expense of organic substances. But it does not necessarily follow that the gaseous nitrogen of the atmosphere cannot contribute, within certain limits, to the production of nitrates. It is to ascertain whether this co-operation takes place that the new experiments were undertaken.

In the next year, 1860,* BOUSSINGAULT placed a mixture of 100 grams of very rich vegetable soil, and 300 grams of quartz sand, in a large balloon, such as he used in the previous vegetation experiments, moistened the mass, and then closed it perfectly by means of a caoutchouc cap. A second experiment was also arranged, in which the conditions were precisely similar, excepting that 5 grams of cellulose were added to the mixture. The materials could not be stirred, and it was decided to leave them in contact with confined air for a considerable time. The two vessels were, in fact, left for 11 years, when, in 1871, they were opened, and the contents examined.

The result was that in both cases there was a very considerable amount of nitrification, representing in Experiment 1 rather more, but in Experiment 2 with the cellulose less, than one-third of the original nitrogen of the soil. The actual loss of carbon was more than 4 times as great in Experiment 2 with the cellulose, as in Experiment 1 without it; amounting in Experiment 1 to about 16 per cent., and in Experiment 2 to about 43 per cent. of the original quantity.

Lastly, as to the nitrogen:—In Experiment 1, without cellulose, there was out of 0·4722 gram total nitrogen in the original soil, a loss of 0·0212 gram, corresponding to 4·5 per cent. of the original amount; and in Experiment 2, with the cellulose, there was, upon the same original amount of nitrogen a loss of 0·0081 gram, corresponding to 1·71 per cent. of the original.

* 'Compt. Rend.,' vol. 76, 1873, p. 22.

In regard to these new results BOUSSINGAULT says that, contrary to his anticipation, the combustion of the carbon of the non-nitrogenous organic matter, the cellulose, added to the soil, had not favoured the production of nitric acid.

He gives reasons for concluding that the process of nitrification had been completed before the opening of the vessels in 1871. At the same time, he shows that the amount, both of oxygen, and of salifiable bases, remaining, was sufficient for the production of much more nitrate.

Upon the whole he concludes as follows :—

It results from these researches, that, in the nitrification of vegetable soil, in a confined atmosphere not renewed, that is in stagnant air, gaseous nitrogen does not appear to contribute to the formation of nitric acid. The nitrogen determined in the soil in 1871, was not more than, but was even not quite so much as, in 1860. In the conditions of the experiment, the nitrification must have taken place at the expense of the organic substance of humus, which is found in all fertile soils.

Although, as we have already said, the experiments in question were obviously suggested by the results obtained in 1858 and 1859, which showed a gain of nitrogen, BOUSSINGAULT does not, throughout the discussion of the new results, offer any explanation of, or even refer to, the earlier ones. Further, it will be observed that in recording the negative results of the new experiments, he is careful to define the conditions under which they were obtained.

Always placing the greatest reliance, both in the work and in the conclusions of BOUSSINGAULT, we had been much impressed with the significance and importance of the earlier results and conclusions above referred to, which did not seem to be satisfactorily explained by the new ones, and in April, 1876, one of us wrote to him as follows :—

"We have been very much struck with some of your results with Leguminosæ, especially those with lupins in confined air in 1858 and 1859, and those with lupins and haricots in free air in 1858. May I ask whether it is your opinion that the free azote of the air does enter into combination, either by the direct agency of vegetation, or through that of the soil? And, if so, under what conditions do you think this action takes place, and what is the nature of the action?"

In answer BOUSSINGAULT wrote a long and interesting letter, dated May 19, 1876, in which he discussed various points of the subject of the sources of the nitrogen of vegetation, and replied as follows in reference to the special questions relating to his experiments in 1858 and 1859 :—

"Quant à l'absorption de l'azote gazeux de l'air par la terre végétale je ne connais pas une seule observation irréprochable qui l'établisse ; non seulement la terre n'absorbe pas d'azote gazeux mais elle en émet, ainsi que vous l'avez reconnu avec Mr. LAWES, comme l'a vu REISET pour le fumier, comme nous l'avons constaté, M. SCHLŒSING et moi, dans nos recherches sur la nitrification.

"S'il est en physiologie un fait parfaitement démontré, c'est celui de la non assimilation de l'azote libre par les végétaux, et je puis ajouter par les plantes d'un ordre inférieur, telles que les mycodermes, les champignons."

Thus, then, although by the terms of our inquiry, BOUSSINGAULT's attention was specially directed to the evidence of gain of nitrogen from the air by the soil which his experiments in 1858 and 1859 afforded, he, in 1876 states that he is not aware of any irreproachable observation establishing the reality of such an action, whilst, on the contrary, he considers it established that soils emit rather than gain free nitrogen.

Further, he considers it perfectly demonstrated, that neither plants of a higher, nor those of an inferior order, such as mycoderms and fungi, assimilate free nitrogen.

It is to be observed that, although BOUSSINGAULT clearly ignores the significance of the results to which we had directed his attention, he did not offer any explanation of them. Subsequently, on several occasions when passing through Paris, one of us sought to meet M. BOUSSINGAULT, and to discuss the question with him further, but he was each time in Alsace. However, one of us visited him at Liebfrauenberg in 1883, and had an interesting conversation with him on the subject. No special reference was made to his experiments of 1858 and 1859; but he clearly maintained the same view as to the non-fixation of free nitrogen, as given in the sentences above quoted from his letter of 1876.

It is remarkable, that in that letter he should so expressly give his opinion against the supposition that the lower organisms within the soil effect the fixation of free nitrogen, notwithstanding the evidence of his experiments of 1858 and 1859 that the gain, if there really were gain, was chiefly by the soil, and chiefly as organic matter, the accumulation of which he attributed to the development of mycodermic vegetation. It is true that, in the discussion of the results, he did not give any clear indication whether he considered that the apparent fixation was due in the first instance to the process of nitrification, the mycoderms only appropriating the nitrogen of the nitrates formed, or whether he supposed that the mycoderms themselves were the primary agents, and that the nitrification was only the result of the oxidation of the mycodermic remains.

It did indeed seem, that, in the results in question, there was *the germ of the germ explanation* of the fixation of free nitrogen, if such took place at all, in connection with vegetation. But we confess that BOUSSINGAULT's very distinct conclusion against the assumption of any such agency, notwithstanding the indications of some of his own experiments, leads us still to ask for further confirmation of the evidence of others in the same direction, which has been accumulating during the last few years.

PART III.

Summary, and General Considerations and Conclusions.

It seems desirable to endeavour to summarise the results, both experimental and critical, of this extended inquiry, relating to a very difficult and very complicated subject, and involving the consideration of very conflicting evidence, and of equally conflicting opinions in regard to it.

We will first give a *résumé* of the results, and conclusions, as given in Part I. of this paper, on—

1. *The Evidence relating to other Sources than Free Nitrogen.*

In our earlier papers we had concluded that, excepting the small amount of combined nitrogen coming down in rain and the minor aqueous deposits from the atmosphere, the source of the nitrogen of our crops was, substantially, the stores within the soil and subsoil, whether derived from previous accumulations, or from recent supplies by manure.

More recently, we have shown that the amount of nitrogen as nitric acid in the soil, was much less after the growth of a crop than under corresponding conditions without a crop. In the case of gramineous crops it was concluded that most if not the whole of their nitrogen was taken up as nitric acid. In the experiments with leguminous crops the evidence indicated that, in some cases the whole of the nitrogen had been taken up as nitric acid, but that in others that source seemed to be inadequate.

It has been further shown that, under otherwise parallel conditions, there was very much more nitrogen as nitric acid in soils and subsoils, down to a depth of 108 inches, where leguminous than where gramineous crops had for some time been grown. The indication was, that nitrification had been more active under the influence of leguminous than of gramineous growth and crop residue.

At the same time, comparing the amounts of nitrogen as nitric acid in the soil where the shallow rooting *Trifolium repens* had previously been grown, with those where the deeper rooting *Vicia sativa* had yielded fair crops, it was found that, down to a depth of 108 inches, the *Vicia* soil contained much less nitric acid than the *Trifolium repens* soil; and it was concluded that most, if not the whole, of the nitrogen of the *Vicia* crops had been taken up as nitric acid.

New results of the same kind, which related to *Trifolium repens* as a shallow rooting and meagrely yielding plant, to *Melilotus leucantha* as a deeper rooting and freer growing one, and to *Medicago sativa* as a still deeper rooting and still freer growing plant, very strikingly illustrated and confirmed the result of the exhaustion of the nitric acid of the subsoil by the strong, deep-rooting, and high nitrogen-yielding Leguminosæ. Still, the figures did not justify the conclusion that the whole of the large amount of nitrogen taken up by the *Medicago* crops could have had its source in nitric acid. It was obvious that much nitrification takes place near the surface; but as the surface soil became even somewhat richer in nitrogen, it was clear that it had not been the primary source of the whole of the nitrogen taken up by the plants. The source of much of it must have been either the atmosphere, or the subsoil; and if the subsoil, and yet not wholly as nitric acid, the question arises, in what other form of combination?

In another experiment, where one leguminous crop—beans—had been sown for many years in succession, but had frequently yielded very small crops, and sometimes failed, and over the whole period had given an average of little more than 30 lbs. of nitrogen per acre per annum, the land was then left fallow for several years, after which, in 1883, barley and clover were sown. In that year, in 1884, and in 1885, about 300 lbs. of nitrogen were removed per acre, chiefly in the clover crops. This result was obtained—where another leguminous crop had to a great extent failed, where the surface soil had become very poor in total nitrogen, where there existed a very small amount of ready formed nitric acid to a considerable depth, and where the surface was unusually poor in nitrogenous crop residue for nitrification.

Further, not only had this large amount of nitrogen been removed in the clover crops, but the surface soil became determinably richer in nitrogen. Here again, then, the primary source of the nitrogen of the crop could not have been the surface soil itself. It must have been either the atmosphere, or the subsoil; and assuming it to be the subsoil, the question arises whether it was taken up as nitric acid, as ammonia, or as organic nitrogen?

The various results adduced could leave no doubt that nitric acid was an important source of the nitrogen of the Leguminosæ. Indeed, existing evidence relating to nitric acid carries us quantitatively further than any other line of explanation. But it is admittedly inadequate to account for the amounts of nitrogen taken up, either by the *Medicago sativa* on the clover-exhausted land, or by the clover on the bean-exhausted land.

Direct experiments were made to determine whether the nitrogen of the Rothamsted raw clay subsoils, from which it was assumed much nitrogen had been derived in some way, was susceptible of nitrification, provided the nitrifying organisms, and other necessary conditions, were present. It was found that the nitrogen of such subsoils, containing only between 0·04 and 0·05 per cent. of nitrogen, and not more than six or eight parts of carbon to one of nitrogen, was susceptible of nitrification. It was also found that nitrification was more active in leguminous, than in gramineous crop subsoils.

Although it was clear that the nitrogen of raw clay subsoils, which constitutes an enormous store of already combined nitrogen, was susceptible of nitrification, provided the organisms are present, and the supply of oxygen is sufficient, the results did not indicate that these conditions would be adequately available in such cases as those of the very large accumulations of nitrogen by the *Medicago sativa* for a number of years in succession on the clover-exhausted land, or by the red clover on the bean-exhausted land.

The question arose—whether roots, by virtue of their acid sap, might not, either directly take up, or at any rate attack and liberate for further change, the otherwise insoluble organic nitrogen of the subsoil? Accordingly, specimens of the deep, strong, fleshy root, of the *Medicago sativa* were collected and examined, when it was found that the sap was very strongly acid. The degree of acidity was determined, and attempts were made so to free the extract from nitrogen so as to render it available for determining whether or not it would attack and take up the nitrogen of the raw clay subsoil. Hitherto, however, these attempts have been unsuccessful.

When this difficulty arose, it was decided in the meantime to examine the action on soils and subsoils, of various organic acids, in solutions of a degree of acidity either approximately the same as that of the lucerne root-juice, or having a known relation to it.

It was found that the weak organic acid solutions did take up some nitrogen from the raw clay subsoil, and more from the poor lucerne surface soil. But when solutions of only approximately the acidity of the root-sap were agitated with an amount of soil which it was thought would be sufficient to yield so much nitrogen as to insure accurate determination, it was found that the acid frequently became neutralised by the bases of the soil, and that less nitrogen remained dissolved after a contact of 24 hours, or more, than after only 1 hour. The strength of the acid liquids was therefore increased, and the relation of soil to acid diminished. More nitrogen was then taken up, and more after the longer than after the shorter period of contact. Still, on adding fresh acid solution to the already once extracted soil, a limit to the amount of nitrogen rendered soluble was soon reached.

Here again, the conditions of experiment in the laboratory are not comparable with those of the action of living roots on the soil, and the results obtained did not justify any very definite conclusion as to whether the action of the roots on the soil, by virtue of their acid sap, is quantitatively an important source of the nitrogen of plants having an extended development of roots, of which the sap is strongly acid.

Dr. G. LOGES has published the results of experiments in which he acted upon soils by pretty strong hydrochloric acid, and determined the amount of nitrogen taken up. One of his soils contained, however, 0·804, and the other 0·367, per cent. of nitrogen; whilst the surface soil of the lucerne plot at Rothamsted contained only about 0·125, and the subsoil, which is assumed to have yielded large quantities of nitrogen to the crops, little more than 0·04 per cent. Again, in the one case LOGES found 40 per cent., and in the other 22·6 per cent., of the total nitrogen taken up.

It is obvious, therefore, that such an action is not directly comparable with that of root-sap on a poor subsoil. LOGES concluded however that the substance taken up is an amide or peptone body.

MM. BERTHELOT and ANDRÉ have also published the results of experiments to determine the character of the insoluble nitrogenous compounds in soils, and of the changes they undergo when acted upon by hydrochloric acid. They found the nitrogen in the extract existed partly as ammonia, but in much larger proportion as soluble amides, and that the amounts obtained of both, increased with the strength of the acid, the time of contact, and the temperature. They also found that when the clear filtered acid extract is exactly neutralised by potash, one portion of the amide still remains soluble, whilst another is precipitated, showing that the amides rendered soluble constitute two groups. Such re-precipitation is quite in accordance with the results obtained in our own experiments, in which less nitrogen remained dissolved after 24 hours' than after only 1 hour's contact, when, with the longer period, the acidity of the extract became neutralised.

In the experiments of BERTHELOT and ANDRÉ, as in those of LOGES, the strength of acid used was much greater than in the Rothamsted experiments, and very much greater than is likely to occur in any root-sap. Further, the soil they operated upon was about 4 times as rich in nitrogen as the Rothamsted subsoils, and with the strongest acid, and a temperature of 100° C., nearly one-third of the total nitrogen of the soil was dissolved.

Still, the results of LOGES, and of BERTHELOT and ANDRÉ, are of much interest as confirming the supposition that the insoluble nitrogenous compounds in soils are, or yield, amide bodies, and as indicating the changes to which they are subject when acted upon by acids. Supposing, however, the acid root-sap so to act on the insoluble organic nitrogen of the soil, and especially of the subsoil, the question still remains, whether the amide rendered soluble is taken up as such, or undergoes further change before serving as food for the plant? It is seen that ammonia is an essential result of the reaction; and the further question arises, therefore, whether the liberated ammonia is taken up as such, or is first oxidated into nitric acid? Then, again, is the soluble amide subjected to further change, perhaps first yielding ammonia, and this again nitric acid? On this supposition we are again met with the difficulty as to the sufficient aëration of the subsoil.

Independently of much other evidence, our own direct experiments have shown it to be probable, if not certain, that fungi can utilise both the organic carbon and the organic nitrogen of the soil; though they seem to develop the more freely when the humic matters have not undergone the final stages of change by which the compound of so low a proportion of carbon to nitrogen as is found in raw subsoils, has been produced. As bearing on the question whether amides, rendered soluble within the soil, may be taken up as such by chlorophyllous plants the results of various experiments of others, made to determine whether such plants can take up such bodies, and assimilate their nitrogen, have been considered.

Upon the whole it seems probable, that green-leaved plants can take up soluble complex nitrogenous organic bodies, when these are presented to them under such conditions as in water-culture experiments, and that they can transform them and appropriate their nitrogen. If this be the case, it would seem not improbable that they could take up directly, and utilise, amide bodies rendered soluble within the soil by the action of their acid root-sap.

In connection with the subject of the conditions under which the insoluble organic nitrogen of soils and subsoils may become available to chlorophyllous plants, some results of Frank are referred to. He observed that the feeding roots of certain trees were covered with a fungus, the threads of which forced themselves between the epidermal cells into the root itself, which in such cases had no hairs, but similar bodies were found external to the fungus-mantle, which prolonged into threads among the particles of soil. Frank concluded that the chlorophyllous tree acquires its soil nutriment through the agency of the fungus.

Such a mode of accumulation by some green-leaved plants, obviously allies them in this respect very closely to fungi themselves; indeed, it is by an action on the soil which characterises non-chlorophyllous plants, that the chlorophyllous plant acquires its soil-supplies of nutriment. But inasmuch as, in the cases observed, the action was most marked in the surface layers of soil rich in humus, and it is stated that the development has not been observed on the roots of any herbaceous plants, the facts so far recorded do not aid us in the explanation of the acquirement of nitrogen by deep and strong rooted Leguminosæ from raw clay subsoils. Still, in view of the office within the soil which is by some attributed to micro-organisms, and other low forms, the observations are not without interest.

It is admitted that existing evidence on the various points which have been referred to is insufficient to explain the source of the whole of the nitrogen of the Leguminosæ.

The question arises, therefore, whether the free nitrogen of the atmosphere is fixed, either by the plant, under the influence of electricity or otherwise, or within the soil, by the agency either of electricity or of micro-organisms? We believe that the results of Boussingault, and those obtained in conjunction with the late Dr. Pugh at Rothamsted, are conclusive against the supposition of the fixation of free nitrogen by the higher plants, under conditions in which the possibility of electrical action, or of the influence of micro-organisms, is excluded. The following is a brief *résumé* of the more detailed account and discussion, given in Part II., of the recently published results and conclusions of others, from experiments for the most part made under such conditions as not to exclude the possibility of the influence of electricity or of micro-organisms.

2. *The Evidence relating to the Fixation of Free Nitrogen.*

In the experiments of M. BERTHELOT, in all of which the gains of nitrogen are comparatively small, they have in some cases been attributed to electrical action, and in others to the agency of micro-organisms within the soil.

M. BERTHELOT first showed that free nitrogen was fixed by various organic compounds under the influence of the silent electric discharge, at the ordinary temperature; and he suggested that such actions probably take place in the air during storms, and when the atmosphere is charged with electricity, organic matters absorbing nitrogen and oxygen. He also experimented with currents of much weaker tension, more comparable with those incessantly occurring in the air, and in all cases he found that nitrogen was fixed by the organic substance. The gains were in amount such as would explain the source of the nitrogen which be considers crops must derive from the atmosphere.

Subsequently, he found that free nitrogen was brought into combination by argillaceous soils, when exposed in their natural condition, but not when they were sterilised. He also found gain when the natural soils were enclosed. He considered the results showed that there was gain of nitrogen quite independently of any absorption of combined nitrogen; in fact that there was fixation of free nitrogen due to living organisms. He further considered that such gains, not only serve as compensation for exhaustion by cropping, &c., but explain how originally sterile argillaceous soils eventually become vegetable moulds.

He also made experiments on the fixation of free nitrogen by vegetable earth supporting vegetation; and he found that there was a gain about equally divided between the soil and the plant, the latter having taken it up from the soil, which he considers is the true source of gain.

The results obtained under the influence of the silent discharge in bringing free nitrogen into combination with certain vegetable principles, of course owed their special interest to the inference that thus free nitrogen might be brought into combination within the plant; but M. BERTHELOT now considers it doubtful whether the higher plants do bring free nitrogen into combination at all. Obviously, however, if there are organic compounds within the soil which have the power of bringing free nitrogen into combination under the influence of electricity, the soil may be the source, and yet the agent may be the feeble electric current. But, so far as it is assumed that free nitrogen is brought into combination in the atmosphere itself, the resulting compounds will be found in the air, and in the aqueous depositions from it; and the limit of the amount of combined nitrogen so available over a given area, in Europe at any rate, is pretty well known.

In conclusion, although it must be admitted that M. BERTHELOT carefully considered, and endeavoured to estimate, all other sources than free nitrogen, yet the conditions of risk and exposure to accidental sources of gain, in experiments in open air, are such that results so obtained cannot of themselves be accepted without reservation. But the fact that he found distinct gains in experiments in closed vessels, and that he obtained negative results with sterilised soils, is certainly in favour of the conclusion at which he arrived.

M. JOULIE made numerous vegetation experiments in which the soils and the plants were, with certain precautions, exposed to the free air, and in which known amounts of combined nitrogen were supplied. He found very variable, but in some cases very large, gains of nitrogen. He considered that the variations of result were largely due to the varying conditions as to mineral-supply in the different experiments.

M. JOULIE concluded that microbes probably play an important part in the fixation of nitrogen. He did not think that his results were favourable to the supposition that the plants themselves effected the fixation. For the present he limits himself to the establishment of the great fact of the fixation of the free nitrogen of the atmosphere, leaving to the future the exact explanation.

It is to be observed that the large gains shown were chiefly with a polygonous plant, buckwheat, and not with plants of the leguminous family, which are reputed to be "nitrogen collectors."

To show the practical importance of the fixation of free nitrogen, M. JOULIE calculates what would be the gain per hectare according to some of his results. It may be confidently affirmed, however, that such gains as he so estimates, do not take place, either with or without vegetation, in ordinary soils, in ordinary practice.

Dr. B. E. DIETZELL made vegetation experiments, in which plants were watered with distilled water, the drainage was returned to the soils, and the pots and their contents were exposed to free air, but protected by a linen roof; a rich garden soil, containing 0·415 per cent. of nitrogen, was used, several different conditions as to manuring were adopted, and peas and clover were the subjects of experiment. Thus the plants were of the leguminous family; but notwithstanding this, there was, in no case, a gain of nitrogen. In one there was neither gain nor loss, and in all the others there was a loss, in some cases amounting to about 15 per cent. of the total nitrogen involved.

That there should be loss with a soil containing 0·415 per cent. of nitrogen, that is about three times as much as most ordinary arable soils, is not at all surprising; and it is seen that, neither from the combined nitrogen of the atmosphere, or that due to other accidental sources, nor from free nitrogen, either directly or indirectly, did these reputed "nitrogen-collectors" gain nitrogen to compensate the losses from the rich soil. Indeed, Dr. DIETZELL'S results are quite accordant with well established facts.

Professor FRANK also made vegetation experiments in free air. His soil was a humus-sand, containing only 0·0957 per cent. nitrogen; distilled water was used for watering, and the vessels were deep and narrow cylinders, without any arrangement at the bottom for drainage, or for aëration.* In three experiments without a plant, in one with two lupins, and in one with one lupin and incarnate clover together,

* See foot-note at p. 61.

there was a loss of nitrogen; whilst in one with three lupins, and in one with one lupin there was a gain. FRANK considered it probable that where a loss was indicated with vegetation, there had nevertheless been a gain, but not enough to compensate the loss.

In another experiment, with a soil about 12 times as rich in nitrogen, and many times richer than ordinary arable soils, he found a loss, due mainly to evolution of free nitrogen; and referring to this result, he says that if such losses take place in ordinary agriculture there must be natural compensation.

In the experiments in the deep and narrow vessels, without drainage, and without plants to cause evaporation, movement, and aëration, loss by evolution of free nitrogen is only what would be expected. Such loss would also be expected in the two cases of loss with growth, in both of which there was admittedly decomposing organic matter. It was also to be expected in the very rich soil. But it is doubtful whether, in the two cases of gain with growth, and therefore movement within the soil, and aëration of it, there would be any loss. In none of the experiments with loss, however, were the conditions comparable with those of ordinary soils, under ordinary treatment, and the losses found cannot be taken as any indication of what takes place in ordinary practice. It is probable that in such practice the loss by evolution of free nitrogen is much less than is generally assumed in discussions of this subject. Doubtless there is, however, frequently considerable loss by the drainage of nitrates.

FRANK considers that, independently of direct evidence against the supposition that the gains were due to the absorption of combined nitrogen from the atmosphere, an objection to such a view is that it would not explain the circulation of nitrogen in nature; and his main conclusion is, that there are two actions going on within the soil, one liberating nitrogen, and the other bringing it into combination, the latter favoured by vegetation.

Upon the whole it would seem that the losses found by FRANK may be explained by the special conditions of the experiments themselves; whilst the gains, if not to be accounted for by sources of error incidental to experiments made in free air, can only be explained by fixation in some way.

The most remarkable of the results indicating the fixation of free nitrogen are those of Professor HELLRIEGEL and Dr. WILFARTH. HELLRIEGEL found that whilst plants of the gramineous, chenopodiaceous, polygonous, and cruciferous families required combined nitrogen to be supplied within the soil, papilionaceous plants did not depend on such soil-supplies.

Peas sometimes grew luxuriantly in washed sand with nutritive solutions free from nitrogen, but sometimes failed, root-nodules being developed coincidently with luxuriance, but not without it. But when to the non-nitrogenous sandy matrix a few c.c. of the watery extract of a rich soil were added, the luxuriance was always marked, as also was the development of the root-nodules. Lupins, however, failed when treated in the same way, but succeeded when seeded by a watery extract of a sandy soil where lupins were growing well, and root-nodules were then abundantly produced.

The amounts of produce recorded seemed to leave no doubt that they contained much more nitrogen than was supplied in the seed ; whilst the amount added in the soil-extract was quite immaterial. The negative result with Gramineæ, with peas under sterilised conditions, or in sand not seeded with rich soil-extract, and with lupins in sand not seeded, or seeded with the rich soil-extract, and, on the other hand, the positive result with peas in the seeded sand, and with lupins when the sand was seeded with an extract from a suitable soil, seemed to exclude the supposition of any other source of gain than the fixation of free nitrogen under the influence of micro-organisms ; and at first Hellriegel was disposed to connect the action with the root-nodules and their contents.

Wilfarth gave the results of a subsequent season's experiments, which fully confirmed those recorded by Hellriegel, both as to the negative result with other plants, and to the positive result with Papilionaceæ. Peas grew luxuriantly when the nitrogen-free soil was seeded with the watery extract from any cultivated soil, but serradella and lupins only when seeded with an extract from soil where these plants were growing.

In four experiments with lupins nearly 50 times as much dry substance was produced, and nearly 100 times as much nitrogen was assimilated, with, as without, seeding with the soil-extract !

Wilfarth concluded that the Papilionaceæ can derive the whole of their nitrogen from the air, but that it is doubtful whether the root-nodules are connected with the fixation, though the results point to the agency of bacteria in some way.

In reference to these results, whilst it can hardly be said that there is any unsolved problem in regard to the source of the nitrogen of other than our leguminous crops, it must be admitted that in spite of all the investigations and discussions of the last 50 years, the source of the whole of the nitrogen of these crops has not been satisfactorily explained by results obtained on the lines of inquiry until recently adopted. Evidence obtained on new lines should therefore receive careful consideration ; and there can be no doubt that in recent years cumulative evidence has been adduced indicating that certain chlorophyllous plants may avail themselves of nitrogen brought into combination under the influence of lower organisms ; the development and action of which would seem in some cases to be a coincident of the growth of the higher plants to be benefited. But such a conclusion is of such fundamental importance that further confirmation must yet be demanded before it can be considered to be fully established.

So long ago as 1853, Professor Emil von Wolff obtained 6 times as much dry produce of clover, grown in an ignited rich meadow soil, as in the same soil in its natural state. Thus, the increased growth, and the increased assimilation of nitrogen, took place in a soil not only nitrogen-free, but sterilised ; so that, unless micro-organisms were acquired during growth, the supposition of their influence in fixing free nitrogen would be excluded.

Much more recently WOLFF has made numerous experiments with oats, potatoes, and various Papilionaceæ, in river-sand; in some cases unwashed, and in some washed; in some without manure, in some with purely mineral manure, and in some with nitrate in addition. Accordantly with common experience, there was little increase in the oats or potatoes with mineral, but much with nitrogenous manure ; and, on the other hand, with the Papilionaceæ there was very marked increase with the mineral manure, and but little more by adding nitrate. In the experiments with lupins, beans, and clover, in unwashed sand, the results indicated gain of nitrogen beyond that probably due to the nitrogenous impurity in the sand ; but with sand-peas, grown in washed sand, which was assumed to be nitrogen-free, the gains from some external source were unmistakable.

As to the explanation, WOLFF does not suppose that free nitrogen is fixed by the plants themselves ; nor does he favour the view that it was fixed by the agency of micro-organisms. The plants may take up combined nitrogen from the air by their leaves ; but he thinks it more probable that combined nitrogen is absorbed from the air by the soil, and that free nitrogen is fixed within the soil under the influence of porous and alkaline bodies. He admits that it is not explained why cereals do not benefit by these actions as well as Papilionaceæ ; and he suggests whether the greater evaporation from their leaves causes greater aëration of the soil.

Here, then, the gain of nitrogen by the Leguminosæ is explained in a very different manner from that assumed by other recent experimenters. It seems to us, however, that the undoubted fact that the Gramineæ, and other plants than the Papilionaceæ, do not benefit by the actions supposed, excludes the supposition that WOLFF's results with Papilionaceæ are to be so explained. It is true that neither in the growth of the clover in ignited soil, nor in that of the sand-peas in the washed sand, were the conditions such as would seem favourable for the presence, development, and agency of micro-organisms. But if, in the experiments in free air, there was no accidental source of combined nitrogen, it would seem that the influence of micro-organisms is at least as probable as that of the actions which WOLFF supposes.

Professor ATWATER made numerous experiments, both on the germination and on the growth of peas. In eleven out of thirteen experiments on germination more or less loss of nitrogen was observed. In all but one out of fifteen experiments on vegetation, there was a gain of nitrogen, which was very variable in amount, and sometimes very large. As a general conclusion, he states that in some of the experiments half or more of the total nitrogen of the plants was acquired from the air.

He considers that germination without loss of nitrogen is the normal process ; that loss, whether during germination or growth, is due to decay, and therefore only accessory. Nevertheless, he goes into calculations of some of his own results, showing, by the side of the actual gains, the greater gains supposing there had been a loss of 15 per cent. of nitrogen, and the still greater gains if there had been a loss of 45 per cent., as in an experiment by BOUSSINGAULT under special conditions. Further, he says that whilst actually observed gains are proof of the acquisition of nitrogen, the failure to show gain only proves non-fixation, if it be proved that there was no liberation. He suggests that the negative results obtained by BOUSSINGAULT and at Rothamsted may be accounted for by liberation; though at the same time he recognises that the conditions of the experiments excluded the action of either electricity or microbes. We may remark that, in the experiments both of BOUSSINGAULT and at Rothamsted, any cases of decay were carefully observed, and the losses found explained accordingly; and it may be confidently asserted that the conclusions drawn were not vitiated by any such loss. This specious objection, putting out of court all negative results, is, however, a very old one; as also is the one resuscitated by ATWATER, that luxuriance must be forced to a certain degree to favour the fixation of free nitrogen. On this point we may state that the results obtained at Rothamsted were as distinctly negative when luxuriance was favoured by supplies of combined nitrogen as when it was not.

ATWATER concludes that his results do not settle whether the nitrogen gained was acquired as free or combined nitrogen, by the foliage, or by the soil. He considers, however, that, in his experiments, the conditions were not favourable for the action either of electricity or of micro-organisms; and he favours the assumption that the plants themselves were the agents. Lastly, he considers the fact of the acquisition of free nitrogen in some way to be well established; and that thus facts of vegetable production are explained, which otherwise remain unexplained. To this, and other points involved, we shall refer again in our concluding remarks.

Lastly, we have to summarise those of the results and conclusions of BOUSSINGAULT which bear upon the present aspect of the question of the sources of the nitrogen of vegetation. In his earlier experiments, as in those at Rothamsted, sterilised materials had been used as soils; but in 1858 he commenced a series in which more or less of a rich garden soil was mixed with sand and quartz. In some cases the plants were grown in free air, and in others in closed vessels with confined air. In several cases there was more or less gain of nitrogen; but the greatest gain was in an experiment with a lupin grown in a closed vessel. BOUSSINGAULT points out that it was the soil and not the plant that had fixed the nitrogen. The result was so marked that he repeated the experiment in 1859, when he obtained almost identically the same amount of gain as in 1858. He also put 120 grams of the rich soil into a shallow dish, moistened it with distilled water, and exposed it to the air as an experiment on fallow. The results showed a small gain of nitrogen.

BOUSSINGAULT further found that mycodermic vegetation went on in rich soil, and he considered the gains of organic nitrogen represented the remains of such vegetation; whilst the fallow experiment indicated that the experimental plants had little to do with the action. His general conclusion was, that from the numerical results it must be believed that the soil had fixed nitrogen; and he considered that, if there were not absolute proof, there was strong presumption, that the nitrogen of the air takes part in nitrification.

In the next year, 1860, he put into one large glass balloon a mixture of rich soil and sand, and into another a similar mixture with cellulose in addition; each was moistened with distilled water, and the vessels were then closed up for 11 years. During this period, without cellulose rather more, and with cellulose rather less, than one-third of the nitrogen of the soil was nitrified; but in neither case was there any gain of total combined nitrogen. There was, indeed, in both cases, a slight loss of nitrogen indicated. BOUSSINGAULT concluded that free nitrogen had not contributed to the formation of nitric acid.*

The later results of BOUSSINGAULT did not therefore confirm those he obtained in 1858 and 1859; and in answer to one of ourselves he wrote in 1876, that he was not aware of any irreproachable observation which established the reality of the fixation of free nitrogen by the soil. He further stated his belief that neither the higher plants, nor mycoderms, nor fungi (*champignons*), fix free nitrogen. He also maintained the same view in conversation in 1883.

BOUSSINGAULT'S very distinct final conclusion against the supposition of the fixation of free nitrogen within the soil, by the agency of the lower organisms, notwithstanding his own clear recognition in 1858 and 1859 of the possibility of such an action, points to the necessity for still further confirmation of the evidence of others on the point during the last few years; for it will be remembered that whatever other sources of error were possible, the experiments in question were made in closed vessels, and not in free air, with all the risks incident to experiments so conducted; and if there may have been error with such an experimenter, and under such conditions, caution should surely be exercised in accepting very important conclusions founded on results obtained for the most part under less favourable conditions.

3. *General Considerations and Conclusions.*

So much for the evidence of direct experiment as to whether the higher plants, or soils, by the agency either of micro-organisms or otherwise, fix the free nitrogen of the atmosphere. It is clear that since experimenting in free air instead of in closed vessels has become more general, there has been a great accumulation of evidence which is held to show the fixation of free nitrogen. But not only are the gains in

* Quite recently ('Compt. Rend.,' vol. 106, 1888, pp. 805 and 898) M. SCHLŒSING referring to these results says that for his part he was satisfied with this result of BOUSSINGAULT, and should not have entered upon new experiments, had not the question been recently taken up and answered in a contrary sense. He then gives the results of experiments in which he submitted various soils to the action of air in closed vessels, supplying oxygen as it was used up. The result was that the air of the vessels neither lost nor gained nitrogen. There was therefore no fixation.

some cases small, and in others very large, but the modes of explanation are so different, indeed so conflicting, that it seems essential to hold final judgment in abeyance for the present.

The various modes of explanation of the observed gains of nitrogen are :—that combined nitrogen has been absorbed from the air, either by the soil or by the plant; that there is fixation of free nitrogen within the soil by the agency of porous and alkaline bodies; that there is fixation by the plant itself; that there is fixation within the soil by the agency of electricity; and finally that there is fixation under the influence of micro-organisms within the soil. The balance of the evidence recorded, is undoubtedly in favour of the last-mentioned mode of explanation. Indeed, it seems to us that, if there be not experimental error, there is fixation within the soil, under the influence of micro-organisms, or other low forms.

Assuming that definite decision on the point must wait for further evidence and discussion, it will nevertheless be well, in the meantime, to consider the facts of agricultural production in their bearing on the question, with a view of forming a judgment as to how far the establishment of the reality of the fixation of free nitrogen, either by the plant or by the soil, is so essential for the solution of the problems which such production presents, as is by some supposed.

It has been seen that much of the investigation that has been undertaken in recent years, has been instigated by the assumption that there must exist natural compensation for the losses of combined nitrogen which the soil suffers by the removal of crops, and for the losses which result from the liberation of free nitrogen from its combinations under various circumstances. In some cases, however, the object seems to have been for the most part limited to an attempt to solve the admitted difficulty as to the explanation of the source of the whole of the nitrogen of the Leguminosæ.

As to the losses which the soil sustains by the removal of crops, BERTHELOT for example assumes that 50 to 60 kilog. of nitrogen will be annually removed from a hectare of meadow (= 45 to 54 lbs. per acre), and that as only 10 kilog., or less, of this will be restored as combined nitrogen in rain, &c., there will be an annual loss of from 40 to 50 kilog. per hectare (= 36 to 45 lbs. per acre); so that, if there were not compensation from the free nitrogen of the air, the soil would become gradually exhausted. Further, he considers that the fact of the fixation of free nitrogen, not only explains how fertility is maintained, but how argillaceous soils which are sterile when first brought into contact with the air, gradually yield better crops, and at length become vegetable moulds. FRANK again, assumes that the average loss of nitrogen by the removal of crops is 51 kilog. per hectare (= 45 lbs. per acre).

It is quite true, that a good hay crop may contain as much as 50 to 60 kilog. of nitrogen per hectare, but it may safely be affirmed that, in ordinary practice, even in the case of an unusually fertile meadow, such an amount is not annually removed for a number of years in succession, without the periodical return of manure supplying nitrogen; whilst, taking the average of soils, the annual yield will not reach the amount supposed, even with the ordinary periodic return, and without such return gradual exhaustion would be very marked. Indeed, it is well known that there is no more exhausting practice than the annual removal of hay without return of manure; so that, in point of fact, restoration in anything like the degree supposed certainly does not take place. Next to the removal of hay, the consumption of grass for the production of milk is the most, but still very much less, nitrogen-exhausting; whilst if the grass be consumed by store or fattening animals, the loss is very much less still; indeed it is very small.

Obviously, however, it is more important to consider, what is the probable average loss of nitrogen over a given area by the removal of crops generally, and not by that of grass alone. Moreover, in making such an estimate it is not the total nitrogen of the crops that has to be reckoned; but, taking into account the return by manure, only the amount eventually lost to the soil. With the great variation according to circumstances, it is of course very difficult to estimate this at all accurately; but we may state that two independent modes of estimate lead to the conclusion that, for Great Britain for example, the average annual loss of nitrogen is more probably under than over 20 lbs. per acre (= 22·4 kilog. per hectare). In fact, the loss by cropping, under the usual conditions of more or less full periodical return by manure, is by no means so great as is generally assumed in discussions of this subject.

The loss of nitrates by drainage may, however, in some cases be considerable. There may also, under some circumstances, be loss from the soil by the evolution of free nitrogen. Such loss may take place in the manure heap, or in soils very heavily manured, as in market gardening, for example. But in ordinary agriculture such excessive manuring seldom takes place; and the soil is generally much poorer in nitrogen than in the cases of the experiments which have been quoted as showing great loss from rich soils. Loss may also take place when the soil is deficiently aërated; but here again the conditions of the experiments cited, in which considerable loss by evolution of free nitrogen was observed, are not the usual conditions of soils in actual practice. Indeed, the balance of evidence is against the supposition that there is a constant and considerable loss by the evolution of free nitrogen from arable soils which are only moderately rich in organic nitrogen, and which are fairly drained, either naturally or artificially. Some illustrations bearing upon this point will be found at pages 62-3.

Again, M. BERTHELOT thinks it probable, though not absolutely established, that there is loss of nitrogen from the plant itself during growth. Long ago, we ourselves supposed that there was such loss; but careful consideration of the evidence relating to the subject has led us to conclude that it is not proved, and to believe that it probably does not take place. It may be observed that when in his vegetation experiments M. BOUSSINGAULT found a loss of nitrogen, there was coincidently some decaying vegetable matter, such as fallen leaves; and in somewhat parallel experiments at Rothamsted, no loss of nitrogen was found as a coincident of growth, and in the absence of dead vegetable matter. Indeed, if there were such loss during growth when there was no decay, either in M. BOUSSINGAULT'S experiments or in our own, it must have been almost exactly balanced by corresponding gain ; an assumption which is without any proof, but which has nevertheless had its advocates.

In fact we conclude, that under the existing conditions of practical agriculture in temperate climates, the annual loss of combined nitrogen over a given area, by cropping and otherwise, is by no means so great as has been assumed ; that the restoration required to compensate the loss is therefore correspondingly less ; and further, that the known facts relating to the maintenance or the reduction of the fertility of soils, do not point to the conclusion that such loss as actually does take place, is compensated by such restoration.

The well-known accumulation of nitrogen which takes place in the surface soil within a few years, when arable land is laid down to grass, is, it may be admitted, not conclusively explained. At the same time, there is, to say the least, quite as much evidence in favour of the assumption of a subsoil, as of an atmospheric, source. At Rothamsted, for example, there is, in soil and subsoil, to the depth at which the action of some deep-rooted and large nitrogen-accumulating plants has been proved, a store of about 20,000 lbs. of already combined nitrogen per acre. It is true that whilst many other soils and subsoils will contain as much, or more, many will contain much less. Still, if further investigation should confirm the indications given in this and former papers, that in the case of the deep and strong rooting, and high nitrogen-yielding, Leguminosæ, much at any rate of their nitrogen probably has its source in the combined nitrogen of the subsoil, and that the accumulation in the surface soil is due to nitrogenous crop-residue, the nitrogen of which has come from the subsoil, it is obvious that a like explanation would be applicable to the accumulation which takes place when arable land is laid down to grass, including herbage of various root-ranges, and various habits of root-collection.

Then, again, as to the supposition that the gains of nitrogen in argillaceous matters of very low initial nitrogen contents, which gains are attributed to the fixation of free nitrogen, serve to explain the gradual formation of vegetable soils, there cannot be any doubt that, so far as nitrogen is concerned, the natural fertility of most soils is due to the accumulation of ages of natural vegetation with little or no removal of it, by animals or otherwise. If the amounts of nitrogen even now brought into combination over a given area under the influence of electricity in Equatorial regions, were not exceeded in the earlier periods of the history of our globe, that would be quite sufficient, with growth and with little or no removal, through the ages which modern science teaches us to reckon upon, for the ascertained accumulations in natural prairie, or forest lands ; and it is these which have to a great extent furnished us with our meadows and pastures, and arable soils. Frequently the natural forests have been on the more elevated, or the more undulating lands, and the soils they have formed are less rich than the prairie lands, for the most part found in the valleys or on the plains. Taking the vast areas of fertile natural prairie on the American continent, for example, sometimes of several feet in depth, it may be estimated that, in such cases, each foot of depth will contain from 6000 to 10,000 lbs., or even more, of combined nitrogen per acre (= 6720 to 11,200 kilog. per hectare); and the probable time of these accumulations entirely obviates the necessity of calling in the aid of the free nitrogen of the atmosphere, brought into combination either under the influence of the plants themselves or of micro-organisms within the soil.

Further, the history of agriculture so far as it is known, indicates that soils under cultivation without supplies by manure from external sources, do, as a matter of fact, gradually become less fertile. This, as a rule, will take place more rapidly in undulating or high forest lands, than in the natural grass or prairie lands of the plains.

Again, if we compare the amount of nitrogen in the surface soil of permanent grass land, with that of adjoining land of the same original character, but which has for some time been under arable culture, we find that the latter is much poorer in nitrogen. In illustration, it may be stated that whilst the surface soil of the grass land at Rothamsted contains from 0·25 to 0·30 per cent. of nitrogen, that of the corresponding arable land only contains from 0·1 to 0·15 per cent. The arable soil has, in fact, originally been covered with natural vegetation of some kind, with comparatively little removal, and consequent accumulation; whilst, under arable culture, much of the accumulated nitrogen has been used up, and the loss has not been compensated by free nitrogen brought into combination, under the influence either of electricity, or of organisms within the soil. Whether or not there is any restoration of the kind supposed, we believe that a consideration of the origin of soils generally, and of the history of agriculture in different countries, will lead to the conclusion that the losses of combined nitrogen by cropping, and in other ways, are not compensated by corresponding amounts of free nitrogen constantly brought into combination.

The Rothamsted field experiments have indeed now been continued long enough to afford some pertinent examples bearing upon this subject.

Thus, in the case of the fields under continuous wheat, continuous barley, alternate wheat and fallow, and continuous root-crops, the average annual yield of nitrogen in the crops with mineral, but without nitrogenous manure, has only been about or under 20 lbs. per acre (= 22·4 kilog. per hectare); the amount has declined to less than the average in the later years, and, coincidently with the continuous and diminishing growth, the percentage of nitrogen in the surface soil has been considerably reduced. The loss by the removal of even such small crops, together with that by drainage, has, therefore, as a matter of fact, not been compensated by free nitrogen brought into combination, either by the plants, or within the soil.

In a field where the leguminous crop, beans, had been grown 25 years out of 32, with mineral but without nitrogenous manure, and had yielded less than average agricultural crops, the percentage of nitrogen in the surface soil was also greatly reduced.

In another field, where the leguminous crop, red-clover, had been sown 12 times in 30 years, the clover failed many times, the yield of nitrogen in the crops very greatly diminished, and the percentage of nitrogen in the surface soil was greatly reduced.

Again, in a rich garden soil, where red clover has been grown for more than thirty consecutive years, and has yielded throughout good, but gradually much diminishing crops, it was found, after the first 22 years, that the nitrogen in the surface soil had been reduced from 0·5095 to 0·3634 per cent., calculated on the soil dried at 100° C.

Even in an actual course of rotation, of turnips, barley, clover or beans, and wheat, with mineral, but without nitrogenous manure, the percentage of nitrogen in the surface soil has been much reduced; whilst in a parallel rotation in which fallow takes the place of the clover or beans, the reduction is still greater.

Thus, in all the cases cited, including gramineous, cruciferous, chenopodiaceous, and even leguminous crops, and a rotation of crops, when each has been grown for many years in succession without nitrogenous manure, and has yielded comparatively small and declining amounts of nitrogen in the crops, there has, coincidently, been a considerable reduction in the amount of nitrogen in the surface soil. There has, in fact, not been compensation from the free nitrogen of the air, or at any rate not at all in amount corresponding to the annual losses.

Lastly, grass land which, under the influence of a full mineral manure, including potash, but without any supply of nitrogen for more than thirty years, has grown crops containing large amounts of comparatively superficially rooting leguminous herbage, succeeded by increased amounts of gramineous herbage, has, under those conditions, yielded about the same amount of nitrogen per acre as M. BERTHELOT assumes to be the average produce of a meadow; but it has done so only with coincident great reduction in the nitrogen of the surface soil.

Whether, therefore, we consider the facts of agriculture generally, or confine attention to special cases under known experimental conditions, the evidence does not favour the supposition that a balance is maintained by the restoration of nitrogen from the large store of it existing in the free state in the atmosphere. Further, our original soil-supplies of nitrogen are, as a rule, due to the accumulations by natural vegetation, with little or no removal, over long periods of time; or, as in the case of many deep subsoils, the nitrogen is largely due to vegetable and animal remains, intermixed with the mineral deposits. The agricultural production of the present age is, in fact, so far as its nitrogen is concerned, mainly dependent on previous accumulations; and as in the case of the use of coal for fuel there is not coincident and corresponding restoration, so in that of the use or waste of the combined nitrogen of the soil, there is not evidence of coincident and corresponding restoration of nitrogen from the free to the combined state.

In the case of agricultural production for sale, without restoration by manure from external sources, a very important condition of the maintenance of the amount of nitrogen in the surface soil, or of the diminished exhaustion of it, is the growth of plants of various range and character of roots, and especially of leguminous crops. Such plants, by their crop-residue, enrich the surface soil in nitrogen. It is, as a rule, those of the most powerful root-development that take up the most nitrogen from somewhere; and this fact points to a subsoil source. But independently of this, which obviously might be held to be only evidence of the necessity of obtaining water and mineral matters from below, in amount commensurate with the capability of acquiring nitrogen from the air, the experimental results recorded in this paper can leave little doubt that such plants obtain at any rate much of their nitrogen from the subsoil. The question remains—whether or not the whole of it is derived from the soil and subsoil? At present it is not proved that it is. It is equally not yet conclusively proved that it comes from the atmosphere. It may be safely affirmed that, in the case of our gramineous, our cruciferous, our chenopodiaceous, and our solaneous crops, atmospheric nitrogen is not the source. If, therefore, it should be proved to be the source in the case of the Leguminosæ, it may be that the development of the organisms capable of bringing free nitrogen into combination within the soil is favoured by leguminous growth and crop-residue, as there can be little doubt is the case with those which induce nitrification.

Bearing in mind, however, the very large store of already existing combined nitrogen, especially in subsoils, it is obviously important to consider, in what way, or in what degree, this store may contribute to chlorophyllous vegetation?

There is in the first place the question, whether the roots of some plants, and especially those of certain deep and powerfully rooting Leguminosæ, whose root-sap is strongly acid, may either directly take up organic nitrogen from the soil and subsoil, or may attack and liberate it for further change, the nitrogen so becoming more available.

Again, so far as is known, the Fungi generally, derive their nitrogen largely, if not exclusively, from organic nitrogen. In the case of those of fairy rings for example, there can be no doubt that they take up from the soil organic nitrogen which is not available to the meadow plants, and that, on their decay, their nitrogen becomes available to the associated herbage. In the case of the fungus-mantle observed by FRANK on the roots of certain trees, it is to be supposed that the fungus takes up organic nitrogen, and so becomes the medium of the supply of the soil-nitrogen to the tree. More pertinent still, is the action of the nitrifying organisms in rendering the organic nitrogen of the soil and subsoil available to the higher plants. It may well be supposed, therefore, that there may be other cases in which lower organisms bring the organic nitrogen of the soil and subsoil into a more available condition; whilst it seems not improbable that the growth and crop-residue of certain plants favour the development and action of special organisms. In conclusion we would submit that, whether or not it may eventually be conclusively proved that lower organisms have the power of bringing free nitrogen into combination within the soil, it would at any rate be not inconsistent with well established facts, were it found that the lower serve the higher, chiefly, if not exclusively, by bringing into a condition available to them, the combined nitrogen already existing, but in a comparatively inert state, in our soils and subsoils.

Postscript.

(Added October, 1888.)

As it seemed to us that, of the various results which have been considered relating to the fixation of free nitrogen, those of Hellriegel and Wilfarth are the most definite and significant, we decided to institute experiments on somewhat similar lines. We hoped to commence them early in the summer, but were not able to do so until the beginning of August. Decisive results cannot, therefore, be expected this season, but the experience gained will be of value in subsequent experiments.

This preliminary series comprises experiments with peas, blue lupins, and yellow lupins. The peas are grown (1) in washed sand, with the necessary mineral nutriment added, but with no supply of combined nitrogen beyond a small determined amount in the washed sand, and that in the seed sown; (2) in similarly prepared sand, but seeded with the extract from a rich garden soil; (3) in the rich garden soil itself. Each description of lupin is also grown—(1) in sand prepared as for the peas; (2) in the same washed sand, seeded with the extract from a sandy soil where lupins had grown luxuriantly; (3) in the lupin sandy soil itself; (4) in rich garden soil. The pots are all arranged in a small greenhouse.

As the plants are still growing, no quantitative results are as yet available. It may be observed, however, that, so far as can be judged by the eye, there seems, in the case of the peas, to be somewhat more growth where the sand was seeded with the soil-extract, supposed to contain organisms, than where it was not; whilst in the cases of the lupins there is apparently even somewhat less growth with than without the sandy soil-extract. Both with the peas and the lupins, the growth is very much more luxuriant in the garden soil; and in the case of the yellow lupins it is almost as luxuriant in the sandy soil in which lupins had grown, as in the garden soil. These first experiments can obviously be only considered as initiative; but it is intended to analyse the products in due course, and to undertake a new series earlier in the season next year.

II. *On the Secretion of Saliva, chiefly on the Secretion of Salts in it.*

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Previous Observations.

The earliest observations on variations in percentage of salts in saliva with which we are acquainted, are those of Ludwig and Becher,* in 1851. They analysed successive portions of saliva, obtained under different conditions, from the submaxillary gland of the Dog.

Three experiments were made on the effect of protracted secretion; in two of these the percentage of salts sank in the successive portions of saliva, but in the remaining one, the third and fourth samples of saliva contained a rather higher percentage of salts than the second and first samples. The total amount of saliva collected in this case was 48·5 grm.

Three experiments were made in the following manner:—Saliva was collected, then blood withdrawn from the animal, water injected in the place of the blood, and saliva again collected; in two of these experiments the defibrinated blood was re-injected, and a further portion of saliva obtained. In all these cases the percentage of salts in the saliva sank during secretion.

Lastly, in one experiment fourteen samples of saliva were obtained, in all 177 grm.; and twice during the course of the experiment 150 grm. of a 7·33 per cent. solution of sodium chloride were injected. After the first injection there was a rise in the percentage of salts in the saliva; after the second injection there was a fall in the percentage of salts below that of the first sample. A few only of the samples of saliva were analysed.

These observations showed that during secretion the percentage of salts falls in most, but not in all, cases; and they indicated that the percentage of salts depends upon the condition of the gland with regard to fatigue.

Heidenhain † placed the matter on a different basis. He analysed successive small quantities of saliva, secreted at different rates, and found that, up to a certain limit,

* Ludwig and Becher, 'Zeitschr. f. rat. Med.,' New Series, vol. 2, 1851, p. 278.

† Heidenhain, 'Studien des physiol. Instituts zu Breslau,' Part 4, 1868, pp. 30 *et. seq.*, and 'Archiv f. d. gesammte Physiologie,' vol. 17, 1878, p. 3.

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the percentage of salts in saliva increases with its rate of secretion. As we shall frequently have occasion to refer to this conclusion, we shall, for the sake of brevity, call it HEIDENHAIN's law.

Since, in experiments like those of LUDWIG and BECHER, the rate of flow of saliva would, as a rule, steadily decrease, it was most probable that the variations in the percentage of salts observed by them were due to variations in the rate of secretion. And HEIDENHAIN* came to the conclusion that the percentage of salts in saliva was not influenced by the state of the gland, except in so far as the state of the gland led to an alteration in the rate of flow; so that at the end of a protracted secretion, the percentage of salts would be the same as at the beginning, provided the rate of secretion were the same.

WERTHER,† in the course of some observations on the secretion of the various salts which occur in saliva, has repeated HEIDENHAIN's experiments, taking larger quantities of saliva, and confirms HEIDENHAIN's conclusions.

So far, then, it would appear that the secretion of salts depends in some not clearly definable way upon the secretion of water, and upon that alone.‡

Both in HEIDENHAIN's and in WERTHER's Tables there are a considerable number of departures from the law that an increased rate of secretion causes an increased percentage of salts. In HEIDENHAIN's§ experiments, out of thirty-six estimations there are thirteen divergences from the law. Some of these, it is true, are slight. They are all referred by HEIDENHAIN to unavoidable variations in the rate of secretion during the time of collecting each sample of saliva. But it must be noticed that HEIDENHAIN does not expressly say that he observed during the collection of the samples of saliva any especial variation in the rate of secretion of those particular samples which, on analysis, were found not to follow the law of increased percentage of salts with increased rate of secretion. Hence, although the explanation is a probable one, it is, as matters stand, not satisfactorily proved.

Causes of Variation in the Rate of Secretion, on apparently Equal Stimulation of the Chorda Tympani.

In order to observe accurately the connection between the rate of secretion and the percentage of salts in the saliva, it is essential that each sample of saliva should be secreted at the same rate throughout. It is, however, impossible, except in very large Dogs, to obtain a sufficient quantity of saliva for analysis, the rate of secretion

* HEIDENHAIN, *op. cit.*, 1878.

† WERTHER, 'Archiv f. d. ges. Physiologie,' vol. 38, 1886, p. 293.

‡ It was some time ago pointed out by one of us ('Journal of Physiology,' vol. 2, p. 269, 1879) that the percentage of salts in saliva does not always increase with an increase in the rate of secretion. But the only analyses given were of parotid saliva in the Dog, obtained first by stimulating the sympathetic, and then by injecting pilocarpin. For an account of the recent observations of NOVI, *cf.* p. 150.

§ 'Archiv f. d. ges. Physiologie,' vol. 17, p. 8, Table II.

of which is constant. During the time of collecting the saliva the rate of secretion varies ; in two successive samples the variation in rate will almost certainly be not quite the same, and, in consequence, the relation between the percentage of salts and the rate of secretion will be obscured. The variations in the rate of secretion which occur when the chorda tympani is stimulated are partly due to normal causes, and partly to abnormal causes brought about by the exposure of the nerve. Normally, when the chorda tympani is stimulated the rate of secretion rapidly rises to a maximum, and then slowly declines. When the nerve is dissected out, its irritability gradually falls, and if, as often is the case in dissecting out the chorda, some lobules of the sub-lingual gland are cut through, so that their secretion oozes out and soaks into the nerve, its irritability falls rapidly ; in either case it may happen that, in collecting a sample of saliva, the stimulus previously causing a rapid secretion causes only a slow one ; on seeking to correct this by increasing the strength of the stimulus the secretion often becomes over-rapid, and a mixture of salivas secreted at very different rates is the result.

Further, as HEIDENHAIN has pointed out, a variation in rate is brought out on chorda stimulation by the unequal irritability of the nerve along its course ; a very slight shifting of the electrodes in either direction may cause a considerable variation in the rate of secretion. In Experiment 2, No. I., for example, the electrodes were placed on the part of the chorda adjoining the lingual nerve, but the number of drops of saliva produced by stimulating for 30 seconds varied from 1 to 4 with the index of the secondary coil at 12 cm., and from $3\frac{1}{2}$ to 8 with the index of the secondary coil at 11·5 cm.

VARIATIONS IN THE PERCENTAGE OF SALTS IN CHORDA SALIVA OBTAINED UNDER NORMAL CONDITIONS.

Our first experiments were to try whether, by noting the variations in the rate of secretion during the time of collecting each sample of saliva, we could account for any variation that might occur in the percentage of salts. But, as we had no doubt of the general truth of HEIDENHAIN'S conclusions, we performed the experiments under somewhat different conditions from those of HEIDENHAIN, so as to still further test these conclusions.

Unless otherwise mentioned the following procedure was adopted in each of the Experiments.

Morphia in 5 per cent. solution was injected sub-cutaneously; in half to three-quarters of an hour, when severe pinching of the skin produced no movement, the animal was given chloroform. A three-way tube was tied in the trachea, one limb of the tube being connected at intervals with a bottle containing a mixture of chloroform and ether.

The lingual nerve was ligatured and cut peripherally of the point where it gives off the chorda tympani; lifting this up, the central end of the lingual nerve and then the chordo-lingual were isolated for some little distance, the chordo-lingual cut, and the chorda tympani isolated for a variable distance. During stimulation the lingual nerve was raised by the ligature, and the electrodes slipped under the chorda; in the intervals of stimulation the chorda was covered up.

The interrupted current of a du Bois' induction machine was used for stimulation; in the account of the experiments, the distance of the index of the secondary coil from the primary is given in centimetres; thus, $c = 12{\cdot}5$ indicates that the index of the secondary coil was at 12·5 cm. of the scale attached to the machine. One Daniell's cell was used, but not the same in the several experiments.

A glass cannula was tied in WHARTON's duct, and, for convenience of collecting the saliva and of counting the drops, the cannula was bent at the end. The object of counting the drops was to note variations in the rate of secretion whilst collecting any one sample of saliva; since the size of the drops depends upon the rate of secretion and upon various other conditions, the number of drops collected in successive samples often gives a very rough indication only of the amount of saliva in each.

The saliva was collected in small-necked bottles graduated in centimetres, so that the amount of saliva obtained at any time could be roughly estimated. The saliva was measured in the following way:—The level of the saliva in the bottle was marked by a strip of adhesive paper, the bottle was emptied and dried, and then water was allowed to run into it from a burette up to the level of the strip of paper.

Before collecting a sample of saliva under any given conditions, 1 *to* 3 *c.c. of saliva obtained under these conditions were thrown away.*—In the account of the several experiments the amount thrown away is, for special reasons, occasionally mentioned, and always when the amount thrown away was less than ·75 c.c. It may be mentioned that one to two drops is probably as much as the gland ducts and lumina contain.

The sympathetic nerve, when it was necessary to stimulate it, was dissected out from the vagus in the neck, ligatured, and cut. When salt solution or other fluid in quantity was injected into the blood it was first warmed to about 38° C.

Pilocarpin was injected into the blood as pilocarpin nitrate, and atropin as atropin sulphate.

Method of Analysis.—The amount of each sample of saliva collected is given in the description of the experiments. When there was sufficient saliva, 2·8 to 3·0 c.c. were taken for analysis. The weighed quantity of saliva was dried in a platinum crucible at 100° C. This temperature was found to be quite high enough to drive off all the water in five to six hours. When further heating of the crucible produced no diminution in weight, the weight of the residue, after cooling over sulphuric acid, gave the total amount of solids. The residue was then carefully ignited over a Bunsen flame, the crucible being held in the flame by means of tongs. The smallest possible amount of heat necessary to secure complete ignition of the organic compounds was employed, in order to prevent loss by volatilisation and possible loss by some of the fused salts creeping over the edge of the crucible. Less than three-quarters of a minute generally sufficed to burn off all carbonaceous matter. The residue, weighed after cooling over sulphuric acid, gave the total salts. The weighing was performed to ·0001 grm. In the Tables we have given the percentage composition of the various samples of saliva to three places of decimals only, since, for our purposes, the inaccuracy incidental to the determination of the rate of secretion made of no value the fourth—and sometimes the third—place of decimals in the percentage composition.

Experiment 1.

July 29, 1887. -Dog, rather small. The saliva in each case was obtained by stimulating the chorda tympani.

Number of sample.	Time of beginning to collect saliva.	Position of secondary coil.	Total time of collection of saliva in minutes.	Amount of saliva, and time during which actually secreted.	Rate of secretion in c.c. per min.	Mean rate of secretion.	Percentage of organic substance.	Percentage of salts.	Remarks.
I.	11.46	17	$7\frac{1}{2}$	1 c.c. in 2 min. 4 c.c. in $4\frac{1}{2}$ min.	·5 ·9	·770	1·722	·595	Chorda was stimulated twice, interval of 1 min. between the stimulations. Probably owing to a slight difference in the position of the electrodes, the secretion was much faster during the second stimulation.
II.	12.8	8·5	3	4 c.c. in 3 min.	1·333	1·333	1·433	·628	Secretion continuous.
III.	12.43	17 16	$7\frac{1}{2}$	·7 c.c. in $3\frac{1}{2}$ min. 4·8 c.c. in 4 min.	·200 1·200	·733	1·214	·651	Secretion slow with $c=17$, alter to $c=16$ secretion becomes rapid, towards end of time less rapid.
IV.	1.0	8·5	7	1·8 c.c. in 2 min. 2·5 c.c. in 5 min.	·900 ·500	·614	·738	·553	Secretion decreases in rate after first two minutes.
V.	1.20	16—15	14	3·6 c.c. in 9 min.	·400	·400	·707	·472	Chorda stimulated three times, with intervals of rest.
VI.	1.47	16	$6\frac{1}{2}$	3·8 c.c. in 5 min.	·760	·760	·584	·599	Chorda stimulated five times, with intervals of rest.

If we take here the mean rate of secretion, we see that there are several divergences from HEIDENHAIN's law ; of these I. and VI. can be explained by taking into consideration the variation in the rate of secretion during the time of collecting I. In I. the 4 c.c. of the saliva collected is secreted at a rate of ·9 c.c. a minute, and 1 c.c. at a rate of ·5 c.c. a minute. In V. the percentage of salt is ·472 in saliva secreted at a rate of ·400 c.c. a minute, so that, allowing an increase of ·004 per cent. for each ·01 c.c. increase in rate, the percentage of salts in saliva secreted at a rate of ·5 c.c. a minute would be ·512. Hence, the saliva in I., secreted at a rate of ·9 c.c. a minute, contains ·616 per cent. of salts, *i.e.*, a higher percentage of salts than sample VI.

On the other hand, the difference in the percentage of salts between II. and III. cannot be altogether satisfactorily explained in this manner. The saliva III., secreted at a slower rate than II., should have a lower percentage of salts; in fact, the percentage of salts in it is higher. It is true that a portion of III. may have been secreted at a faster rate than any in II., for in III. 4·8 c.c. were secreted at a mean rate of 1·2 c.c., and, as the rate of secretion slackened towards the end of stimulation, the rate was, of course, considerably faster than 1·2 c.c. a minute at the beginning of the stimulation ; but, in view of the slight increase in the percentage of salts which occurs as the rate of saliva approaches its maximum (*cf.* p. 117), this explanation is insufficient. It is possible that the increased percentage of salts may have been due to the blood-flow through the gland being in this case less than normal (*cf.* p. 126).

In the experiment given above, the stimulation of the chorda tympani was, in most cases, stopped as soon as the secretion, beginning fairly rapidly, began to be obviously slower. In the following experiment, a variation of this procedure was made by stimulating the chorda for a definite short portion of each minute, so that the stimulus ceased at about the period of the maximum rate of secretion for the stimulus used. With both methods, there is usually a small amount of saliva secreted slowly, after the stimulation of the nerve has ceased.

Experiment 2.

August 2, 1887. Small Dog. Sympathetic in neck cut. Saliva obtained by stimulating the chorda tympani for 30 sec. in each minute.

		Position of secondary coil.	Duration of chorda stimulation in minutes.	Amount of secretion in c.c.	Rate of secretion per min. in c.c.	Percentage of organic substances.	Percentage of salts.	Amount* of organic substance secreted in 100 minutes in grams.	Amount of salts secreted in 100 minutes in grams.	Number of drops of saliva secreted in each minute.
I.	12.52–1.6	12·0 for 5 min. 11·5 for 9 min.	7	3·4	·486	·616	·633	·2994	·3076	$1\frac{1}{4}$. 4. 3. 1. 5. 4. $6\frac{3}{4}$. $3\frac{1}{2}$. 8. $7\frac{1}{2}$. 7. 5. $3\frac{1}{2}$. 5.
II.	1.14–1.40	6·5	13	3·1	·238	·510	·562	·1214	·1338	8. 6. 4. 4. $3\frac{3}{4}$. 3. 3. 3. $2\frac{3}{4}$. $2\frac{1}{4}$. $2\frac{3}{4}$. $1\frac{1}{2}$. $1\frac{3}{4}$. 2. $1\frac{1}{2}$. 2. $1\frac{1}{4}$. $1\frac{3}{4}$. $1\frac{1}{2}$. $1\frac{1}{4}$. 1. 1. 1. 1. 1. 1.
III.	2.22–2.35	11·5	$6\frac{1}{2}$	3·4	·525	·473	·652	·2480	·3423	4. 4. 3. 6. 6. 7. 4. 5. 4. 5. 5. 5. $4\frac{1}{2}$.
IV.	2.39–2.51	8·5	6	2·4	·400	·414	·598	·1655	·2390	5. 4. 4. 4. 4. 4. 3. 3. 4. 2. 3. 2.
V.	3.5 –3.16	11·5	$5\frac{1}{2}$	2·5	·455	·355	·626	·1615	·2848	5. 6. 6. 6. 4. 5. 3. 3. 3. 2. 5.
VI.	3.24–3.45	5·5	$10\frac{1}{2}$	2·1	·200	·210	·447	·0420	·0894	Since, with the strength of induced shocks used, the secretion stopped in about 10 sec., the chorda was stimulated in alternate 10 sec. The amount of secretion in 10 sec. varied from $\frac{1}{2}$ to 2 drops, being usually 1 drop.

* This is, of course, calculated from the rate of secretion of saliva, and the percentage of organic substance in it.

In this experiment, the percentage of salts follows HEIDENHAIN'S law in all five cases, and does so *although the slower secretions were obtained by stimuli so strong as to rapidly exhaust the irritability of the nerve, instead of by weak stimuli.* This, taken with Experiment 1, in which, out of six cases, there was but one exception to the law, and that a not very certain one, is strong confirmation of HEIDENHAIN'S suggestion that the exceptions found in his experiments are due to variations in the rate of flow of saliva during the time of collecting any one sample.

In all these cases, the saliva is obtained by stimulating the chorda tympani, under normal conditions of blood supply, except that curari or an anæsthetic in sufficient, but not in excessive, amount may have been given; as we shall see later, under other conditions the percentage of salts does not necessarily increase with the rate of secretion of the saliva. Before considering what these conditions are, we may say a word or two about the relation between the increase of flow of saliva and the rate of increase of the percentage of the salts.

HEIDENHAIN (*op. cit.*, p. 9) states that, with increasing rate of secretion, the percentage of salts increases up to a maximum of ·5 to ·6 per cent., so that his law really only holds within certain limits; when the rate of secretion passes a certain limit—variable in different glands—the percentage of salts in the saliva no longer increases. WERTHER points out that the highest percentage of salts given by HEIDENHAIN for sub-maxillary saliva is ·66 per cent., whilst in his own experiments the maximum is ·77 per cent. In our experiments the maximum percentage of salts is also ·77 per cent. (*cf.* p. 122, Table VI.). LUDWIG and BECHER in one case found ·78 per cent. of salts.

We do not think that there is any satisfactory proof that under normal conditions of blood flow, and with saliva obtained by stimulating the chorda tympani, there is any upper limit in the rate of secretion beyond which an increase in rate no longer produces an increase in the percentage of salts.

In Experiment 2, three of the samples of saliva are secreted at a fairly constant rate, and under similar conditions are:—

TABLE I.

	Rate of secretion per minute in c.c.	Percentage of salts.
IV.	·400	·598
V.	·455	·626
III.	·525	·652

Comparing IV. and V., we see that an increase in rate of ·055 c.c. a minute gives an increase in the percentage of salts of ·028 c.c.; the rate of increase is about ·0051 per cent. of salts for ·01 c.c. a minute of saliva.

Comparing V. and III., we see that an increase in rate of secretion of ·089 c.c. a minute gives an increase in the percentage of salts of ·026 per cent., *i.e.*, the rate of increase is about ·0037 per cent. of salts for ·01 c.c. a minute of saliva.

Here the first increase in the rate of secretion produces a greater proportional increase in the percentage of salts than does the subsequent additional increase in the rate of secretion.

Taking similarly from Experiment 1 the three samples of saliva which were secreted with the least variation in rate during the collection of each sample, we have :—

Table II.

	Rate of secretion per minute in c.c.	Percentage of salts.	Increase in percentage of salts corresponding to an increase of ·01 c c. per minute in rate of secretion.
V.	·400	·472	
			·0035
VI.	·760	·599	
			·0005
III.	1·333	·628	

or inserting the calculated rates from I. and IV. :—

Table III.

	Rate of secretion per minute in c.c.	Percentage of salts.	Increase in percentage of salts corresponding to an increase of ·01 c.c. per minute in rate of secretion.
V.	·400	·472	
			·004
I_a.	·500	·512	
			·0033
VI.	·760	·599	
			·0012
I_b.	·900	·616	
			·0003
III.	1·333	·628	

We conclude then that the percentage of salts in saliva increases as long as the rate of secretion increases, but that the increment in the percentage of salts becomes less with each successive equal increment in the rate of secretion.

We may now pass to consider the conditions under which the statement just made no longer holds.

Effect of Stimulating the Sympathetic Nerve.

As far as we know, no attention has been called to the fact that the percentage of salts in sympathetic saliva is greater than that which corresponds to its rate of secretion, if chorda saliva be taken as a basis of comparison.

The following experiments bring out clearly the lack of correspondence between the rate of flow and the percentage of salts, when sympathetic and chorda saliva are compared :—

Experiment 3.

August 5, 1887.—Dog. Weight, $5\frac{1}{4}$ kilos. Cannula in right sub-maxillary duct. Cannula for injection in left jugular vein.

2.15. Stimulate chorda, $c = 19$, no secretion ; $c = 18$, fairly copious viscid secretion.

2.25. Stimulate sympathetic, $c = 8$, very slow secretion.

$2.47\frac{1}{2}$. Inject 3 mgrm. pilocarpin. Let 25 drops run away, then Collect I. The drops in successive 30 sec. were

$$4.4.4.4.5.4.4.4.4.3.4.3.3. = 2{\cdot}6 \text{ c.c. in } 6\tfrac{1}{2} \text{ min.}$$

$3.1\frac{1}{2}$. Stimulate sympathetic, $c = 8$ for 3 min. The secretion stops for 5 min. and then begins again, going on at a rate of 1 to 2 drops in 30 sec.

3.11. Inject 3 mgrm. pilocarpin.

3.12. Collect II. Drops in each 30 sec. were

$$\tfrac{1}{2}.\tfrac{1}{2}.\tfrac{1}{2}.\tfrac{1}{2}.1.1.2.1.2.2.2.\underset{c=8}{\underline{\underline{2}}}.0.0.0.\underline{1}.\tfrac{1}{2}.\tfrac{1}{2}.\underline{3}.3.\underset{c=10}{\underline{\underline{4}}}.\underline{1}.\tfrac{1}{2}.\underline{1\tfrac{1}{2}}.\underline{2}.2.2.2.\underset{c=15}{\underline{\underline{3}}}.0.\underline{1}.3.\underline{\underline{3}}.$$
$$\underline{1}.\underline{2}.2 = 2{\cdot}8 \text{ c.c. in 18 min., the actual time of secretion being 16 min.}$$

During the secretion of the drops doubly underlined, the sympathetic was stimulated. During the secretion of the drops singly underlined, the chorda was stimulated for a part of the 30 sec. ; this was done chiefly to prevent the complete cessation of the secretion, which the previous trial had shown to be the result of strong stimulation of the sympathetic.

3.30. III. Immediately after collecting II., the collection of saliva in III. was begun; thus the first part of this was saliva secreted previously and of the same nature as that in II. Drops in each 30 sec. were

$$2.1.1.1.2.3.2.2.3.2.3.2.2.3.2.2.2.2.3.2.3.2.2.3.2 = 2{\cdot}6 \text{ c.c. in } 12\tfrac{1}{2} \text{ min.}$$

$3.42\frac{1}{2}$. IV. Saliva collected immediately after the end of the previous collection, so that this saliva contained part of saliva secreted under conditions of III., *i.e.*, from pilocarpin alone. The sympathetic, during the secretion of the underlined drops in the following, was stimulated with *weak* induction shocks, $c = 25$ to 30 for 15 to 25 sec. The chorda (at beginning of experiment) first gave a secretion with $c = 18$.

$$2.\underset{c=20}{\underline{3}}.0.\underline{1}.0.\underline{0}.0.\underset{c=25}{\underline{1}}.\tfrac{1}{2}.\tfrac{1}{2}.1.2.\underset{c=25}{\underline{2}}.2.\underline{3}.1.\tfrac{1}{2}.\tfrac{1}{2}.1.2.\underline{3}.\tfrac{1}{2}.\tfrac{1}{2}.\underline{2}.\tfrac{1}{2}.\tfrac{1}{2}.1.2.\underset{c=30}{\underline{2}}.2.\underline{2}.2.$$
$$\underset{c=27}{\underline{2}}.1.1.\underline{2}.1.1.\underline{2}.1.1.\underline{1}.\underline{1}.\underline{1}.1.1.1.2 = 3 \text{ c.c. in 24 min.; actual time of secretion 22 min.}$$

4.9. After about 6 drops had been allowed to run away, the saliva was again collected, the chorda being stimulated in alternate periods of 30 sec. for 25 to 30 sec., $c = 16$. The first stimulation was for 15 sec. only. The number of drops secreted in each 30 sec. were

$2 \, . \, \frac{3}{4} \, . \, 3\frac{1}{4} \, . \, 1 \, . \, 3 \, . \, 1 \, . \, 4 \, . \, 1 \, . \, 4 \, . \, 1 \, . \, 4 \, . \, 1 \, . \, 4 \, . \, 1 \, . \, 3 \, . \, 1 \, . \, 4 \, . \, 1 \, . \, 4 \, . \, 1 \, . \, 4 \, . \, 1 \, . \, 3 = 2\cdot8$ c.c. in $11\frac{1}{2}$ min.

	Time of collecting saliva.	Duration of secretion in min.	Number of c.c. of saliva.	Rate of flow per min. in c.c.	Percentage of organic substance.	Percentage of salts.	Remarks.
I.	2.51 –2.57½	6½	2·6	·400	·324	·728	Saliva obtained by injecting 3 mgrm. of pilocarpin. 3.11. Inject 3 mgrm. pilocarpin.
II.	3.12 –3.30	16	2·8	·175	1·138	·726	Stimulate sympathetic and chorda occasionally.
III.	3.30 –3.42½	12½	2·6	·208	·563	·704	
IV.	3.42½–4.6½	22	3·0	·136	·463	·711	Weak stimulation of sympathetic. $c = 20$ to 30.
V.	4.9 –4.20½	11½	2·8	·243*	·857	·623*	Stimulate chorda. $c = 16$.

Here pilocarpin produces a fairly rapid secretion, the saliva having a high percentage of salts, viz., ·728. Whilst the secretion from pilocarpin is going on but more slowly, the sympathetic is stimulated; the rate of secretion of saliva is reduced from $\frac{1}{2}$ to $\frac{2}{3}$, but the percentage of salts remains nearly the same (·726); the subsequent saliva from pilocarpin alone, although faster, has a less percentage of salts (·704); this is increased by weak stimulation of the sympathetic (·711), although the rate of secretion is considerably decreased, and this is the more noteworthy, since the percentage of organic substance was very little affected by the weak stimulation of the sympathetic. Finally, stimulation of the chorda decreases the percentage of salts, although about doubling the rate of secretion.

The divergence from HEIDENHAIN's law will perhaps be more easily seen, if II.–V. are arranged in order of rate of secretion.

* In V. there is a mixture of salivas secreted at different rates, viz., very nearly 2·2 c.c. at a rate of ·37 c.c. a minute, and ·6 c.c. at a rate of ·11 c.c. a minute.

It is most unlikely, that a difference in rate of ·01 c.c. a minute could cause here a greater difference in percentage of salts than ·01. If we take this as being the true relation, we have

	Rate of secretion per minute in c.c.	Percentage of salts.
V_a.	·37	·679
V_b.	·11	·419

Table V.

	Rate of secretion per minute in c.c.	Percentage of salts.
V.	·370	·679 about*
III.	·208	·704
II.	·175	·726
IV.	·136	·711

It will be observed that the experimental errors are such as to make the contrast less striking, for, by admixture of some saliva II. with saliva III., the percentage of salts in III. is too high; and by admixture of saliva III. with saliva IV., the percentage of salts in IV. is too low.

Experiment 4.

August 10, 1887.—Rather small Dog.

11.45. Stimulate chorda, $c = 16$ to $c = 13$, no secretion; $c = 12$, rapid secretion.

12.5. Collect I. Stimulate chorda for 30 sec. in each minute, $c = 12$. Number of drops in each 30 sec. of stimulation were

$$4 . 4 . 4 . 2 . 7 . 3 . 5 . 5 . 4 = 2{\cdot}7 \text{ c.c.}$$

There was a secretion of about half a drop, lasting about 15 sec. after the end of each stimulation, so that 2·3 c.c. were secreted in the $4\frac{1}{2}$ min. of stimulation, and ·4 c.c. in $2\frac{1}{4}$ min. after the stimulation had ceased.

12.25. Inject 1 c.c. ·1 p.c. atropin sulphate into jugular vein.

12.40. Stimulation of the chorda, $c = 12$ to $c = 3$, gives no secretion or a mere trace. Stimulate sympathetic; 7 drops of saliva thrown away.

1.33. Collect II. Stimulate sympathetic as a rule for 30 sec. in each minute, $c = 8$. There is a slow secretion which usually continues for 15 or more sec. afterwards. In 139 min., the sympathetic being stimulated altogether for 56 min., 2 c.c. of saliva are collected. This gives the rate of secretion as ·036 c.c. per minute; but, as the saliva continued to flow in the intervals of stimulation, the rate was really slower than this.

4.0. Stimulate chorda, secretion slow, 2 to 3 drops a minute; the amount is proportional to the length of the intervals of rest.

4.6. Collect III. Stimulate chorda, $c = 8$ in alternate 15 sec. for $6\frac{1}{2}$ min., then $c = 8$ to $c = 6$ for 15 sec. in each 45 sec. for 21 min. *i.e.*, total time of stimulation was $13\frac{1}{2}$ min. out of 34 min., 2·5 c.c. of saliva were collected; reckoning the time of stimulation only, the rate of secretion is ·185 c.c. a minute. This, of course, is a little too high.

4.47. Inject 1·5 c.c. ·5 p.c. pilocarpin. Twelve drops thrown away.

4.50. Collect IV. There were 4 drops every 30 sec. for 4 min.; then 3 drops every 30 sec. for $4\frac{1}{2}$ min.

4.59. Collect V. Drops in each 30 sec., were

3 . 3 . 3 . 2 . 2 . 2 . 2 . 2 . 2 . 2 . 2 . 1 . 1 . 2 . 1 . 2 . 1 . 1 . 2 . 2 . 1 . 1 . 2 . 1 . 2 . 1 . 1 . 2 . 1 . 1 . 2 . 1.

5.16½. Inject 1·25 c.c. ·5 p.c. pilocarpin. About $3\frac{1}{2}$ c.c. secreted, then stimulate chorda. Ten drops thrown away.

5.32. Collect VI. Stimulate chorda $c = 8$ in alternate 30 sec., very little effect on secretion, increasing, perhaps, the continuous secretion by $\frac{1}{2}$ a drop a minute during the earlier period of collecting; secretion begins 4 and ends $2\frac{1}{2}$ drops a minute.

* *Cf.* note p. 119.

		Total time in min. during which saliva collected.	Time in min. of electrical stimulation.	Amount of saliva collected in c.c.	Rate of secretion per min. in c.c.	Percentage organic substance.	Percentage of salts.	Amount organic substance secreted in 100 min. in grams.	Amount of salts secreted in 100 min. in grams.	Remarks.
I.	12.5	9	$4\frac{1}{2}$	2·3 in $4\frac{1}{2}'$ ·4 in $2\frac{1}{4}'$	·511 ·177	1·338	·616	. .	. .	Stimulate chorda $c = 12$. 12.25. Inject 1 mgrm. atropin, chorda nearly paralysed.
II.	1.33	139	56	2·0	·036	1·335	·455	. .	. .	Stimulate sympathetic.
III.	4.6	34	$13\frac{1}{2}$	2·5	·185	·258	·258	·0477	·0477	Stimulate chorda $c = 8$ to $c = 6$. 4.47. Inject 7·5 mgrm. pilocarpin.
IV.	4.50	$8\frac{1}{2}$	. .	3·2	·376	·098	·358	·0368	·1346	
V.	4.59	17	. .	3·1	·182	·085	·238	·0154	·0432	5.16. Inject 6 mgrm. pilocarpin.
VI.	5.32	$17\frac{1}{2}$	$8\frac{3}{4}$	3·1	·177	·141	·352	·0249	·0623	Stimulate chorda $c = 8$, rate of secretion barely affected.

It will be seen in the above experiment that, notwithstanding the very slow rate of secretion of the sympathetic saliva, it contains a higher percentage of salts than five out of the six other samples of saliva collected. Comparing it with sample IV. we see that, whilst it was secreted at one-tenth the rate, it contains ·1 per cent. more salts.

In the other samples of saliva, with the exception of VI., the salts follow HEIDENHAIN's law. The exception we shall have occasion to refer to later (p. 148).

A still more striking instance of the high percentage of salts which may be present in sympathetic saliva, compared with that proper to its rate of secretion, is given in another experiment made by us, the details of which have been already published.* From this we take the following:—

TABLE VI.

Saliva obtained by—	Rate of secretion per minute in c.c.	Percentage of salts.
Stimulating left chorda before atropin given	4·13	·742
Stimulating left chorda and sympathetic after 15 mgrm. atropin	·073	·619
Stimulating right chorda before atropin given	4·200	·766
Stimulating right sympathetic after 15 mgrm. atropin	·023	·705

Here, on the right side, the rate of secretion of the chorda saliva is about 180 times that of the sympathetic saliva, but it contains only ·056 per cent. more salts.

It will be noticed that, on the left side, the sympathetic saliva has ·123 per cent. less salts than the chorda saliva; whilst, on the right side, the sympathetic saliva has only ·061 per cent. less salts than the chorda saliva, *i.e.*, the fall in the percentage of salts is greater on the side on which the sympathetic saliva is more rapidly secreted. It is, however, just possible that this might have been due to a slight admixture of chorda saliva.

The chief point, however, with which we are concerned is that, when chorda saliva is taken as a standard, the percentage of salts in sympathetic saliva is much higher than that which corresponds to its rate of secretion.

In view of this fact, and of the fact that stimulating the sympathetic nerve causes a very great diminution in the amount of blood flowing through the gland, and in consequence a very great diminution in the supply of oxygen to the secretory cells, the possibility was suggested that if, in other ways, either the blood flow through the gland or the oxygen in the blood were diminished, the percentage of salts in saliva might be increased. We accordingly made some observations on these points.

* LANGLEY, 'Journal of Physiology,' vol. 9, 1888, p. 59.

Effect of Dyspnœa.

*Experiment 5a.**

Jan. 16, 1888. Dog, weight 29 kilos. Sympathetic nerve uncut. The saliva in each case was obtained by stimulating the chorda, $c = 14$. The saliva was secreted fairly rapidly, so that the requisite amount was obtained with a single stimulation.

		Time during which saliva collected in minutes.	Number of c.c. of saliva.	Rate of secretion per min. in c.c.	Percentage of organic substance.	Percentage of salts.	Amount of organic substance in 100 minutes in grams.	Amount of salts in 100 minutes in grams.	Remarks.
I.	12.37	$1\frac{1}{2}$	2·7	1·80	1·203	·486	2·1654	·8748	
II.	12.41	$1\frac{1}{2}$	2·2	1·47	1·193	·515	1·7537	·7570	12.39.30 Clamp trachea tubes. 12.40.10 Stimulate chorda, saliva not collected. 12.41.0 Begin collecting saliva II. 12.42.30 End of collection of saliva and of stimulation. Unclamp trachea tubes.
III.	12.49	$1\frac{1}{2}$	2·6	1·73	1·174	·487	2·0310	·8425	
IV.	12.55	$1\frac{1}{2}$	2·4	1·60	·969	·558	1·5504	·8928	Dyspnœa produced as in II.; no saliva was thrown away between collecting IV. and V.
V.	$12.56\frac{1}{2}$	2	1·6	·80	·942	·390	·7536	·3120	
VI.	1.10	$1\frac{1}{2}$	2·1	1·40	·854	·480	1·1956	·6720	Secretion, rapid at first, became slow towards end.

* The remainder of this experiment is given on p. 135.

In this experiment dyspnœa reduces the rate of secretion of water, but, except when it is prolonged (V.), does not do so to any great degree.

Its chief effect is on the secretion of salts; in II. and in IV., although the rate of secretion of saliva falls, the percentage of salts rises. The *rate* of secretion of salts is diminished in II., but is increased in IV.; in the latter case the rate of secretion of water is very slightly diminished, so that the increase in the rate of secretion of salts may have been due to an increase in the strength of the stimulus caused by a shifting of the electrodes on the chorda tympani. At any rate, when the rate of secretion of water is diminished to a greater extent, as in II. and as in Experiment 6, the rate of secretion of salts falls.

The rate of secretion of organic substance also falls; that this is not solely due to a progressive exhaustion of the gland is indicated by the rate of secretion of organic substance being greater in III. than in II. The percentage of organic substance falls in each successive sample of saliva secreted; with a constant stimulus this is observed normally. But here the drop in the percentage of organic substance does not take place regularly; the more dyspnœa decreases the rate of secretion of water, the less is the drop in the percentage of organic substance, that is to say, dyspnœa decreases the rate of secretion of water more than it decreases the rate of secretion of organic substance.

Experiment 6.

Jan. 21, 1888.—Dog. Chloroform. (Morphia was not given in this case.) Trachea connected with bottle of chloroform and ether. Stimulate chorda, $c = 11$, secretion very slight; repeat, $c = 7$, fairly rapid secretion, obtain about 1 c.c. Whilst collecting saliva for analysis, the secondary coil was in all cases at 7.

12.0. I. Stimulate chorda 2 min. 25 sec. Obtain 2 c.c. saliva.

12.9½. Empty the cannula. Clamp trachea tube, to produce dyspnœa.

12.10. Stimulate chorda for 2 min. Cannula full ($= \frac{1}{3}$ c.c.), and 8 drops saliva; drops not collected.

12.12. II. Stimulate chorda 1½ min.; saliva collected; at end of stimulation unclamp trachea tube.

12.17. Clamp trachea tube.

12.17½. Stimulate chorda 2 min.; saliva collected; at end of stimulation unclamp trachea tube.

12.25. Clamp trachea tube.

12.25½. Stimulate chorda 2½ min.; saliva collected; unclamp trachea tube. Add contents cannula to saliva collected. Total amount saliva is 2·5 c.c. The total time of secretion is 6 min. + 1 min. (required to fill the cannula). Stimulate chorda; let 11 drops run away.

12.36 to 12.47. III. Stimulate chorda 2½ min., 2½ min. (secretion is slow towards end of these periods of stimulation) and 45 sec. Total time of secretion is 5¾ min. Saliva collected = 2 c.c.

12.53 to 1.58. Stimulate chorda occasionally; let about 50 drops saliva run away. Vago-sympathetic in neck cut.

2.0 to 2.11½.	IV. Stimulate chorda 1½, 1½, 1½, and ½ min., that is for 5 min.; there were considerable variations in the rate of secretion. Saliva collected = 2·6 c.c.
2.14 to 2.22.	Clamp trachea tubes and stimulate chorda (3 times) to obtain saliva during dyspnœa, as in collecting (II.). Saliva not collected.
2.24	V. Clamp trachea tube.
2.24½.	Stimulate chorda for 1½ min., then unclamp trachea tube.
2.30 2.50.	Above repeated four times. Chorda stimulated 1½, 1, 1¼, and 1½ min. Total time secretion 6¾ min. Saliva collected 3 c.c.
2.51 to 3.9.	Chorda occasionally stimulated; 21 drops thrown away.
3.13 to 3.30.	VI. Stimulate chorda 1½, 1½, 1½, and ½ min., = 5 min. Saliva collected = 3·2 c.c.
	Trachea tubes clamped for 2 min.; no spontaneous secretion.

	Time of beginning stimulation chorda $c=7$.	Number c.c. of saliva collected.	Rate of flow of saliva per minute in c.c.	Percentage organic substance.	Percentage of salts.	Remarks.
I.	12.0	2·0	·83	1·827	·654	
II.	12.12	2·5	·356	2·102	·657	Dyspnœa.
III.	12.36	2·0	·348	1·669	·556	
IV.	2.0	2·6	·52	·784	·515	
V.	2.24	3·0	·44	·760	·651	Dyspnœa.
VI.	3.13	3·2	·64	·627	·522	

The rates of flow given for II. to VI. are all a little too high, since the duration of stimulation is here taken as the duration of secretion of saliva. There was usually a secretion of something less than a drop of saliva after the end of each stimulation.

In this experiment dyspnœa produces more marked effects; it reduces the rate of secretion of all the constituents of saliva, and presumably by decreasing the irritability of the gland-cells. But its effect on the various constituents of saliva is unequal; it reduces the rate of secretion of water most, and in consequence the percentage composition of the saliva is altered.

Saliva II., collected during dyspnœa, is secreted at less than half the rate of saliva I., but it has nevertheless an equal percentage of salts, and about ·1 per cent. more salts than saliva III., secreted at nearly the same rate. So, also, saliva V., collected during dyspnœa, although secreted distinctly more slowly than salivas IV. and VI., before and after it, has ·13 to ·14 per cent. more salts.

With regard to the percentage of organic substance, it is in the first case of dyspnœa increased, and in the second decreased, but less than it would otherwise have been.

An example of the effect of dyspnœa, when the saliva is obtained by injecting pilocarpin, is given in Experiment 7.

TABLE VII.

	Rate of secretion per minute, in c.c.	Percentage of organic substance.	Percentage of salts.	Saliva obtained by
VI.	·360	1·547	·529	Pilocarpin and stimulating chorda.
VII.	·250	·446	·474	Pilocarpin.
VIII.	·311	·517	·557	More pilocarpin during dyspnœa.

There is here an increase in the percentage of salts during dyspnœa, but the results are complicated by the earlier procedure in the experiment.

Dyspnœa appears also to have an after-effect, tending to increase the percentage of salts, and possibly also of organic substance, in the saliva subsequently secreted; but this after-effect is not great and soon disappears.

The prominent effect of not too prolonged dyspnœa is that, whilst decreasing the rate of secretion of saliva, it increases the percentage of salts, and tends to increase the percentage of organic substance in the saliva.

EFFECT OF CLAMPING THE CAROTID.

When one carotid is clamped, the blood flow through the sub-maxillary gland is, as is well known, not stopped, but simply diminished, the degree of diminution varying in different cases. We have tried the effect of clamping the carotid on the composition of saliva in a few cases only; the effect, however, is marked: *clamping the carotid increases the percentage of salts in saliva both during the period of clamping and for a short time afterwards.*

Experiment 7.

July 5, 1888.—Dog. Weight, $11\frac{1}{4}$ kilos.

		Time in min. during which saliva collected.	Number of c.c. of saliva collected.	Rate of secretion in c.c. per min.	Percentage of organic substance.	Percentage of salts.	Remarks.
I.	12.29	2	3·1	1·550	1·239	·499	Saliva obtained by injecting into vein 4 mgrm. of pilocarpin.
II.	12.33	5	2·5	·500	·540	·512	Carotid clamped; decreases at once the rate of secretion.
III.	$12.44\frac{1}{2}$	14	1·8	·128	·322	·674	
IV.	12.59	3	2·8	·933	·837	·675	12.58. Inject 2 mgrm. pilocarpin. Carotid clamped.
V.	$1·7\frac{1}{2}$	7	2·5	·357	1·478	·622	1.5. Inject 2 mgrm. pilocarpin.
VI.	$1.18\frac{1}{2}$	5	1·8	·360	1·547	·529	Chorda stimulated.
VII.	1·22	8	2·0	·250	·446	·474	
VIII.	1.50	9	2·8	·311	·517	·557	Inject 10 mgrm. curari, and 3 mgrm. pilocarpin. Dyspnœa by stopping artificial respiration, 3 times.
IX.	$2.0\frac{1}{2}$	15	3·1	·206	·819	·625	Dyspnœa 4 times. Carotid clamped. 2.28. Inject 4 mgrm. pilocarpin. 2.30. Unclamp carotid.
X.	2.35	16	2·0	·125	·746	·442	
XI.	2.56	13	2·5	·192	1·120	·476	2.54. Inject 8 mgrm. pilocarpin.
XII.	$3.17\frac{1}{2}$	19	2·2	·116	·984	·424	3.9 Inject 15 mgrm. pilocarpin.

It will be seen on comparing I. and II. that clamping the carotid increases the percentage of salts from ·499 to ·512, although the rate of secretion falls from 1·55 c.c. to ·50 c.c. a minute; so also in IX., clamping the carotid during dyspnœa increases the percentage of salts considerably more than does dyspnœa alone (VIII.).

It will be seen also that clamping the carotid has a very great after-effect; this is, in fact, in III. and V. greater than the effect whilst the carotid is clamped; thus in III., collected after removing the clamp from the carotid, the percentage of salts rises from ·512 to ·674, whilst the rate of secretion falls from ·5 c.c. a minute to ·128. That the after-effect is slight on clamping for the third time may have been due to the much longer duration—half-an-hour—of the closure of the carotid.

Clamping the carotid increases also somewhat the percentage of organic substance above that which corresponds to the rate of secretion of saliva (*cf.* VIII. and IX.).

A similar result was obtained in Experiment 8.

Table VIII.

Secretion obtained by injecting pilocarpin.

	Rate of secretion per minute in c.c.	Percentage of salts.	Remarks.
I.	·675	·559	
II.	·250	·420	
VII.	·333	·660	Dyspnœa, carotid clamped.
VIII.	·417	·572	Collected 8 min. after unclamping carotid.

The Effect of Loss of Blood.

The details with regard to this are given on p. 130, together with the effect of injecting dilute salt solution into the blood. It will be seen that bleeding decreases the rate of secretion and increases the percentage of organic substance in the saliva. The rate of secretion of salts falls, but its percentage is greater than that which corresponds to the rate of secretion of the saliva.

The Effect of injecting dilute Salt Solution into the Blood.

When dilute salt solution is injected into the blood, the percentage composition of the blood is, of course, altered. To the eye, the tissues, especially the abdominal viscera, become more flushed, and the veins fuller. According to Worm-Müller and others, increasing the volume of the blood 20 to 50 per cent. by transfusion causes no increase of arterial blood pressure, except for a brief time immediately after the injection, since the small arteries dilate and all the capillary areas become fuller. We have made no direct observations upon the effect of injecting dilute solutions of sodium chloride into the blood on the circulation through the sub-maxillary gland; in our experiments the injection has been followed by increased vigour of heart beats; since at the same time the amount of blood in the body was increased, we conclude that, both during rest and during secretion, more blood flows through the gland than normal, and that the capillary blood pressure is increased.

We find that *injection of dilute salt solution in moderate quantity increases the rate of secretion of saliva with a given stimulus, the percentage of salts in the saliva rising nearly normally; and that injection of dilute salt solution in larger quantity increases further the rate of secretion with a given stimulus, but in this saliva the percentage of salts rises much less than normally, and may even fall.*

Experiment 8.

July 6, 1888. Dog. Weight $8\frac{1}{4}$ kilos. The pilocarpin nitrate solution is injected into a branch of the right crural vein, the sodium chloride solution into a branch of the left crural vein. Vago-sympathetic and chorda cut.

		Time during which saliva collected in min.	Number of c.c. of saliva.	Rate of secretion per min. in c.c.	Percentage of organic substance.	Percentage of salts.	Remarks.
I.	11.55	4	2·7	·675	·250	·559	11.52. Inject 3 mgrm. pilocarpin. Rate of secretion falls from 8 to 2 drops a minute.
II.	12.0	10	2·5	·250	·221	·420	12.10–12.20. Inject 200 c.c. NaCl ·2 per cent.; little, if any, effect on rate of secretion. 12.21. Inject 3 mgrm. pilocarpin.
III.	12.22	2	3·3	1·650	·294	·752	
IV.	12.25	3	3·0	1·000	·203	·607	12.28–12.32. Inject 100 c.c. NaCl ·2 per cent.; no appreciable effect on rate of secretion.
V.	12.33	18	2·9	·161	·264	·467	Secretion falls from 5 to 1 drop a minute in 10 min., then inject 200 c.c. NaCl ·2 per cent.; secretion quickens to 3 drops a minute. 12.51. Inject 3 mgrm. pilocarpin. Urine is brightish red, from hæmoglobin.
VI.	12.53	3	2·6	·867	·598	·621	
VII.	12.57	9	3·0	·333	·506	·660	Dyspnœa. Clamp trachea tubes three times, altogether for 6 min.; carotid clamped throughout. 1.10. Inject 3 mgrm. pilocarpin; causes a slight increase only in rate of secretion.
VIII.	1.14	6	2·5	·417	·389	·572	1.20. Let 160 c.c. blood flow from crural artery.* Inject 3 mgrm. pilocarpin; little, if any, increase in rate of secretion.
IX.	1.22	13	2·8	·215	·905	·566	Secretion equal rate throughout. 1.36–1.38. Inject 150 c.c. NaCl ·2 per cent.
X.	1.38	12	2·8	·233	·464	·502	1.53. Let 200 c.c. blood flow from crural artery. Inject 3 mgrm. pilocarpin; secretion continues to fall in rate.
XI.	1.56	24	2·8	·117	·939	·457	2.21. Inject 350 c.c. NaCl ·2 per cent.; secretion becomes somewhat faster.
XII.	2.28	12	2·6	·217	·374	·436	

* After each withdrawal of blood the heart-beat could be but feebly felt through the chest-wall; on injecting salt solution the heart-beat was strongly felt.

Here neither the first injection of 200 c.c. NaCl ·2 per cent. into the blood, nor the second of 100 c.c., affected appreciably the slow rate of secretion going on at the time, owing to the previous dose of pilocarpin; but the secretion obtained after the first injection of NaCl solution, by giving more pilocarpin (III. and IV.) was unusually rapid in rate; and it seems to us certain that this was due to the salt solution leading to an increased flow of blood through the gland, during the dilatation of the small arteries brought about by the additional dose of pilocarpin. At the same time, it is possible that after injection of salt solution more pilocarpin passes through the gland in a given time, and so helps to increase the rate of secretion. The percentage of salts increased in III. nearly as much as it would normally have done.

There is some difficulty with regard to the percentage of salts in IV., V. In IV. the percentage of salts decreases more than normal; in V. it decreases less than normal. It is possible that the former was due to a more marked action of the salt solution, and that the latter was due to the breaking up of the red blood corpuscles, which about this time gave rise to hæmoglobinuria; possibly, also, from the same cause, the percentage of salts in VI. was rather higher than normal, taking I. as a standard.

The third injection of ·2 per cent. NaCl—200 c.c.—trebles the rate of secretion which was slowly going on owing to the previous injection of pilocarpin. The subsequent saliva (VI.) obtained by injecting more pilocarpin is rapid, and, judging from other experiments, more rapid than it would have been but for the injection of the salt solution.

In the latter part of the experiment, the effect of injecting salt solution is much more obvious; it increases the rate of secretion, and decreases the percentage of organic substance and of salts. Taking the samples VIII. to XII. we have—

Table IX.

	Rate of secretion per min. in c.c.	Percentage organic substance.	Percentage of salts.	Amount in grams of organic substance secreted in 100 min.	Amount in grams of salts secreted in 100 min.	Remarks.
VIII.	·417	·389	·572	·162	·239	3 mgrm. pilocarpin injected.
IX.	·215	·905	·566	·195	·122	After withdrawing 160 c.c. blood, and injecting 3 mgrm. pilocarpin.
X.	·233	·464	·502	·108	·177	After injecting 150 c.c. NaCl ·2 p.c.
XI.	·117	·939	·457	·110	·053	After withdrawing 200 c.c. blood, and injecting 3 mgrm. pilocarpin.
XII.	·217	·374	·436	·081	·095	After injecting 350 c.c. NaCl ·2 p.c.

In X. not only does the percentage of salts decrease with an increased rate of secretion of water, but the actual rate of secretion of salts decreases slightly. The latter is no doubt due to the stimulus being weaker in X. than in IX., owing to a partial elimination of the pilocarpin. In the other cases the rate of secretion of salts increases with the rate of secretion of water.

The *rate* of secretion of organic substance is not much affected by the injection of dilute salt solution. This is clearest at the end of the experiment, when the salt solution is injected after bleeding. The injection, diminishing considerably the percentage of organic substance in the saliva, diminishes somewhat the amount secreted in a given time, but not more than we should expect from the decrease in the strength of the stimulus.

This, taken with what has been said above on the secretion of water and of salts, indicates that *the secretion of organic substance depends wholly, or almost wholly, upon the strength of the stimulus, whilst the secretion of water and of salts depends also upon the amount of blood flowing through the gland.* The same result, although less conspicuously, follows the first injection ; the percentage of organic substance in III. and IV. is much less than corresponds to the rate of secretion ; the amount of organic substance secreted in a given time is greater than in I. and II., since more pilocarpin was injected, and thus the stimulus stronger.

Experiment 9.

August 12, 1887. Dog. Weight 15 kilos. Vago-sympathetic cut.

		Number of c.c. of saliva collected.	Rate of secretion per min. in c.c.	Percentage of organic substance.	Percentage of salts.	Amount of organic substance in 100 min. in grams.	Amount of salts in 100 min. in grams.	Remarks.
I.	10.36–10.40	3·0	·750	·479	·413	·359	·310	10.34. Inject 5 mgrm. pilocarpin into jugular vein.
II.	10.42–10.51	3·2	·355	·416	·401	·148	·142	
III.	10.55–10.59	3·3	·825	1·570	·545	1·295	·450	Chorda stimulated, $c = 5$.
								11.4. Inject 5 mgrm. pilocarpin.
IV.	11.6–11.11	3·5	·700	·865	·525	·606	·368	
V.	11.11–11.21	4·9	·490	·672	·515	·329	·252	
								11.25. Inject about 1 mgrm. atropin sulphate; secretion stops.
								11.55. Inject 10 mgrm. pilocarpin; no secretion, nor on stimulating chorda.
VI.	12.11–1.1	3·1	·124*	·619	·284	·077	·035	Chorda stimulated 30 sec. in each minute, $c = 0$.
								1.15–1.24. Inject 15 mgrm. pilocarpin.
VII.	1.31–2.13	3·6	·086	·148	·177	·013	·015	
								2.20–2.28. Inject 560 c.c. NaCl ·6 p.c. into jugular; drops increase from $1\frac{1}{2}$ to $3\frac{1}{2}$ a minute.
VIII.	2.31–2.46	3·1	·207	·084	·134	·017	·028	
IX.	2.51–2.59$\frac{1}{2}$	{3·36 in $4\frac{1}{2}$ min. {·94 in 4 min.	·747 ·235	·181	·209	..	..	{Chorda stimulated 9 times for 30 sec., with intervals of 30 sec., $c = 0$.
X.	3.6–3.17	3·6	·327	·061	·153	·020	·050	

* This rate of secretion is too high, since the secretion usually continued in the intervals of non-stimulation; on the other hand, stimulation gave occasionally a mere trace of saliva; the maximum number of drops in 30 sec. was 2.

In this experiment the injection of normal salt solution into the blood very considerably increases the rate of secretion of saliva, and this increase does not lead to any corresponding increase in the secretion of salts. Comparing salivas VII. and VIII., we see that, although in VIII. the secretion of saliva is more than twice as fast as in VII., yet it contains a *less* percentage of salts. And all three samples of saliva obtained after injection of normal salt solution have a much less percentage of salts than corresponds to their rate of secretion; this is readily seen when these samples are compared with those similarly obtained, but before injection, as in the following Table:—

TABLE X.

Number of sample.	Manner of producing secretion.		Rate of secretion.	Percentage of salts.
VI.	Stimulate chorda after pilocarpin given	— *before* injecting NaCl ·6 .	·110 about	·2844
IX.		— *after* injecting NaCl ·6 .	·700 about	·2092
II.	Pilocarpin	— *before* injecting NaCl ·6 .	·355	·4008
X.		— *after* injecting NaCl ·6 .	·327	·1528
VII.	Pilocarpin	— *before* injecting NaCl ·6 .	·086	·1772
VIII.		— *after* injecting NaCl ·6 .	·206	·1344

At the same time, when the three samples of saliva obtained after the injection of salt solution are compared together, instead of with those obtained earlier, it is found that the percentage of salts in them follows HEIDENHAIN's law.

Here, as in Experiment 8, the rate of secretion of organic substance does not increase to the extent that it normally would, with the increased rate of secretion of water.

EFFECT OF INJECTING INTO THE BLOOD A 2 PER CENT. SOLUTION OF Na_2CO_3.

We have tried the effect of injecting a 2 per cent. solution of salt into the blood in one experiment only, but the result was quite decisive as regards one point: *the injection considerably increases the rate of secretion obtained by a stimulation of given strength of the chorda tympani.* The experiment was a continuation of 5*a* (*cf.* p. 123). Three samples of saliva were obtained, under normal conditions, by stimulating the chorda, the secondary coil being at 14; the rate of secretion varied from 1·4 to 1·8 c.c. in a minute. Then 250 c.c. of 2 per cent. solution of Na_2CO_3 were injected into the blood; after this the chorda was again stimulated, the rate of secretion was 2·4 to 2·8 c.c. in a minute. Further injection of 500 c.c. of a solution containing 1 per cent. KI and 1 per cent. NaCl still left the rate of secretion, on chorda stimulation, higher than normal.

Experiment 5b.

For first part of the Experiment see 5*a*, p. 123. The saliva was obtained throughout by a single stimulation of the chorda, $c = 14$.

		Time of stimulation in minutes.	Amount of saliva collected in c.c.	Rate of secretion per minute in c.c.	Percentage of organic substance.	Percentage of salts.	Amount of organic substance in 100 minutes.	Amount of salts in 100 minutes.	Remarks.
									1.24–1.33. Inject 250 c.c. 2 p.c. Na_2CO_3 into a branch of the jugular.
VII.	1.34	1	2·8	2·8	·919	·699	2·5732	1·9572	These samples of saliva were obtained by a single stimulation, there being no break in the stimulation between the end of VII. and the beginning of VIII.
VIII.	1.35	$1\frac{1}{2}$	2·2	1·47	·847	·436	1·2451	·6409	
IX.	2.33	$1\frac{1}{4}$	3·0	2·4	·761	·501	1·8264	1·2024	
									2.42–2.51. Inject 100 c.c. KI 1 p.c. + NaCl 1 p.c.
X.	2.53	$1\frac{1}{2}$	3·0	2·0	·772	·551	1·5440	1·1020	Secretion becomes slower towards end.
									3.0–3.7. Inject 200 c.c. KI 1 p.c. + NaCl 1 p.c.
XI.	3.10	$1\frac{1}{2}$	3·2	2·13	·584	·517	1·2439	1·1012	
									3.16–3.21. Inject 200 c.c. KI 1 p.c. + NaCl 1 p.c.
XII.	3.25	$1\frac{1}{2}$	3·2	2·13	·501	·467	1·0671	·9947	

With regard to the effect of the injection of the sodium carbonate on the percentage of salts in the saliva, we have hardly the means of arriving at a well-grounded conclusion. If we take the percentage of salts in I., III., and VI. as a standard, allowing a slight increase in III. and VI. for the after-result of dyspnœa, it appears that the first saliva, VII., collected after injecting sodium carbonate solution, contains a higher percentage of salts than corresponds to its rate of secretion; that the second sample, VIII., collected immediately after VII., contains about a normal percentage; and the third, IX., collected after an interval of an hour, contains rather less than the normal percentage for its rate of secretion. The *rate* of secretion of organic substance, and of salts, as well as that of water, is increased, so that the injection rendered either the nerve or the gland more irritable. The first injection of potassium iodide with sodium chloride increases the percentage of salts; the subsequent two injections decrease the percentage of salts, the rate of secretion in all three being very nearly the same. The decrease in the percentage of salts is no doubt due to the poisonous action of the potassium iodide, but it is not as yet worth while to discuss the matter. We give the results as another proof of what we wish to show, viz., the partial independence between the secretion of water and the secretion of salts in saliva.

Effect of Injecting Strong Salt Solutions into the Blood.

Having found that the injection of dilute salt solution into the blood might lead to a decrease in the percentage of salts, with an increased rate of secretion, we expected that the injection of a strong salt solution would very considerably increase the percentage of salts in the saliva. We find, however, that, whilst injection of strong salt solutions increases the percentage of salts in saliva, the increase is small, considering the amount of salts injected.

Experiment 10.

July 2, 1888. Dog. Weight 6 kilos. Chordo-lingual and vago-sympathetic cut on left side. Pilocarpin solution nominally* ·5 p.c.

		Time of secretion in minutes.	Amount of saliva collected in c.c.	Rate of secretion per minute in c.c.	Percentage of organic substance.	Percentage of salts.	Remarks.
							12.20. Inject 1 c.c. pilocarpin into right jugular.
I.	12.22½	5½	1·6	·292	·347	·590	Secretion begins 6 drops, ends 4 drops, a minute.
II.	12.31	26	2·2	·085	·357	·281	Secretion begins 2 drops, ends 1 drop, a minute. 12.38–1.2. Inject 50 c.c. 20 p.c. NaCl solution into right jugular. 1.4. Inject 1 c.c. pilocarpin.
III.	1.11	12	2·5	·208	·310	·685	Secretion begins 4 drops, ends 2¼ drops, a minute.
IV.	1.27	37	1·4	·038	·608	·352	Secretion begins 1¼ drops, falls slowly to ½ drop, a minute.

1.45. Inject ·5 c.c. pilocarpin; rate of secretion increases to 1 drop a minute, in 5 min. falls to rather less than this.

2.4. Inject 10 c.c. NaCl 20 p.c. Respiration stops, but begins again after artificial respiration. Secretion very slow. Further injection of 3 c.c. of pilocarpin, 1 c.c. at a time, does not quicken the secretion. Injection of 350 c.c. NaCl ·2 p.c. solution causes a slight quickening.

3.5. Stimulation of chorda has very little, if any, effect. After death, gland found to be a little œdematous.

* The pilocarpin used in this experiment was not the pilocarpin nitrate used in the other experiments. In 1882, I received from Kew a packet of leaves from British Guiana, supposed to contain an alkaloid like pilocarpin. Messrs. Brady and Martin treated the leaves in the usual manner for the extraction of alkaloids, and so obtained a small quantity of a brownish, rather viscous, substance. This was dissolved with the aid of a little acid to make a ·5 p.c. solution. I found that the solution differed from pilocarpin in causing rather less secretion of saliva, less slowing of the heart, less fall of blood pressure, and in having a greater paralysing influence on the secretory fibres of the chorda tympani and on the inhibitory fibres of the vagus. The solution prepared in 1882, filtered from some fungus which had grown in it, was used in Experiment 10 (J. N. Langley).

Here the injection into the blood of 10 grm. of sodium chloride, dissolved in 50 c.c. of water, increases in a marked but not immoderate degree the percentage of salt in the saliva. This is better seen by arranging the results in the following manner:—

Table XI.

		Rate of secretion per minute in c.c.	Percentage of salts.
I.	Saliva before injecting NaCl solution	·292	·590
III.	Saliva after injecting NaCl solution	·208	·685
II.	Saliva before injecting NaCl solution	·085	·281
IV.	Saliva after injecting NaCl solution	·038	·352

It will be noticed that the slow secretion (IV.) occurring after injection of the salt solution contains a comparatively high percentage of organic substance; since there is reason to suppose that the strength of stimulus was less in this case than during the secretion of the saliva immediately before it, we take the increased percentage of organic substance to be due to a lessened secretion of water, brought about by the strong salt solution interfering with the blood flow through the gland. This, no doubt, also contributes towards increasing the percentage of salts in the saliva.

Experiment 11.

July 3, 1888. Dog. Weight, $12\frac{1}{2}$ kilos. Chordo-lingual and vago-sympathetic cut on left side. Pilocarpin nitrate ·2 per cent. Injection made into a branch of the crural artery peripherally.

		Time of secretion in minutes.	Amount of saliva in c.c.	Rate of secretion in c.c. per minute.	Percentage of organic substance.	Percentage of salts.	Remarks.
							12.27–12.33½. Inject 5 mgrm. pilocarpin.
I.	12.36	4	2·8	·700	·325	·347	The rate of secretion falls unusually slowly.
II.	12.43	4	2·7	·675	·344	·401	
							12.50–12.54. Inject 50 c.c. NaCl 20 p.c.
III.	12.56	3	2·4	·800	·453	·612	
							1.0–1.3. Inject 50 c.c. NaCl 20 p.c. The secretion becomes slower during the injection.
IV.	1.4	6	3·4	·566	·416	·641	
							1.14–1.17. Inject 50 c.c. NaCl 20 p.c. The secretion becomes very slow during the injection.
V.	1.20	13	0·8	·062	·924	·569	

1.33. Inject 2 mgrm. pilocarpin. The heart stops, or nearly so; recovers on artificial respiration; but, then, cannot get secretion of saliva by injecting pilocarpin, nor by stimulating the chorda. After death, the gland found to be distinctly œdematous.

The injection of strong sodium chloride solution into the blood causes here, as in the previous experiment, an increase in the percentage of salts in the saliva secreted, and, taking the rate of secretion into consideration, there is an increase in the percentage of salts after each injection. But in neither experiment does the percentage of salts reach the maximum (·77 to ·78 per cent.) which may be obtained normally with a rapid secretion. It is possible, however, that, if a rapid secretion had been obtained, the normal maximum percentage of salts might have been exceeded.

According to KLIKOWICZ,* the blood, when strong sodium chloride solution is injected into the circulation, very rapidly gives up sodium chloride to the tissues, and takes up water from them.† From these causes combined, but especially from the former, the injection of sodium chloride into the blood does not lead to a corresponding increase in the percentage of salts in it. Thus, KLIKOWICZ found that the injection of 21 grm. of NaCl—in 10 per cent. solution—into the blood of a Dog weighing 24·5 kilos. only increased the percentage of Cl in the blood from ·301 to ·435, and in the serum from ·371 to ·554; the blood being taken two minutes after the end of the injection, which itself lasted five minutes. But the amount of NaCl injected, if simply added to the amount of blood in the animal, would increase the percentage of Cl in the blood by at least 1·2, if it all remained in the plasma, and by about half as much, if it were equally distributed between blood corpuscles and plasma.

In our experiments, about twice as much NaCl per kilo. of body weight was injected as in KLIKOWICZ's experiments. By the light of KLIKOWICZ's results we should suppose that in our experiments the percentage of salts in the plasma, during the collection of saliva, varied from 1 to 1·5 per cent.

In Experiment 11, the injection of strong salt solution not only increases the percentage of salts in the saliva, but also the percentage of organic substance; this is very markedly the case in V. In V. the small rate of secretion makes it unlikely that the salt solution exerts a stimulating action on the gland. An indirect action may take place by means of the blood vessels. The salt solution may diminish the normal blood flow through the gland, either by weakening the heart beat, or by counteracting the vaso-dilator effect of pilocarpin. And this would be sufficient to account for the rise in percentage of the organic substance, and for a part of the rise in percentage of the salts.

Since it seemed possible that a mixture of the various salts found in saliva might have a greater effect than sodium chloride alone, we tried one experiment, injecting into the blood a solution containing about 19 per cent. of the salts found in saliva, viz. :—

* KLIKOWICZ, 'Archiv f. Anat. u. Physiol. (*Physiol. Abth.*),' 1886, p. 534.

† The injection of strong sodium chloride caused the sub-maxillary gland in both of our experiments to become somewhat œdematous.

	Per cent.
Sodium chloride	7·730
Potassium chloride	4·600
Sodium carbonate	4·510
Potassium sulphate	1·045
Calcium carbonate	·750
Calcium phosphate	·565
	19·200

CO_2 was passed through this to dissolve as far as possible the calcium salts, and the mixture was then filtered.

Experiment 12.

July 4, 1888. Dog. Weight, $15\frac{1}{2}$ kilos. Chordo-lingual and vago-sympathetic cut. The pilocarpin nitrate was injected into a branch of the crural vein centrally; the salt solution was injected into a branch of the crural artery peripherally.

		Time of collecting saliva in minutes.	Amount of saliva collected.	Rate of secretion per minute in c.c.	Percentage of organic substances.	Percentage of salts.	Remarks.
I.	12.46	18	2·8	·156	·160	·213	12.42. Inject 3 mgrm. pilocarpin.
II.	1.6	2	3·0	1·500	·583	·600	1.4. Inject 3 mgrm. pilocarpin.
III.	1.10	5	2·5	·500	·273	·341	Secretion begins 10, and ends $5\frac{1}{2}$, drops a minute. 1.6–1.21$\frac{1}{2}$. Inject 50 c.c. solution of salts; this apparently delays the fall in the rate of secretion.
IV.	1.22	8	2·9	·362	·284	·355	Secretion begins $5\frac{1}{2}$, and ends $5\frac{3}{4}$, drops a minute. 1.31. Inject 2 mgrm. pilocarpin.
V.	1.33	2	2·4	1·200	·414	·522	
VI.	1.36	6	3·2	·533	·239	·385	
VII.	1.44	5	2·6	·520	·200	·340	1.49–1.52. Inject 50 c.c. solution of salts.
VIII.	1.54	7	3·1	·443	·160	·284	
IX.	2.4	2	3·7	1·850	·512	·695	2.2. Inject 2 mgrm. pilocarpin. Rate of secretion rapidly falls. 2.6. Inject 4 mgrm. pilocarpin; increases rate of secretion.
X.	2.8	2	3·3	1·650	·933	·713	2.11. Inject 4 mgrm. pilocarpin; very slight effect on rate of secretion.
XI.	2.14	3	3·2	1·067	·927	·509	2.18. Inject 8 mgrm. pilocarpin; slightly increases rate of secretion 3 minutes after injection.
XII.	2.22	4	3·8	·950	1·408	·543	

In this experiment, we may take as normal the first three samples of saliva secreted under the influence of pilocarpin, before the solution of salts was injected, and compare with these the saliva secreted after the injection of salts. Samples X. and XII. we omit from the following Table, and consider them later (*cf.* p. 147), since we think that the percentage of salts in these was largely influenced by the considerable dose of pilocarpin given.

TABLE XII.

	Rate of secretion per minute in c.c.	Percentage of salts.	Variation in percentage of salts corresponding to a variation of ·01 c.c. a minute in rate of secretion.	Remarks.
I.	·156	·213	·0037	Normal.
III.	·500	·341		
II.	1·500	·600	·0026	
†IV.	·362	·355	..	Immediately after injecting 50 c.c. 19 p. c. salts; percentage of salts in IV. much above normal.
III.	·500	·341		
†V.	1·200	·522	·0026	Percentage of salts in V. slightly higher than normal.
II.	1·500	·600		
III.	·500	·341	·0133	Percentage of salts in VI. above normal.
†VI.	·533	·385		
V.	·500	·341	..	Percentage of salts in VII. above normal, and more so than in VI.
†VII.	·520	·340		
†VIII.	·443	·284	·001	Immediately after injecting 50 c.c. 19 p. c. salts; percentage of salts in VIII. above normal, but less so than in VII.
III.	·500	·341		
II.	1·500	·600	·0027	Percentage of salts in IX. slightly above normal.
†IX.	1·850	·695		
†XI.	1·067	·509	·0021	Percentage of salts in XI. normal, or nearly so.
II.	1·500	·600		

It will be seen from the above Table that, of the three rapid secretions obtained after the injection of salts into the blood (V., IX., XI.), two (V. and IX.) have apparently a slightly higher percentage of salts than normal.

The slower secretions (IV., VI., VII., VIII.) have all a higher percentage of salts than normal, and this is very marked in IV., which was taken soon after injecting salts into the blood.

That is to say, *with an increase of salts in the blood which leaves the secretory power of the gland unaffected, the percentage of salts is relatively more increased with slowly secreted than with rapidly secreted saliva.*

The considerable increase in salts in IV. is, we think, due to some interference with the circulation through the gland, for this contains a higher percentage of organic substance than the more rapidly secreted saliva III.

General Remarks on the Effect on the Secretion of Salts and Injecting Salt Solution into the Blood.

We have seen that, when the volume of the blood is increased to any considerable extent by salt solution varying from ·2 to 2 per cent., the rate of secretion for a given stimulus is increased. This we take to be due to a larger quantity of blood passing through the gland ; and from this, together with the fact that bleeding decreases the rate of secretion, we conclude that, within certain limits, the amount of water secreted for a given stimulus—with a given irritability of the gland—varies directly with the amount of blood passing through the gland.

The increased secretion of water brought about by increased blood-flow is accompanied, unless, perhaps, when the blood is excessively diluted, by an increase in the secretion of salts. The extent of the increase depends upon the percentage of salts in the blood ; if the percentage of salts in the blood be sufficiently diminished, the increase in the amount of salts secreted does not keep pace with the increase in the secretion of water, and, consequently, the percentage of salts in the saliva falls. If the percentage of salts in the blood be sufficiently increased, that of the saliva will also be increased.

We have made no direct experiments upon the effect of increasing the volume of the blood without altering the percentage of salts in it ; but we are inclined to think, from a consideration of Experiment 4, that in such case, and with a given stimulus, the secretion of water would be more increased than the secretion of salts ; that is, that the percentage of salts would increase with the increase of flow, but rather less than normally.

In Experiment 4, the saliva is obtained before and after giving a small dose of atropin. After such a dose, stimulation of the chorda tympani with strong induction shocks, or the injection of large doses of pilocarpin, produces on the gland the effect of a weak stimulus only, in so far that the secretion of water is slow, and the percentage of organic substance and of salts in it is small. But the stimuli are still able to produce a maximal, or nearly maximal, effect on the small arteries. Hence, then, in Experiment 4, the samples of saliva III. to VI. inclusive are obtained under conditions of more copious blood supply than normally accompanies a weak stimulation of the gland. Taking I. as a standard, we think that III. to VI. contain a smaller percentage of salts than corresponds with their rate of secretion.

Variation in the Percentage of Salts in Saliva Secreted under the Influence of Pilocarpin.

In comparing samples of saliva secreted at different rates under the influence of pilocarpin, we, of course, leave out of account those in which one sample is secreted before and another after bleeding, clamping the carotid, or other treatment which may modify the character of the saliva subsequently secreted. We take only those cases in which the two or more samples of saliva are obtained in succession by pilocarpin. The number of the sample of saliva will show whether it was secreted at the beginning of an experiment or not.

There are eight cases in which the saliva obtained by pilocarpin can be compared; in six of these the percentage of salts follows HEIDENHAIN'S law. In consequence of the effect of pilocarpin upon the heart and upon the small arteries, we should not expect to find that the statement we have made above with regard to chorda saliva—viz., that, with equal increments in the rate of secretion, the increments in the percentage of salts become less—should necessarily hold with regard to pilocarpin saliva. But five of the six cases which follow HEIDENHAIN'S law show more or less distinctly that the statement is also true, under certain conditions, with saliva obtained by pilocarpin.

TABLE XIII.

		Rate of secretion per minute in c.c.	Percentage of salts.	Increase in percentage of salts corresponding to an increase of ·01 c.c. a minute in rate.	Weight of Dog in kilos.	Remarks.
Ex. 10.	II.	·085	·281	·0149	6	3 mgrm. pilocarpin injected before I.
	I.	·292	·590			
Ex. 7.	XII.	·116	·424	·02	..	4 mgrm. pilocarpin injected before X., 8 mgrm. before XI., 15 mgrm. before XII.
	X.	·125	·442			
	XI.	·192	·476	·0051		
Ex. 4.	V.	·182	·238	·0062	..	7·5 mgrm. pilocarpin injected before V.
	IV.	·376	·358			
Ex. 12.	I.	·156	·213	·0037	$15\frac{1}{2}$	3 mgrm. pilocarpin injected before I., and 3 mgrm. before II.
	III.	·500	·341			
	II.	1·500	·600	·0026		
Ex. 8.	II.	·250	·420	·0033	$8\frac{1}{4}$	3 mgrm. pilocarpin injected before I., and 3 mgrm. before III. The percentage of salts in II. may have been slightly diminished by the injection of NaCl ·2 p. c. (*cf.* p. 22).
	I.	·675	·559			
	III.	1·650	·752	·0019		
Ex. 9.	II.	·355	·401	·0003	15	5 mrgm. pilocarpin injected before I.
	I.	·750	·403			
	V.	·490	·515	·0005	..	5 mrgm. pilocarpin injected before IV.
	IV.	·700	·525			

In Experiment 9, it is clear that the fall in the percentage of salts is too slow compared with the fall in the rate of secretion. The probable reason of this we will consider with the two cases in which the percentage of salts did not follow HEIDENHAIN'S law. These are—

TABLE XIV.

		Rate of secretion per minute in c.c.	Percentage of salts.	Weight of Dog in kilos.	Remarks.
Ex. 11.	I.	·700	·347	$12\frac{1}{2}$	5 mgrm. pilocarpin injected before I.
	II.	·675	·401		
Ex. 12.	IX.	1·850	·695	$15\frac{1}{2}$	2 mgrm. pilocarpin injected before IX., 4 mgrm. before X., 4 mgrm. before XI., 8 mgrm. before XII.
	X.	1·650	·713		
	XI.	1·067	·509		
	XII.	·950	·543		

The explanation of the variations found in experiments 9, 11, 12 we take to be that the pilocarpin did not bring about the normal increase in the blood flow through the gland. In the absence of direct observations on the blood flow in these cases, we cannot, of course, positively assert that the explanation we give is the true one, but there are certain facts somewhat in favour of it.

In Experiment 11, the rate of secretion of organic substance is rather faster in II. than in I.:—

TABLE XV.

	Amount of organic substance secreted in 100 min.
I.	·2275
II.	·2322

So that presumably the stimulus to the gland-cells either was a little stronger in II. than in I., or the gland had increased in irritability; but, since the rate of secretion of water was rather slower in II., despite this, it is probable that the blood flow through the gland was less in amount in II.

In Experiment 9, the salts fall with the falling secretion, but less than normally; but, since the percentage of organic substance also falls less than normally, the cause of the variation may be that the water is secreted less rapidly than corresponds with the strength of stimulus and a full blood flow; that is to say, the percentage of salts falls less than normally because the flow of blood through the gland falls more than normally.

Lastly, we have to consider the exceptions from HEIDENHAIN'S law in Experiment 12. If we compare the percentage of salts in IX. and XI. with that in II., we see that both contain very nearly a normal percentage of salts, it being, we think, a trifle higher than normal in IX. on account of the injection of salts taking place shortly before (*cf.* p. 142). The exceptions to the law are X. and XII.

Taking the rate of secretion of organic substance in IX. to XII., we have :—

TABLE XVI.

	Amount of organic substance secreted in 100 min.
IX.	·9472
X.	1·5394
XI.	·9891
XII.	1·3376

i.e., the stimulus to the gland, as indicated by the rate of secretion of organic substance, was considerably greater in X. and XII. than in IX. and XI. In X., notwithstanding the stronger stimulus, less water is secreted than in IX. ; probably, then, during the secretion of saliva X. less blood was flowing through the gland. And this is the more likely in this instance, for, when successive doses of pilocarpin are injected into the blood, they produce less and less increase of the blood flow through the gland.* But, as we have seen, a decreased flow of blood during an increased stimulus is adequate to cause an increase in the percentage of salts. A similar line of argument applies to XI. and XII.

A comparison of IX. and XII. shows that there remains an intimate connection between the percentage of salts and the rate of secretion of water, for XII. secreted, if the foregoing reasoning is sound, under the influence of a stronger stimulus than IX. and with less blood flow, and in consequence, having a higher percentage of salts than normal, has nevertheless a considerably lower percentage of salts than IX., which is secreted at a rapid rate.

We may consider here one or two other results bearing on this question. In Experiment 7 (X. to XII.), increasing the quantity of pilocarpin given, increases the rate of secretion of organic substance very much more than it increases the rate of secretion of water or of salts. When a fairly large dose of pilocarpin is given at the beginning of an experiment, a rapid secretion of saliva takes place and the percentage of organic substance in it is high. Subsequent doses produce a less and less rapid secretion of saliva ; so that, although the percentage of organic substance remains high, the rate of secretion of organic substance much diminishes. At the same time, there is a great decrease in the irritability of the chorda tympani, shown by the fact that electrical stimulation of the chorda tympani has very little

* LANGLEY, 'Journal of Anat. and Physiol.,' vol. 11, 1876, p. 176 ; and 'Journal of Physiology,' vol. 1, 1878, p. 366.

effect. It follows from this, since there is good ground for supposing that pilocarpin causes a secretion by stimulating the endings of the chorda tympani, that pilocarpin also is unable to stimulate strongly the gland. The high percentage of organic substance, then, in the saliva secreted slowly after repeated doses of pilocarpin must in the main be referred to the diminished blood supply to the gland. And it is probable that the percentage of salts in the saliva, though higher than normal, is not very greatly so, because the stimulus to the gland is far from the normal maximum. In this way we should explain the results of Experiment 7, X. to XII.

An instance of an increase in the percentage of salts caused directly by an increased stimulus, and not indirectly by increasing the rate of secretion of water, is, perhaps, given in Experiment 4. In sample VI., stimulation of the chorda has, in consequence of the previously injected atropin and pilocarpin, a barely appreciable effect on the rate of secretion of water, but it increases the percentage of organic substance and of salts above that of V., secreted previously to, and at the same rate as, VI.

The Effect of a small dose of Atropin upon the Percentage Composition of Saliva.

It has been argued by one of us* that atropin paralyses "secretory" and "trophic" fibres simultaneously. The results of Experiments 4 and 9 offer some confirmation of this.

In both of these experiments a small dose of atropin is given, such that a stimulation of the chorda and injection of pilocarpin still produce some secretion. If atropin affected the "secretory" before the "trophic" fibres, the saliva obtained after atropin has been given should contain a high percentage of organic substance in proportion to its rate of secretion. This is not the case; on the contrary, the percentage of organic substance is small; the percentage is, in fact, so small that it, at first sight, appears as if the "trophic" fibres were affected more than the "secretory" fibres by the atropin. But we have seen that the secretion of water depends in part upon the amount of blood flow through the gland. In these experiments, after atropin had been given, the stimulation of the chorda and the injection of pilocarpin caused a copious blood flow, with but slight activity of the gland cells; hence, the secretion of water was abnormally increased.

Sub-lingual Saliva.

Werther† has shown that the sub-lingual saliva of the Dog contains a very high percentage of salts, and a rather low percentage of organic substance. The three analyses given by him are as follows :—

* Langley, 'Journal of Physiology,' vol. 9, 1888, p. 55.

† Werther, 'Archiv. f. d. ges. Physiol.,' vol. 38, 1886, p. 298.

TABLE XVII.

	Percentage of organic substance.	Percentage of salts.	Percentage of solids.
I.	·19	1·34	1·53
II.	·43	·94	1·37
III.	·34	·94	1·28

Since WERTHER's are the only observations on the subject, we collected the saliva secreted by the left sub-lingual gland during the course of Experiment 12. In about $1\frac{3}{4}$ hour, 2·6 c.c. of saliva were secreted. This was analysed, with the following result :—

TABLE XVIII.

Percentage of organic substance.	Percentage of salts.	Percentage of solids.
·602	1·034	1·636

The result confirms WERTHER's observations as to the high percentage of salts; the percentage of organic substance is, however, somewhat higher than in his analyses. In the course of our experiment, 19 grm. of salts were injected into the blood; this, taken in conjunction with WERTHER's observations, shows that the injection into the blood of a considerable amount of salts has very little effect upon the secretion of salts by the sub-lingual gland.

THE RAPIDITY OF SECRETION OF CERTAIN SALTS WHEN INJECTED INTO THE BLOOD.

We have tried also two experiments on the rapidity with which substances injected into a vein appear in the saliva. In these experiments pilocarpin was injected with the substances to be observed, and each successive three drops of saliva secreted collected separately and tested. We found that :

1. On injection of 50 c.c. of lithium citrate into the blood, the first drop secreted, both from the sub-maxillary and from the parotid gland, showed the lithium band in the spectroscope.
2. On injection of 50 c.c. of potassium iodide into the blood, this salt was present in all the drops of saliva after the first six. The potassium iodide was first detected in the sub-maxillary saliva, as the secretion from this gland was more rapid than that from the parotid. The presence of potassium iodide was observed by adding to the saliva a little starch solution and then a drop or two of strong nitric acid. There may, of course, have been traces of potassium iodide in the first drops of saliva, which this test failed to show.

3. The excretion of the salts injected continued during the whole of the experiment —two to three hours.
4. For a given amount of saliva, the parotid secreted more potassium iodide than the sub-maxillary gland.
5. On injecting potassium ferrocyanide 1 per cent. into the blood, no trace of it could at any time be found in the saliva.
6. On injecting sulphindigotate of soda into the blood, no indigo white could be found in the saliva. We tried this because it seemed possible that the known absence of sulphindigotate of soda from the salivary secretion, after copious injection of it into the blood, might be due to its conversion into indigo-white.

THE RECENT OBSERVATIONS OF NOVI.—After we had completed our experiments and nearly finished our account of them, we received a separate copy of a paper by NOVI on the effect of injecting a strong solution of sodium chloride into the blood on the percentage of chlorine in saliva.

NOVI'S observations, undertaken at the suggestion of LUDWIG, were made on Dogs, and in the following manner:—About 80 c.c. of blood were withdrawn from the Dog, then saliva collected; after this a 10 per cent. solution of salt was injected into the blood, a fresh sample of saliva collected, and a further portion of blood withdrawn. This was repeated one or more times, according to the weight of the animal and the freedom of secretion of saliva. The percentage of chlorine in the several samples of blood serum and of saliva was then determined. The chief results obtained were:

1. That an increase of chlorine (as chlorides) in the blood plasma increases the percentage of chlorine in the saliva with a given rate of secretion, and may increase it with a slower rate of secretion.
2. That with a rapid rate of secretion an increase of chlorine in the blood plasma may increase the percentage of chlorine in the saliva above the maximum that can normally be obtained.

With the former of these results our observations are in the main in agreement, but we find that an increase in the salts in plasma which does not interfere with the secretory power of the gland has very little effect on the percentage of salts in saliva as long as the secretion of water is rapid, and that the increase in the percentage of salts which take place with a slower secretion is partly due to the injection of strong salt solution leading to a decreased blood flow through the gland (*cf.* p. 136, *et seq.*).

The latter of these results, our observations leave undecided. For, whilst, on the one hand, we never obtained after injection of salts into the blood so high a percentage of salts in the saliva as can be obtained normally, on the other, we only once obtained a rapid secretion (Experiment 12, IX.) after the injection of salts; and it is possible that in this case the rate of secretion was less than the maximum, and that an insufficient amount of salts was injected.

At the same time, it may be pointed out that Novi observed an increase above normal in the percentage of chlorine in saliva in three cases only, and these only slightly exceed the maximum known to occur in normal saliva. The maximum found by Werther in normal saliva is ·352 per cent.; in the three cases of Novi, mentioned above, the percentages of chlorine are ·360, ·363, ·382, and Novi mentions that the error in estimation of the chlorine may be as much as ·03 per cent.

Novi also found that, when the chlorine in serum was increased to ·7 per cent., no more saliva could be obtained from the gland. His method of obtaining saliva was to place dilute acids or weak ammonia in the mouth, and so to set up a secretion reflexly. In our Experiments 10 and 11, the injection of strong salt solution distinctly lowers the amount of saliva that could be obtained from the gland by stimulating the chorda tympani and by pilocarpin, and, when injected in sufficient amount, prevented all secretion from taking place. Hence, it seems to us probable that, if the injection of salts can lead to an increase in the percentage of salts in the saliva above normal, the amount of salts injected must be regulated with great nicety.

Some Remarks on the Theory of Secretory Nerves.

We do not propose to discuss the question whether there is more than one kind of secretory nerve; but there are some facts in the foregoing experiments bearing on the question which we cannot leave without mention.

The most striking of these is the effect of bleeding in Experiment 8. The loss of blood causes an increase in the percentage of organic substance in the saliva.* This tends to show that with a given stimulus the percentage of organic substance increases as the blood-flow through the gland decreases. Further, the actual rate of secretion of organic substance is somewhat decreased by loss of blood, and this may be fairly interpreted as meaning that with a lessened blood supply the gland-cells become less irritable. Lastly, loss of blood increases the percentage of salts much less than it increases the percentage of organic substance; that is to say, the secretion of salts is much less affected by the strength of stimulus than the secretion of organic substance.

Hence, it would appear that with a given stimulus to the gland-cells a decrease in

* Since writing this we have met with an observation of Zerner ('Medizinische Jahrbücher,' 1887, p. 534) bearing on the secretion of organic substance. He finds in the Dog that, after lowering the blood pressure by section of the cervical spinal cord, the more slowly secreted sub-maxillary saliva, obtained by stimulating the chorda tympani, has a higher percentage of organic substance than the more rapidly secreted saliva, similarly obtained under normal conditions. Two experiments are given. One of these is inconclusive, since the sympathetic was stimulated before obtaining the second sample of chorda saliva, and this, as Heidenhain has shown, is sufficient to increase the percentage of organic substance. The other experiment, although more satisfactory, is not entirely so, for the chorda tympani was stimulated with strong shocks between the time of collecting the two samples of saliva analysed.—Feb. 6, 1889.

the blood-flow through the gland will lead to a diminished irritability of the gland, and, therefore, to a decrease in the rate of secretion of all the constituents of saliva. It will lead also directly to a considerable decrease in the secretion of water, to a less decrease in the secretion of salts, and to a still less decrease in the secretion of organic substance. With decreased blood-flow there will be less saliva, and this will contain a somewhat higher percentage of salts and a considerably higher percentage of organic substance. Further, with a given decrease of blood-flow, the stronger the stimulus, the higher will be the percentage of organic substance and of salts.

Now, sympathetic saliva is just such saliva as, from the above-mentioned facts and deductions, we should expect to be produced by simultaneous stimulation of a secretory nerve and of glandular vaso-constricted fibres. Since the decrease in the blood-flow through the gland is much greater on stimulating the sympathetic than on bleeding, the sympathetic saliva should be secreted more slowly and contain a higher percentage of organic substance than saliva secreted after bleeding. And this is the case.

That the effect of decreasing the blood-flow through the gland is as we have given it, is supported by several other experiments besides the one we have quoted here; but we do not enter into the matter further, for two reasons—the one that there are certain of HEIDENHAIN'S observations which appear to be contradictory to ours, and these we have not yet repeated, and the other that, since in the Cat stimulation of the sympathetic gives a saliva containing a low percentage of organic substance, it is desirable to investigate what in this case is the effect of a decreased blood flow.

SUMMARY OF CHIEF RESULTS.

HEIDENHAIN has shown that, when saliva is obtained by stimulating the chorda tympani, the percentage of salts in the saliva depends upon the rate of secretion; so that, the faster the secretion, the higher the percentage of salts is up to a limit of about ·6 per cent. WERTHER has come to the same conclusion, but finds that the percentage of salts may be as much as ·77.

In both HEIDENHAIN'S and WERTHER'S experiments there are a considerable number of exceptions to this rule, attributed by them to variations in the rate of secretion of saliva during the time of collecting any one sample.

We have repeated, with some modifications, the experiments of HEIDENHAIN, paying especial attention to the rate of secretion of the saliva, and find, in 10 out of 11 cases, that his law of an increase in the percentage of salts with an increase in the rate of secretion holds. The single exception may possibly be due to a modification in the blood-flow through the gland during the time of collecting the saliva. The slowly secreted saliva contains a low percentage of salts, whether it is produced by a weak nerve stimulus, or by a very strong nerve stimulus, which lowers the irritability of the nerve-fibres.

We do not find any rate of secretion beyond which an increase in rate fails to increase the percentage of salts in the saliva. The increment in the percentage of salts decreases with each equal successive increment in the rate of secretion.

As a rule, in saliva obtained by injecting pilocarpin, the percentage of salts follows HEIDENHAIN'S law. We take the exception to be due to the action of pilocarpin on the circulation, the blood-flow through the gland being less than normally accompanies the degree of stimulation of the gland-cells.

The percentage of salts in saliva obtained by stimulating the sympathetic is higher than corresponds to its rate of secretion, the saliva obtained by stimulating the chorda being taken as a basis of comparison.

Dyspnœa decreases the rate of secretion of saliva, and, if not too prolonged, increases the percentage of salts, and tends to increase the percentage of organic substance in the saliva. This holds, whether the saliva be obtained by stimulating the chorda tympani, or by injecting pilocarpin. Dyspnœa has for a short time an after-action, tending to increase the percentage of salts, and possibly that of organic substance.

Clamping the carotid during secretion has the same general effect as dyspnœa, but it causes a still more marked increase in the percentage of salts. Its after-effect is also much greater and lasts longer. Bleeding has a similar effect to dyspnœa and to clamping the carotid, and it causes a marked increase in the percentage of organic substance.

Injection of dilute salt solution in sufficient quantity considerably increases the rate of secretion of saliva; the percentage of salts in the saliva decreases, although the rate of secretion of salts usually increases; the percentage of organic substance decreases—that is, increasing the volume of the blood with dilute salt solution chiefly increases the rate of secretion of water.

The percentage of salts in samples of saliva obtained *after* the injection of dilute salt solution increases with the rate of secretion; it is only when these are compared with samples obtained before the injection that a discrepancy in the normal relation between percentage of salts and rate of secretion of water appears.

Injection of sodium carbonate 2 per cent. also increases the rate of secretion of saliva; in this case the percentage of salts is about normal; the percentage of organic substance falls slightly only.

Injection of strong salt solution increases the percentage of salts in saliva; this is in accordance with the recent observations of NOVI, that the chlorine in the salts of saliva is increased for a given rate of secretion by increasing the percentage of sodium chloride in the blood. We find, however, that on injection of strong salt solution into the blood which leaves the secretory power of the gland unaffected, the increase in the percentage of salts is much greater with slowly than with rapidly secreted saliva, and that, when the secretory power of the gland is affected by the strong salt solution an increase in the percentage of organic substance also takes place; this and a part of the increase in the percentage of salts we attribute to a decrease of the blood-flow through the gland.

Saliva produced by stimulating the chorda tympani or by injecting pilocarpin, after a small dose of atropin has been given, contains a low percentage of organic substance and of salts.

We, like WERTHER, find that sub-lingual saliva has a considerably higher percentage of salts than sub-maxillary saliva.

If lithium citrate, potassium iodide, potassium ferrocyanide, and pilocarpin are injected into the blood, lithium can be detected in the first drop of saliva secreted, iodine after the first six drops; potassium ferrocyanide cannot be detected at any stage of secretion.

III. *On the Organisation of the Fossil Plants of the Coal-Measures.*—Part XV.

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[Plates 1–4.]

Some years ago M. Renault described* some specimens of petioles of Ferns, which he identified with Corda's genus *Zygopteris,* identical, in part, with Cotta's genus *Tubicaulis.* In my memoir, Part VI.,† I described, from the lower Carboniferous rocks of Lancashire, two of M. Renault's species, viz., *Zygopteris Lacattii* and *Z. bibractensis;* but, from an unwillingness to multiply genera based only upon the ill understood fragments of imperfectly known plants, I proposed (*loc. cit.*, p. 677) the provisional adoption of the neutral generic term *Rachiopteris* for a considerable number of these objects, which appeared to be either rhizomes or petioles of Ferns. Subsequent researches have, I think, shown the wisdom of doing so ; at all events, further discoveries, which I now propose to put on record, unmistakably confirm my opinion.

In the same memoir (*loc. cit.*, p. 173) M. Renault described a rhizome, with petioles, the latter of which closely resembled those of Corda's genus *Anachoropteris,* and to which the French palæontologist gave the name of *Anachoropteris Decaisnii.* But the structure of the rachis of this plant, especially of the transverse section of its vascular bundle, was wholly different from that of any plant previously observed. Having obtained a stem identical with this *Anachoropteris,* but without any petioles connected with it, I figured my specimen in my memoir, Part VI., Plate 58, fig. 51, where I described it as closely resembling M. Renault's *Anachoropteris Decaisnii.*

It must be remembered that Corda's two genera, *Anachoropteris* and *Zygopteris,* were solely based by him upon distinctions between the transverse sections of two petioles. That author knew nothing of the nature of the rhizome of either of these petioles. M. Renault, however, obtained a rhizome associated with a petiole closely identical with that of Corda's *Zygopteris,* which he described* under the name of *Zygopteris Brongniartii.* He thus possessed Corda's two forms of petiole asso-

* 'Annales des Sciences Naturelles,' 5ième Série, *Bot.*, vol. 12, 1869, p. 161.

† 'Phil. Trans.,' vol. 164, 1874.

ciated with two distinct, though not wholly dissimilar, rhizomes. The French *savant* described the two genera *Zygopteris* and *Anachoropteris* as distinguished primarily by the differences between these petioles, but secondarily by the differences between their rhizomes.

In my memoir, Part VIII. ('Phil. Trans.,' vol. 167, pp. 217 *et seq.*), when describing a new rhizome and its petioles, under the name of *Rachiopteris corrugata*, I gave at some length my reasons for not multiplying generic names for these curious plants; pointing out how wholly impossible it was to classify recent Ferns on any such basis, a fact the importance of which is further illustrated by the rhizome which I am about to describe.

Some weeks ago, my young auxiliary collector, Mr. LOMAX, to whom I was indebted for the Calamitean fruits described in my last memoir, Part XIV., brought me a specimen having the central vascular axis of M. RENAULT'S *Anachoropteris Decaisnii*, with petioles of the true Zygopteroid type: thus demonstrating that the axis found by RENAULT in connection with a petiole of CORDA'S type of *Anachoropteris* was equally the axis of a Zygopteroid petiole. The specimen has been a drifted fragment, now imbedded in a hard ganister full of Goniatites.

Fig. 1 (Plate 1) shows the five-rayed transverse section of the vascular axis of the stem or rhizome; at a is a vacant spot, occupied in some sections by a delicate parenchyma—obviously a medullary one—five thin prolongations of which, a', a', are projected into five rays of the vascular axis b. This axis is composed of a mass of scalariform tracheids. Each centrifugal ray first contracts in diameter, and then expands again, terminating in a truncated, more or less bifurcated extremity. The maximum diameter of this axis from the tip of one ray to that of another is rather more than a quarter of an inch. At b' the end of one of these rays is detached, apparently to form the vascular centre of a lateral appendage. At c is a thin band of structure superficially resembling a bundle-sheath; a similar investment encompasses not only the central axis, but each of the separate organs.† Apparent rootlets are seen at d.

Fig. 2 is a second transverse section through the vascular axis, b, of a specimen like fig. 1, from which it differs only in one or two respects. Thus, the detached bifurcate end resembling that of the ray b' of fig. 1 is replaced at fig. 2, b', by a cylindrical vascular bundle, ·05 of an inch in diameter, whilst the corresponding one at b'' has disappeared; between the bundle b' and its investing zone, c, are remains of cellular parenchyma. The black masses e, e are the carbonised remains of the cortical parenchyma.

Fig. 3 is part of another section like fig. 2, but in which the circular section of a

* *Loc. cit.*, Plate IV., fig. 4 *bis*, and Plate V., fig. 5.

† In a recent memoir, to be referred to on a later page, Professor STENZEL, of Breslau, describes specimens which show that these bands do represent zones of specialised, more or less sclerous, cortical tissue.

lateral appendage like b' of fig. 2 reappears at e, but is now enclosed within an entire and separate circle of the tissue fig. 2, c, at c'. The supposed rootlets are also seen at d, and the carbonised cortical tissue at e. But outside the circular aberrant organ b' we now have a transverse section of a large Zygopteroid petiole. The H-shaped section of the vascular bundle of this petiole is seen at f, surrounded by a ring of the structure c at c'.

Fig. 4 represents a more perfect specimen of a similar Zygopteroid petiolar bundle, in which several of the tissues are well preserved. In the slenderness both of its central portion f and its two transverse ones f', f'', this vascular bundle approaches nearer to RENAULT'S *Zygopteris bibractensis* than to any of the other forms hitherto described. In this specimen the cellular tissue of the cortical parenchyma is fairly well preserved, even in the dark masses, and is beautifully so at the two more central portions e', e'. The anomalous zone c entirely invests this vascular bundle, like an endodermal zone.

Fig. 5 is a small detached vascular axis found close to a section like figs. 1 and 2. In its centre there exists a small vacant spot, a, from which there diverge four radiating lines, apparently repeating, on a small scale, the configuration of a, a in figs. 1 and 2. Fig. 5A is obviously a structure identical in its contour with fig. 3, e, but the middle of its central bundle approximates to fig. 5A. We have a point a, from which radiate three lines, a', corresponding to a' of figs. 1 and 2. These two examples, fig. 5, and especially 5A, seem to suggest that, whilst the organs figs. 3 and 4, f, are destined to become true petioles, those of figs. 2, b', and 3, e, indicated by b', b' are destined to become ordinary branches of the rhizome, like the centres of figs. 1 and 2. The vascular axis of each of these circular structures is obviously destined ultimately to assume the pentagonal form of that of the primary stems.*

* On January 12th of the present year, I received from Professor STENZEL, of Breslau, a copy of an interesting memoir by him, entitled "Die Gattung *Tubicaulis*, COTTA." In this memoir the author figures and describes some examples of COTTA'S genus, and of *Asterochlæna*, *Anachoropteris*, and *Zygopteris* of CORDA. He sub-divides *Asterochlæna* into the sub-genera *Meriopteris*, *Asterochlæna*, and *Clepsydropsis*, and the genus *Zygopteris* into *Zygopteris* and *Ankyopteris*. Under the name of *Zygopteris* (*Ankyopteris*) *scandens*, this author describes and figures a plant which appears to me to be identical with my *Rachiopteris Grayii*. In this plant Dr. STENZEL finds the organ which I have represented in fig. 2, b', and fig. 3, e, in exactly the same position as I have done, viz., between the exterior of the main stem and the superior or posterior side of the petiolar bundle, fig. 3, f. He also regards it, as I have done, as a young state of a stem or branch; giving to it the apparently appropriate designation of an *axelsprosse*. His specimens further show that the dubious investing zones of the several organs which I have indicated in my several figures by c and c' are not mere mineral developments, but represent zones of tissue, often of a sclerenchymatous character, as I have already pointed out. He also thinks, as I have concluded, that the organs d, of my figures 1 and 3, are true roots or rootlets. These agreements between two independent observers are, of course, satisfactory. As to the specific name of the plant, since my memoir was received by the Royal Society on June 13th, 1888, whilst Professor STENZEL'S memoir has only been published during the present year, my name of *Rachiopteris Grayii* will have the precedence, unless Professor STENZEL has given the name to his plant in any earlier publication.—February 12th, 1889.

The exterior of the cortex of this plant was densely clothed with hairs. Though longitudinal sections through the specimen described present a somewhat obscure arrangement of tissues and organs, these hairs enable us to distinguish external surfaces from internal structures. The difficulty of doing this is the greater since two distinct stems are pressed closely together in the fragment of ganister in which my specimens are preserved, and also from the fact that the innumerable small cylindrical organs, *d*, variously intersected, and each with an ill preserved vascular bundle in its interior, abound in all my preparations both within the cortex, and externally to it, as at *d, d*. Similar structures appear to exist in M. RENAULT's specimens of *Anachoropteris*, but that observer regards them as representing petiolar bundles. Mine, like his, are ill preserved; but they more closely resemble adventitious roots than petiolar structures. A similar one M. RENAULT himself regards as a "racine adventif." These organs are about ·066 of an inch in diameter.

That no classification of these fossil Ferns based *solely* upon the transverse sections of their petiolar bundles is or can be of much value, is clearly shown when tested amongst those living Ferns the classification of which is chiefly based upon their sporangial reproductive organs. But, I think, I can show that we have here to do with a type of stem-structure which is remarkable, and which appears to throw them into something like a natural group recurring in several allied plants.

In his memoir above referred to, M. RENAULT describes and figures* a transverse section of the stem of his *Zygopteris Brongniartii*. In this section we find a central structure "très probablement cellulaire" "ou à ses prolongements qui, au nombre de six dans le *Zygopteris*, s'enfonçaient plus ou moins dans l'épaisseur de l'étui ligneux *a, a*, formé par les cellules scalariformes." (*Loc. cit.*, pp. 164–5.)

Specimens in my cabinet confirm M. RENAULT's suggestion that this central structure, with its thin radiating arms, is really a cellular one, being either a medulla or of a procambial character—but apparently the former—associated in either case with peculiarities in the primitive development of the vascular bundle which surrounds it. The presence of this peculiar cellular centre within the vascular axis constitutes a feature which seems to unite several otherwise distinct plants into a common group. It is obviously identical with the structure *a, a'*, seen in my figs. 1, 2, and 5 of the present memoir. In my memoir, Part VIII.,† I figured, under the name of *Rachiopteris corrugata*, transverse sections of a stem which has a structure almost identical with that of M. RENAULT's *Zygopteris Brongniartii*, and in which the central cellular structure sends off five or six radiating and sometimes dichotomosing arms, partially subdividing the surrounding vascular cylinder into half-a-dozen groups of cells. Unlike M. RENAULT's similar example, the petiolar bundles given off by this central axis are not Zygopteroid in form, since they lack the two parallel extensions *f', f''* seen in Plate 1, fig. 4, of the present memoir; what

* Plate 3, fig. 1.

† 'Phil Trans.,' vol. 167, Plate 5, fig. 4, and Plate 6, fig. 13.

remains corresponding apparently to the portion f of the same figure. In my previous memoirs I have pointed out the fact that all these Zygopteroid bundles give off alternately from the exterior of each of the two parallel portions f', f'' a considerable mass of vascular tissue, each of which detached portions usually subdivides into two parallel secondary bundles.* The *Rachiopteris duplex*, also described and figured (Part VI., 'Phil. Trans.,' vol. 164, Plate 55, figs. 28 and 35), exhibits the same phenomena, and must also be regarded as one of the Zygopteroid group; and it is more than probable that my *Rachiopteris insignis*, figured in Part VI., Plate 16, figs. 19, 20, and 21, may be regarded as connecting the petiole of *Rachiopteris corrugata*, already referred to, with the Zygopteroid type. The *Rachiopteris duplex* also appears, so far as its petiole is concerned, and especially from the mode in which the vascular bundles of the secondary petioles are detached from those of the primary one, to have corresponding affinities with the same type.

It is obvious, on the one hand, that we cannot retain both CORDA'S genera of *Zygopteris* and *Anachoropteris*; and, since the type of petiole which may be designated Zygopteroid is the more remarkable one of the two, and appears to have several other Carboniferous Fern-petioles closely allied to it, it seems desirable that M. RENAULT and those who wish to multiply these generic names should accept the genus *Zygopteris* and abandon that of *Anachoropteris* as too ill-defined to be of any real value. Personally, I prefer not to multiply these generic names until we obtain a more definite knowledge of structural identities and differences upon which generic groups can be based. Hence, I shall continue for the present to use my own provisional term of *Rachiopteris*; and, since the structure of the petiole of the plant just described is obviously sufficiently different from that of the *Anachoropteris Decaisnii* of M. RENAULT to distinguish it specifically, I shall designate the former plant *Rachiopteris Grayii*, in memory of my lamented friend, ASA GRAY.†

Returning to my figures of the petioles of *Rachiopteris duplex*,‡ it will be observed that the two secondary bundles a, a, given off in alternate pairs from the opposite sides a', a' of the primary petiolar bundle a, obviously supply the two secondary petioles y, y, giving off, in their turn, smaller bundles to a third series of branches or pinnules. Fig. 6 of Plate 2 of the present memoir represents a transverse section of a petiole of *Rachiopteris Lacattii*,§ for which I am indebted to Mr. LOMAX, of

* See memoir, Part VI., Plate 57, figs. 45, a'' and fig. 47, m, m.

† A further ground for abandoning M. RENAULT'S duplicated generic names is found in the fact that twice in his memoir (*loc. cit.*, pp. 165 and 177) he considers that *Zygopteris* is distinguished from *Anachoropteris* by having six of the radial prolongations of the medulla, whilst *Anachoropteris* has only five. My plant just described, which certainly should belong to M. RENAULT'S genus *Zygopteris*, has but five.

‡ Memoir, Part VI. (*loc. cit.*), Plate 65, figs. 35D and 35E.

§ In the memoir referred to on page 157, Professor STENZEL expresses an opinion that the petiole which in previous memoirs, as in the present one, I have identified with RENAULT'S *Zygopteris Lacattii* is really the *Z. elliptica* of the French author. This, however, is a mistake easily explained. In its middle cortex, M. RENAULT'S *Z. Lacattii* contains numerous gum-canals, which are not found in his

Radcliffe, and in which the same bifurcation of the secondary pinnules is seen at y, y, each branch being supplied with a secondary fibro-vascular bundle a', from which a bundle of a third order a'' is given off, as in *Rachiopteris duplex.* This mode of dichotomous branching of secondary pinnules is clearly incompatible with fronds of the ordinary pinnate or bipinnate types. The plant must have had some more distinctive contour. The lowest secondary pinnules of several forms of *Pteris*, *e.g.*, *P. umbrosa* and *P. serrulata* branch in this quasi-dichotomous way. M. ZEILLER has figured in his 'Études des Gîtes Minérales de la France.—Bassin Houiller de Valenciennes,' two Carboniferous Ferns that distinctly branch in this manner, viz., *Mariopteris latifolia* (*loc. cit.*, Plate XVII., and *Diplotemma Zeilleri Stur*, Plate XVI.).

The additions I have made from time to time to our knowledge of the organisation of the interesting fructification, *Calamostachys Binneyana*, have left but few lacunæ in that knowledge to be filled up. Two points, nevertheless, have as yet been obscure, viz., the distribution of the vascular bundles in the central axis of the strobilus and the nature of the peripheral terminations of the fertile bracts or sporangiophores. Figs. 7 and 8 of Plate 2 throw light on both these points.

Fig. 7 is a slightly oblique transverse section through an axis of *Calamostachys Binneyana*, in the centre of which, a, is a quasi-medullary cellular parenchyma more or less invested by scalariform vessels at b, b'. At the points b, b, these tracheids are few in number, but at the four angles b', b' they are much more numerous; especially so in other strobili in my cabinet where such points approximate to the nodes of the axis.

Fig. 8 represents one of the finest tranverse sections of this *Calamostachys* I have obtained. In it a represents the central axis corresponding to fig. 7. This centre is invested by the cortical zone, k. The fertile sporangiophores appear at v, and their much-thickened peripheral extremities are seen at v'. At the points v'', v'' accumulations of tracheids appear. On comparison of this figure with that given on Plate 54, fig. 23, of Part XI. ('Phil. Trans.,' 1881), it will be seen that these clusters of tracheids are concentrated in the immediate neighbourhood of the point v''' of that figure, *i.e.*, where each sporangium, u, is organically united to the thickened end of the sporangiophore, u'. It thus appears that these peripheral terminations of the sporangiophores approach even nearer than they were previously known to do to those of the living Equisetums, in corresponding parts of which similar clusters of tracheids exist. At g, g are transverse sections of the bracts of the next inferior verticil of the sterile organs, and at g', g' tips of a yet lower verticil of similar organs.

On studying a number of slides prepared for me by my active auxiliary, Mr. ISAAC EARNSHAW, of Oldham, I found in several of them sections of fragments of a

Z. elliptica. In the specimens which I first described, this part of the cortex had invariably disappeared. But I have more recently obtained specimens in which this inner cortex, with its characteristic gum-canals, is preserved; as is also the case in the specimen described above, where the layer in question is indicated by b.—February 12th, 1889.

plant differing materially from any that I have hitherto described. The fragments are very Protean in form and structure, though possessing certain remarkable features in common.

Fig 9 (Plate 3) represents one of the most characteristic of these. It is a section of a branching stem or rhizome of the plant enlarged 14 diameters. At A the section has crossed a branch obliquely, revealing a central vascular axis, *a*, composed of very fine vessels of the reticulated or pitted type, some of which are fully ·01 of an inch in diameter. The inner cortex, b', is composed of longitudinal lines of parenchymatous cells with transverse septa. At B is a bifurcating branch, with its vascular axis *a* also bifurcating, composed of a dense mass of vessels partly barred, but some of which are reticulate, like those of A, *a*, though of smaller diameter, some of them not being more than ·00125 of an inch in diameter. The inner cortex, b'', resembles that of A, *a*, but the cells are of much smaller dimensions. More externally we have at b' a dense cortical zone composed of elongated prosenchymatous cells. A small branch with a central vascular bundle appears to have been given off vertically at *c* ; there seems to have been a similar one at *d*, and there may possibly have been a third at *e*. F is obviously a tranverse section of either a branch or a root, the vascular bundle of which occupies its centre surrounded by an inner cortex, enclosed within a more dense external one. The two external surfaces, g' and *g*, are densely clothed with numerous very large, curved multicellular hairs. The basal cells of some of these hairs are fully ·005 of an inch in diameter, whilst some of them are fully ·014 of an inch long. Conspicuously cylindrical throughout the greater part of their length, they are tapering, slender. Fig. 10 furnishes a carefully drawn representation of these hairs, as they appear at B *g*, g', enlarged 43 diameters.

Fig. 11 (Plate 2) is a transverse section of one of these stems enlarged forty-four diameters, its mean one being ·009 of an inch. In its centre, *a*, is a cluster of tracheids, the entire cluster being about ·02 of an inch in diameter, enclosed within *b*, which appears to occupy the position of a true bundle-sheath. These tracheids are reticulated like those of fig. 9. A narrow zone of delicate parenchyma, *c*, lies between the tracheids and the supposed bundle-sheath, which may either have a circumferential phloëm or a procambial tissue. The inner cortex, *d*, consists of a very regular thin-walled parenchyma, which, in turn, is invested by *e*, a coarser prosenchymatous tissue. Externally to this prosenchyma are numerous transverse isolated sections of hairs, *f*, of varying diameters. The most striking feature of this section consists of the remains of four radiating appendages *g*, g', g'', g''. The most perfect of these is *g*, in which we discover the central vascular bundle *a* invested by the two cortical layers d' and e'. These radiating appendages, taking their rise from what has been either the pericambium or the endoderm, and forcing their way through the cortical tissues of the primary axis without receiving any contributions from those tissues, can, I think, only have been either roots or secondary rootlets.

Fig. 12 is an oblique transverse section through a stem similar to fig. 11, the lower end of which displays features identical with those of the latter figure. It exhibits three of the radiating rootlets (?) *d*, *d*, and *d'*; but *d* is fortunately intersected transversely, and enables us to identify several isolated transverse sections of these roots scattered through some of my slides, three of which are figured in figs. 13, 14 (Plate 4), and 15 (Plate 1).

Fig. 13 has a mean diameter of ·0025 of an inch; fig. 14 of ·01, and fig. 15 of ·025. In each of these examples we have the conditions seen in fig. 11, *g*, viz., a central bundle, *a*, invested by the two cortical zones *d'* and *e'*, as well as the apparent bundle-sheath *b*, enclosing the phloëm *c*. In fig. 15, which seems to represent a younger but smaller rootlet, the separation of the cortex into two zones is less distinct than in figs. 13 and 14. Its component cells also exhibit the tendency to arrange themselves in the concentric cycloidal circles so common amongst young rootlets of this character. The vascular bundles of all these rootlets are of the diarch type, though possibly they may also be regarded, both in structure and development, as resembling the concentric bundles of Lycopodiaceous stems.

This plant, which I propose to distinguish as *Rachiopteris hirsuta*, is wholly distinct from any which I have hitherto described. Can it be identified with any living type? The young branches of the living *Marsileæ*, *M. quadrifolia* and *M. salvatrix*, are clothed with hairs absolutely identical with those of fig. 10, and longitudinal sections of these branches display similar irregularities of ramifications to those shown in fig. 9; rootlets, branches, and bases of fronds being alike cut through in sections made in almost any one plane, and their rootlets also remind us strongly of those seen in figs. 11, 12, 13. Without attaching an undue importance to these resemblances, the sections of *Rachiopteris hirsuta* undoubtedly suggest closer relationships with the *Marsileæ* than with any other living plants with the organisation of which I am familiar. The specimens described are from the Halifax deposits.

I have at various times discovered other forms of roots or rootlets in these Halifax Carboniferous beds, some of which at least are sufficiently interesting to be put upon record, as showing the early period at which certain types of these organs made their appearance on the earth. The first of these I discovered in some slides also prepared for me by Mr. ISAAC EARNSHAW, of Oldham.

Fig. 16 (Plate 4) represents a longitudinal section through a very delicate root, of which I have a number of fragments. Their most characteristic feature resides in the circumstance that their secondary rootlets are given off in numerous verticils, *c*, *c*. The cellular parenchyma of which they consist exhibits extremely limited differentiation. A primary vascular bundle, *a*, composed of barred vessels, runs down the centre of the primary axis; and secondary ones, composed of vessels of smaller dimensions, *a'*, *a'*, bend downwards and outwards into the secondary rootlets. The parenchyma, *b'*, immediately surrounding the primary bundle consists of long, narrow, thin-walled cells, which are invested by an external bark, *b*, composed of a larger and more strongly marked form of parenchyma.

I have several transverse sections of this root in my cabinet, one of which is represented in fig. 17 (Plate 4). In its centre is a vascular bundle, *a*, having a somewhat irregular triangular section. This is invested by a zone of delicate parenchyma, *b*. An enlarged representation of the transverse section of this bundle is given in fig. 18 (Plate 3). On the upper side of fig. 17 are long sections of five, *c, c, c, c, c*, of the radiating secondary rootlets of the vertical axis from the cortical layer *b′*, of which they are extensions. Two similar specimens indicate that there were from ten to eleven rootlets in each such verticil. As the cells of the cortex *b* are prolonged into each rootlet they become elongated radially. Fig. 18 indicates that the central bundle of fig. 17, here further enlarged, is a triangular one. Fig. 19 represents a similar bundle of another transverse section like fig. 17, and enlarged equally to fig. 18. It exhibits in a similar way the bundle *a*, surrounded by its investment of either procambium or phloëm. It is obvious, therefore, that the transverse section of the primary bundle of this root was triangular, presenting at least all the essential characteristics of a triarch root. I would distinguish this plant, which, like that last described, is also from Halifax, by the provisional name of *Rhizonium verticillatum*.*

Whatever may be the case with figs. 17 and 18, I think there can be no doubt that fig. 20 is a transverse section of a true triarch root, enlarged 42 diameters. Fig. 21 represents the centre of the same section still further enlarged; *a* is a triarch vascular bundle; *b*, a concentric phloëm; *d*, cycloidally arranged cells of the cortex.

Fig. 22 (Plate 3) is an oblique transverse section of a cylindric rootlet of another description; at *a* a few vessels or tracheids occupy the centre of a mass of delicate elongated cells, either representing phloëm or procambium. The middle cortex, *b*, consists of a loose form of parenchyma, enclosing numerous irregular large lacunæ of the type so common in the roots of the Nympheæ. Of course, this indicates no systematic relation with the latter plants, beyond the fact that our fossil root most probably belonged to some aquatic or semi-aquatic type. At *c* is a compact quasi-epidermal investing layer of cells. The specimen, which is from one of the Oldham nodules, may be designated *Rhizonium reticulatum*.

Fig. 23 (Plate 3) is a somewhat larger rootlet, also with a lacunar cortex. A strongly defined vascular cylinder, *b*, encloses a delicate medullary parenchyma *a*, and is invested by a compact zone consisting of several cycloidally arranged rows of

* I was at first inclined to include these root-like objects in my provisional group of *Rachiopteris;* but, since they present few or no indications of being either rhizomes or petioles, I have determined to utilise CORDA's term *Rhizonium* for them. CORDA's objects, to which he gave this generic name, he appears to have regarded as being probably the roots of Orchidaceous plants; but they are really undistinguishable from the rootlets of *Stigmaria ficoïdes*. CORDA's definition of his genus, "Radiculæ parasiticæ, intertextæ, cortice parenchymatosa; fasciculo vasorum solitaris centrali vagina propria incluso" ('Flora der Vorwelt,' p. 46), is, with the exception of the two first adjectives, fully applicable to my plants.—February 12th, 1889.

endodermal cells, *c*. Numerous thin vertical plates, *d*, chiefly but a single row of cells in thickness, radiate from the zone *c* to the thicker peripheral one *f*, encircling numerous large, radially-disposed lacunæ. Every feature of this apparent root reminds us of the structure of the cortex of *Asterophyllites Williamsonis*, described in my memoir, Part XII.* Whether or not the two belong to the same plant cannot at present be determined. The specimen, which is from Halifax, may be named *Rhizonium lacunosum*.

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* 'Phil. Trans.,' 1883.

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IV. *On the Electromotive Changes connected with the Beat of the Mammalian Heart, and of the Human Heart in particular.*

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Communicated by Professor BURDON SANDERSON, *F.R.S.*

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PART I.

§ 1. *Introduction.*

In our investigation of the action of the excised Mammalian heart,* Dr. REID and I left undetermined certain points relating to its electromotive variations, more especially those which accompany the spontaneous beat of the excised organ.† The nature and direction of deflections were very variable, and indicated no regular origin or mode of progression of the excitatory process. In 62 observations we observed in 17 cases apex negativity alone, in 17 base negativity alone, in 16 apex followed by base negativity, in 12 base followed by apex negativity. We then remarked that the numerous irregularities met with in experiments upon the excised Mammalian heart were presumably due to irregularities and inequalities of tissue in the dying organ, which might have been due to differences of temperature, or to accidental injuries, &c.; but we were unable to verify the supposition by any experimental reproduction

* 'Phil. Trans.,' B., 1887, p. 215.

† *Loc. cit.*, p. 234.

of these irregularities. Most of the observations reported in Part I. had for their object to clear this part of the subject as far as possible to me.

§ 2. *Experimental Modification of the Electrical Variation connected with the Spontaneous Beat.*

The methods followed have been in the main those described in the paper already referred to,* with certain modifications of detail, such as the use of D'ARSONVAL'S chloride of silver electrodes (which proved to be convenient and excellent for the purpose in view), and with this difference, that, in order to examine the as far as possible intact and uninjured organ, the heart was examined *in situ*, the thorax being laid open and its walls fixed to a board immediately after the decapitation of the animal. The heart, having been examined *in situ*, was then excised and re-examined electrically.

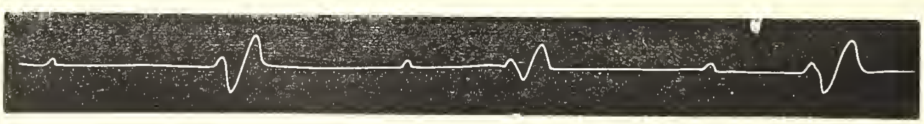
Experiment 1.—*Kitten's heart.* March 31st, 1888.

	Time after death.	A to Hg.	A to H_2SO_4.
	min.	var.	
Spontaneous beat	5	SN	
Excited beat (exc. of apex)	..	SN	
Spontaneous beat after injury of base	10	S	
Heart excised—			
	..	S	
Excited beat (exc. of base)	..	NS	
,, (exc. of apex)	..	SN	
Spontaneous beat	20	SN	
,, after injury of apex	..	N	

When electrode B was in contact with the auricle, which was beating twice to each ventricular beat, the variation was of the following rhythm:—

$$n \quad nSN \quad n \quad nSN \quad n \quad nSN,$$

or, expressed graphically,



When electrode B was in contact with the base of the ventricle the variation was of the rhythm SN . . . SN, the electrometer not being influenced by the auricular contractions.

* *Loc. cit.*, p. 235 (heart led-off to electrometer from two points A and B).

With { A to H_2SO_4 / B to Hg }	variation N signifies A negative to B.						
	,,	S	,,	B	,,	A.	
With { A to Hg	,,	N	,,	B	,,	A.	
B to H_2SO_4 }	,,	S	,,	A	,,	B.	

Experiment 2.—*Kitten's heart.* April 1st, 1888.

	Time after death.	A to Hg.	A to H_2SO_4.
	min.	var.	
Spontaneous beat.	9	SN	
Heart excised—			
Spontaneous beat	..	*s*N	
,, after injury of apex	..	N	
,, ,, ,, and of base .	..	*s*N	

Experiment 3.—*Cat's heart.* April 21st, 1888. Death by decapitation. Five minutes after death the apex of the heart, consisting of the left ventricle, was pulseless and in firm rigor; the base of the heart, consisting of the right ventricle, was at the same time regularly contracting about 30 per minute; the auricles were also regularly contracting at a rate of 120 per minute; this condition was observed till 20 minutes *post mortem.*

Apex to Hg.	Base to H_2SO_4.	Variation N.
Liver to Hg.	Base to H_2SO_4.	,, N.
Neck to Hg.	Base to H_2SO_4.	,, N.
Neck to Hg.	Auricle to H_2SO_4.	,, *n*.
Apex to Hg.	Neck to H_2SO_4.	,, 0.

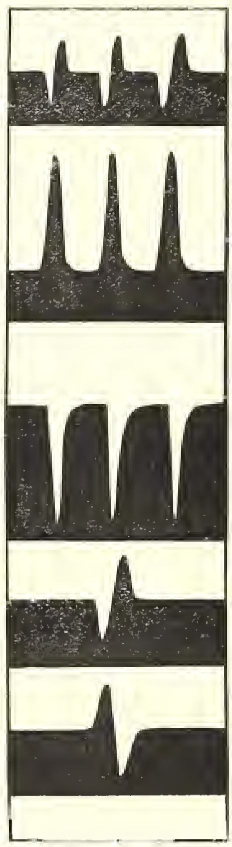
Remarks.—The condition of the ventricles was such that the right ventricle formed a loose pulsating pouch, connected with the upper two-thirds of the firmly contracted left ventricle. The contractions of the right ventricle were regular but small, and visible only at the basal part; the electrometer indicated negativity of the contracting portion. If, with one leading-off electrode applied to an indifferent part, the other leading-off electrode was shifted to a distance from the actually contracting portion, the excursion was quickly lessened and lost; if it was shifted to the auricle, the N variation of a ventricular rhythm gave way to the much more frequent variation *n* of auricular rhythm.

Nothing can be clearer than these effects of injury at base and apex respectively. The diphasic variation SN (viz., apex negativity followed by base negativity) is, in consequence of injury of the apex, converted into the monophasic variation N (unbalanced negativity of base). After a time the diphasic variation SN re-appears, and now it is converted into the monophasic variation S (unbalanced negativity of apex) in consequence of injury of the apex. These facts, illustrated in fig. 1, are precisely similar to those observed by BURDON SANDERSON and PAGE * upon the ventricle of the Frog and Tortoise, the only difference being in the nature of the normal variation antecedent to injury.

* 'Journal of Physiology,' vol. 2, p. 418; vol. 4, p. 335.

The early monophasic variation which Dr. REID and I had so frequently under observation with the Mammalian heart immediately after excision was probably of this nature; it is, indeed, "the expression of local predominance of a change taking place throughout the whole ventricle,"* but our opinion concerning it, to the effect that the single variation is proof of a practically single and simultaneous change

Fig. 1.



Nature of beat.	Electrical variation.	
1. Normal spontaneous . .	SN	Apex negativity, followed by base negativity
2. Spontaneous with injured apex	N	Unbalanced negativity of base
3. Spontaneous with injured base	S	Unbalanced negativity of apex
4. Excited from apex . . .	SN	Apex negativity, followed by base negativity
5. Excited from base . . .	NS	Base negativity, followed by apex negativity

(Apex of heart to mercury of electrometer. Base of heart to sulphuric acid of electrometer.)

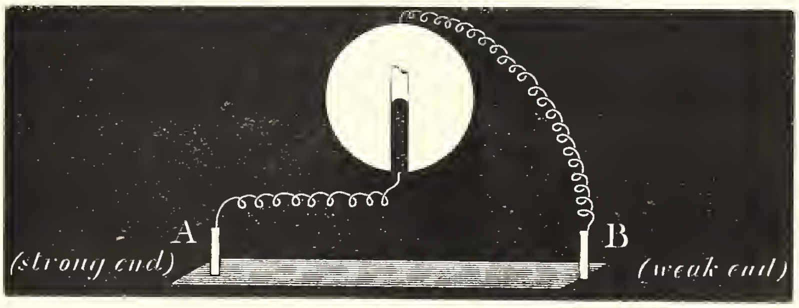
taking place throughout the ventricle, is no longer justified. The diphasic variation can be, as above described, rendered monophasic by injury on the Mammalian, as on the Batrachian, heart; bearing in mind the extreme susceptibility of the Mammalian heart, we must regard as highly probable that a monophasic variation is a consequence of injury, and that the normal variation is diphasic.

Confirmatory experiment.—That a monophasic variation is no evidence of simul-

* *Loc. cit.*, p. 241.

taneity of action throughout a contractile mass, but that it may depend upon unbalanced, and therefore predominant, negativity of a less injured and literally "stronger" portion of tissue, is very clearly shown with a strip cut from a fresh ventricle. It is not difficult in this case to combine mechanical with electrical exploration. Two levers resting upon the ventricle-strip record the passage of the wave of contraction, and the strip is at the same time led off from each end to the electrometer. A stimulus is applied to one end, the contraction passing along the strip is recorded by the two levers, and the electrical variation is watched in the electrometer. Usually the strip will be found not to contract equally strongly in its whole length, but one end gives a stronger contraction than the other. With this inequality of contraction a concordant inequality of electrical action is observed; the variation is not diphasic, but monophasic, indicating a predominant negativity of the more strongly contracting part, while by the asynchronism of the two levers we obtain ocular evidence of the passage of a wave of contraction. Sometimes the effect is the same whether the contraction be started from the stronger or from the weaker end; only in the first case the single phase is an intensified first phase, in the second case it is an intensified second phase. Sometimes it happens that the inequality of tissue is of such a degree that by excitation started at the "strong" end of the strip a monophasic variation is obtained, while by excitation at the "weak" end a diphasic variation is obtained, consisting of a minor first phase (negativity of the weak end), followed by a major second phase (negativity of the strong end).

Fig. 2.



Excitation of A gives Variation S (predominant 1st phase).
" B " " S (predominant 2nd phase);
or nS (minor 1st phase; major 2nd phase).

The phenomena observed on the excised isolated auricle are of a similar character. Adopting the disposition above described, we may witness the passage of a wave of mechanical contraction attended by a monophasic variation, due to injury of its ventricular portion and consequent unbalanced negativity of the appendicular end.

What is the order of occurrence of the two phases?

Examination of the uninjured heart *in situ* shows that in the majority of cases it is (1) negativity of apex followed by (2) negativity of base. Contrary to the case of the Frog, in which the normal variation has its initial phase at the base and its terminal phase at the apex of the ventricle, the normal variation of the Mammalian ventricle exhibits an initial phase at the apex and a terminal phase at the base. In our previously quoted paper on the excised Mammalian organ, Dr. REID and I stated (p. 230) that "in the spontaneous beat of the excised organ the contraction of the apex generally appears to precede that of the base." Out of 25 observations,* in 17 the mechanical effect of contraction manifested itself at the apex first, in 2 at the base first, in 6 there was no appreciable difference. Taken by themselves, these observations went to show that the contraction of the Mammalian heart normally commences at the apex. But the electrical observations by which we sought to confirm this testimony obtained by the mechanical method failed entirely and obliged us to state† "that, as regards the electromotive changes with visible spontaneous beats, our results show no uniformity; we can find in them no evidence either for or against the results we obtained by the graphic method." Observation of the electrical variation of the heart beating *in situ* shows it to be, *in the majority of cases examined* (11 out of 17), composed of (1) negativity of apex, (2) negativity of base; having regard to the fact that the organ is unstable and dying, we may expect to meet with exceptions to the rule, which, although by no means invariable, has been frequently enough verified to allow us to say of the Mammalian ventricle "apex first" with nearly as much certainty as we say of the Frog's heart "base first." As will be shown in Part II., the electrical phenomena of the Human heart afford strong confirmation of this view.

I must admit, however, that these observations on the exposed organ *in situ* have been the most troublesome and unsatisfactory in respect of their irregularities: six exceptions as compared with eleven "regular" results is a considerable proportion, and I have, therefore, sought by further observations to realise the effect of modifying circumstances, and the possible sources of irregularities. I will deal with these points *seriatim*.

A source of fallacy.—A possible source of an error of observation arises from the application of electrode B in close proximity to the auriculo-ventricular groove or in actual contact with the auricle. Under these circumstances, the auricular contraction may influence the electrical reading, which must not therefore be attributed to the ventricle alone; an electrometer reading in reality due to auricular followed by ventricular negativity might be taken to represent basal followed by apical negativity of the ventricle alone. If the auricles should be beating with a more rapid rhythm than the ventricles (as commonly occurs in the moribund Mammalian heart), there will obviously be no danger of confusing auricular with basal negativity; but, if auricles and ventricles should be beating in regular sequence, it is necessary to be on guard

* *Loc. cit.*, p. 249, Table H.

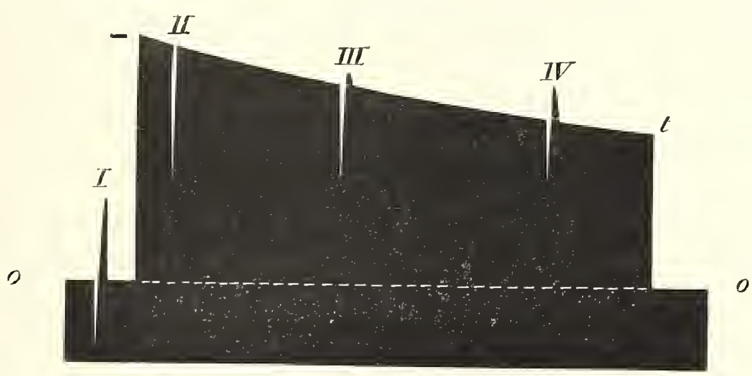
† *Loc. cit.*, p. 234.

against this possibility, which will be particularly misleading if the true basal phase should happen to be weak or absent, but which may also confuse the reading of a normal ventricular variation. An illustration of this point is given in the note to Experiment 1.

The origin of the excitatory process can be experimentally determined.—Apart from the fact that true stimuli are capable of starting the excitatory process from any part of the ventricle, I have found that it is possible by local alteration of temperature to determine the origin of a series of contractions. An excised ventricle which has become quiescent can be made to resume rhythmic contractions by raising its temperature. If this be done in such a way that the apex is more warmed than the base, the diphasic variation at each contraction has directions denoting origin at apex; if in such a way that the base is more warmed than the apex, the directions of the diphasic variation indicate origin at base.

Spontaneous modification of the ventricular variation.—It commonly happens that an original monophasic variation gradually gives place to a diphasic variation; this change may be attributed to either of two causes: (1) to the subsidence of injury at the injured lead-off, or (2) to the development of injury at the normal lead-off.

Fig. 3.



It appears to me probable that both these causes may play a part in producing the effect in question; as regards the first cause, we have in the Mammalian, as in the Batrachian heart, a rapid decline of the negativity manifested immediately after injury; the negativity is doubtless an expression of chemical activity at and near the injured zone, or, in other words, of a state of continued excitation; immediately after injury the degree of the alteration is such as to leave no margin for the manifestation of the excitatory effect (negativity) at the part, and an unbalanced negativity of any other normal part is witnessed in the shape of a monophasic variation when the organ contracts. The alteration is at first at a maximum, and gradually subsides until it leaves a margin of susceptibility within which excitatory effects (negativity) can be evidenced, and now we witness a diphasic variation composed of the preponderant negativity of a normal part and of the recovering negativity of the injured part. It is convenient to allude to these two phases as the *major* phase and the *minor* phase respectively.

In the diagram, fig. 3, *o . . . o* denotes the iso-electric state of two led-off points, A and B; the ordinate *o* — denotes the maximum negativity of B manifested immediately after injury; the line — *t* denotes the gradually declining negativity of B.

I. represents a normal diphasic variation.

1st phase, A negative. 2nd phase, B negative.

II. represents a monophasic variation after injury of B; unbalanced negativity of A.

III. and IV. represent diphasic variations re-appearing as the negativity of B declines.

1st phase, A negative (major phase).
2nd phase, B negative (minor phase).

The facts of experiment are in complete agreement with this theoretical representation. With a normally beating heart *in situ*, led off from the apex to the mercury of the electrometer, from the base to the sulphuric acid, the level of the mercury in the capillary showed that apex and base were iso-electric in the intervals between the beats, each of which was accompanied by the double variation SN, signifying:—

1st phase, apex negative.
2nd phase, base negative.

The base was now injured by crushing with forceps; on re-applying the electrode to the injured base, the mercury in the capillary came to rest in the diastolic period much nearer the end of the capillary (*i.e.*, North in the field of the microscope), indicating negativity of the base; each beat was now accompanied by a single variation S, indicating negativity of the apex. Ten minutes later the mercury had subsided South (indicating declining negativity of the base); each beat was now accompanied by a variation S*n*, signifying:—

1st phase, apex negative (major phase).
2nd phase, base negative (minor phase).

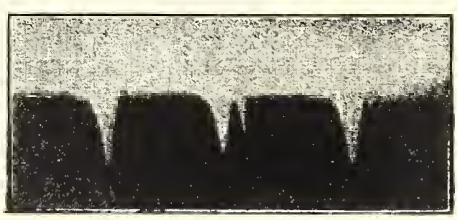
Ten minutes later the variation was still diphasic; but the 1st phase had diminished, while the 2nd phase had increased.

The changes accompanying the subsidence of injury negativity do not always follow the above regular form; in some cases the heart dies so rapidly that spontaneous beats giving the double phase do not re-appear, though mechanical excitation may still be capable of producing a contraction marked by a double phase; in other cases the double phase re-appears at first in an intermittent manner, most beats being still marked by a monophasic and only a few by a diphasic variation, which, as time goes on, becomes gradually more frequent until it is established as the regular accompaniment to every beat.

Effects of double injury.—As stated above, the conversion of a diphasic into a monophasic variation can be effected at will by injury, and I have just said that the monophasic variation thus effected may gradually give place to a diphasic variation ; it remains to add that this may sometimes be done at once by a second injury. Thus, for instance, in a heart giving a spontaneous variation SN (apex negative, base negative), this was replaced by a variation N (unbalanced negativity of base) after injury of apex, and this again was at once replaced by a variation SN after a subsequent injury of the base (*vide* Experiment 2). Apparently, the balance between the two led-off parts can be at least partially restored when, after one part has been injured, the second is similarly injured.

A triphasic ventricular variation.—The ventricular variation sometimes appears to be triphasic, and one might at first sight interpret it as being due to auricular negativity followed by the ordinary diphasic variation. This, doubtless, does frequently give a variation of such a character, but in certain cases a treble variation is undoubtedly caused by the ventricle alone. A variation *n*SN (apex to H_2SO_4; base to Hg) or *s*NS (apex to Hg ; base to H_2SO_4) cannot be auriculo-ventricular, but must be an irregularity such that negativity of the apex manifests itself twice, once at the beginning and once at the end of the contraction of the ventricles. The most probable interpretation appears to me to be that we have to do here with a case of injured base ; apex negativity manifests itself first and is not overcome by subsequent base negativity, which is only sufficient to more or less interrupt the predominant apex negativity ; the latter thus appears to be twice manifested. The following photographic observation is one among several others which can, it seems to me, be thus explained.

Photo. 1.



B*x* A*x* S*p*

←

Experiment 4.—*Kitten's heart*, excised 15 min. *post mortem*, April 13, 1888.

Apex to Hg. Base to H_2SO_4.

S*p* = a spontaneous variation *sn*S.

A*x* = a variation caused by mechanical excitation of the apex, *sn*S.

B*x* = a variation caused by mechanical excitation of the base, *n*S.

Another instance of the same kind, but in which negativity is twice marked at the base, is given in the note to Experiment 5.

It is possible that in some cases differences of temperature are accountable for irregular variations of this character. If, for instance, the base should happen to be warmer than the apex, the negativity of the latter will outlast that of the former, and, if an excitatory process should begin at the apex, it will possibly be twice manifested, once before the beginning and once after the termination of basal negativity. *Vice versâ*, if the apex should happen to be the warmer, an excitatory process commencing at the base might be twice manifested at each ventricular contraction. Obviously, these suppositions require to be submitted to the test of experiment, and I intend to do so as soon as time will allow. At present, however, as will be seen, I am led to pursue the phenomena in another direction, and I have mentioned the supposition now only for the sake of completeness.

§ 3. *Observations on Animals with One or Both leading-off Electrodes applied to the Body at a Distance from the Heart.*

The observations to be described in this section lead up to those which will be discussed in Part II. They are the steps by which I gradually learned on animals what parts of the body are equivalent to leads-off from base and from apex of the ventricles. Instead of exposing the heart and leading off from it by both electrodes, I led off by only one electrode from the exposed heart and by the other from various distant parts of the body; finally, I led off by both electrodes from various distant points on the intact animal.

Experiment 5.—*Cat.* April 12, 1888. Death by decapitation. Five min. *post mortem*, heart exposed. Electrode A from apex to Hg. Electrode B from stump of neck to H_2SO_4.

Spontaneous variations, SN alternating with SNS. Electrode B shifted from neck to base of heart.

Spontaneous variations, SN alternating with SNS. Thus it appears that the lead-off from the neck was equivalent to a lead-off from the base of the heart.

NOTE.—The alternation, SN and SNS, noticed in this case was observed to coincide with a well-marked bigeminal character of the contractions of the heart, which was beating slowly. The contraction of the base of the heart, upon which electrode B was applied, was evidently stronger at each SNS variation than at each SN variation. (S indicates base negative, N indicates apex negative.) Thus in this case the excitatory process originated at the base, and at every other beat when the base contracted more strongly, the negativity was twice manifested at the base. I have several times noticed this form of electrical disturbance.

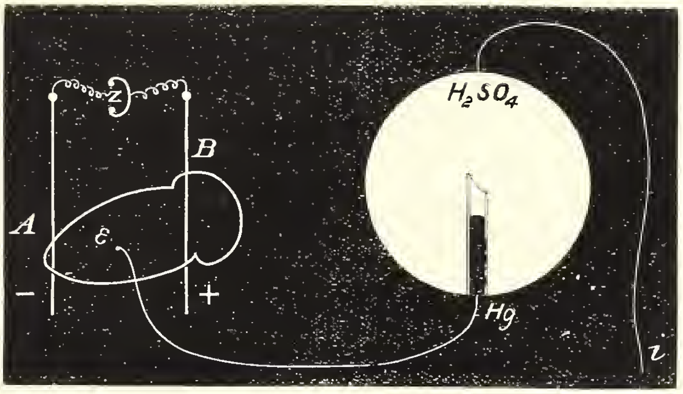
Experiment 6.—*Cat.* April 17, 1888. Killed by decapitation; heart exposed.

Apex to	Hg. and	Base to	H_2SO_4.	Variation	N.
Apex to	,, ,,	Mouth to	,,	,,	N.
Base to	,, ,,	Mouth to	,,	,,	NS.
Apex to	,, ,,	Mouth to	,,	,,	N.
Base to	,, ,,	Mouth to	,,	,,	NS.
Mid ventricle to	,, ,,	Mouth to	,,	,,	NS.
Apex to	,, ,,	Base to	,,	,,	N.
Mid ventricle to	,, ,,	Base to	,,	,,	NS.
Apex to	,, ,,	Base to	,,	,,	N.
Apex to	,, ,,	Mid ventricle to	,,	,,	N.

Here again it appears very plainly that a lead-off from the mouth is equivalent to a lead-off from the base of the heart.

Complementary experiment.—In order to obtain some idea of the distribution of potential in surrounding parts when different portions of the heart are at different potentials, I took observations with the capillary electrometer, leading off from various parts of the body, and leading in the current of a Daniell cell by pins transfixing the heart at base and apex (*vide* fig. 4). The observations were taken on animals the day after death, left exactly in the position in which their hearts had been examined on the previous day.

Fig. 4.



Experiment 7.—*Cat.* April 17, 1888. Twenty-four hours after death. Heart transfixed at apex and base by pins constituting the electrodes of a Daniell cell. Direction of current as in diagram above. ϵ and i are the leading-off electrodes to mercury and sulphuric acid. The Daniell circuit is made and broken by a spring key, and the consequent variations of the electrometer observed when ϵ and i are applied to various parts of the body. The results are as follows :—

$$\text{with}\left\{\begin{array}{l}\epsilon\text{ near A}\\ i\text{ near B}\end{array}\right.\text{ the variation was S, that is, }\left.\begin{array}{r}-\\+\end{array}\right\}$$

$$\text{with}\left\{\begin{array}{l}\epsilon\text{ near A}\\ i\text{ in mouth}\end{array}\right.\quad ,, \quad ,, \quad \text{S,} \quad ,, \quad \left.\begin{array}{r}-\\+\end{array}\right\}$$

$$\text{with}\left\{\begin{array}{l}\epsilon\text{ to leg}\\ i\text{ in mouth}\end{array}\right.\quad ,, \quad ,, \quad \text{S,} \quad ,, \quad \left.\begin{array}{r}-\\+\end{array}\right\}$$

$$\text{with}\left\{\begin{array}{l}\epsilon\text{ near B}\\ i\text{ in mouth}\end{array}\right.\quad ,, \quad ,, \quad \text{N,} \quad ,, \quad \left.\begin{array}{r}+\\-\end{array}\right\}$$

$$\text{with}\left\{\begin{array}{l}\epsilon\text{ near B}\\ i\text{ at leg}\end{array}\right.\quad ,, \quad ,, \quad \text{N,} \quad ,, \quad \left.\begin{array}{r}+\\-\end{array}\right\}$$

These variations were precisely such as might have been anticipated from theoretical considerations; they were reversed with reversal of the Daniell current; they are confirmatory of the supposition above made that a lead-off from the mouth is equivalent to a lead-off from the base, and that a lead-off from the lower extremity is equivalent to a lead-off from the apex. Experiments of a similar character with single induction shocks gave precisely similar results.

Experiment 8.—*Kitten.* April 21st, 1888. Death by chloroform.

1. Mid ventricle to Hg.	Mouth to H_2SO_4.	Variation	*n*S.
2. Mid ventricle to Hg.	Leg to H_2SO_4.	,,	*n*S.
3. Leg to Hg.	Mouth to H_2SO_4.	,,	*s*N.
4. Auricle to Hg.	Leg to H_2SO_4.	,,	*s* and *ss* or *sn*S.

This experiment presents several points of interest. The third observation is the first which I have made upon an animal leading off the heart from two remote points, viz., leg and mouth. The variations with each contraction of the heart were small, but unmistakable. (It should be remarked that the chest was open and that circulation had ceased, the animal having been dead about ten minutes.) The variation was such as to indicate—

1st phase. Negativity at leg.
2nd phase. Negativity at mouth.

As will presently be shown, we have reason to admit the leg as indicator of the apex potential, the mouth as indicator of the base potential. Thus we have in this case—

1st phase. Negativity of apex.
2nd phase. Negativity of base.

In the fourth observation the auricles were contracting twice to each ventricular contraction; when the auricle contracted the variation was *s*, indicating negativity of auricle; when the auricle and ventricle contracted in sequence the variation was *s*S or *sn*S,* indicating

1. Negativity of auricle, viz., action of auricle.
2. Negativity of leg, ,, ,, of ventricle apex.
3. Negativity of auricle, ,, ,, of ventricle base.

Observations 1 and 2 were defective, inasmuch as the leading-off electrode from mid ventricle was shifted between the two observations.

Complementary experiment.—The animal was left *in statu quo*, and two hours later differences of potential were artificially established by pins inserted into the heart, through which were passed make or break induction shocks, or the direct current of a Daniell cell (*vide* fig. 4). The results were as follows:—

With induced currents. (Make current from apex to base; break current from base to apex.)

			Variations.
Mouth to Hg.	Leg to H_2SO_4	At make	S.
		,, break	N.
Leg to Hg.	Mouth to H_2SO_4 . . .	,, make	N.
		,, break	S.
Leg to Hg.	Heart near base to H_2SO_4	,, make	N.
		,, break	S.
Leg to Hg.	Heart near apex to H_2SO_4	,, make	S.
		,, break	N.

With the constant current. (Current from apex to base through heart.)

		Variation at closure.
Leg to Hg.	Mouth to H_2SO_4	N.
Leg to Hg.	Heart near base to H_2SO_4 . .	N.
Leg to Hg.	Heart near apex to H_2SO_4 . .	S.
Mouth to Hg.	Heart near base to H_2SO_4 . .	N.
Mouth to Hg.	Heart near apex to H_2SO_4 . .	S.

On reversal of the current the variations were reversed.

The whole series of these variations was exactly as might have been anticipated. They were uniformly such as to indicate potential of the same sign in the vicinity of either pole as compared with the potential at more distant parts. It is worth again calling attention to the fact that potential at leg agreed with sign of pole at apex, while potential at mouth agreed with sign of pole at base, when the body was led off

* It is often very difficult by inspection to distinguish between such variations; in the above case it was impossible to say whether the variation consisted of two movements in the same direction (*s*S), or of two movements in the same direction separated by a movement in the opposite direction (*sn*S); the latter is probably the correct reading.

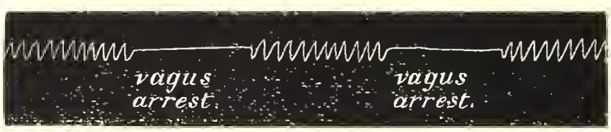
to the electrometer from leg and mouth. And I may add that I also experimentally verified the fact that, while potential at mouth agreed with sign of pole at base, potential at rectum agreed with sign of pole at apex.

In the preceding experiments the electrical variations of the heart were observed by leading off from remote points with the thorax opened, and with the heart therefore lying in contact with the tissues by its posterior surface only. The next step was to determine whether the variations can be observed on the intact animal, with the heart in contact with its normal surroundings.

Experiment 9.—*Cat.* April 23rd, 1888. Death by chloroform. Led off to the electrometer by electrodes in the mouth and in the vagina. Variations observed synchronous with the heart's beat, but too rapid to allow their character to be determined. Apparently each variation was double, but it was impossible to tell which was the first and which was the second phase, the rhythm being—

. . . . . . *snsnsns* . . . . . .

Both vagi exposed, isolated, and divided. Excitation by induced currents of either vagus abolished the variations, the right vagus being in this respect more efficacious than the left. After each period of arrest, the first movement of the mercury in the electrometer was closely watched; it was southwards (with mouth to H_2SO_4 and vagina to Hg); graphically expressed, the effects were—



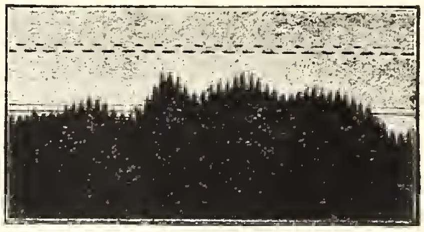
Similar results were obtained with the H_2SO_4 electrode transferred to the eyeball and with the subsequent transfixion of the heart by a pin connected with the Hg of the electrometer. This was between ten and fifteen minutes *post mortem*. The heart was now exposed by opening the thorax, and vagus effects were repeatedly obtained up to about half an hour *post mortem*, excitation of the right vagus being uniformly the more effectual. Towards the end of the experiment the following point was noted: excitation of the right vagus arrested the movements of the auricles and of the ventricles; excitation of the left vagus arrested the movements of the ventricles, and not those of the auricles, which continued to pulsate.* In both cases the move-

* This was one among a considerable variety of effects which vagus stimulation may produce upon the contractions of the Mammalian heart *post mortem*. I have seen vagus stimulation under these circumstances entirely without effect upon any of the four chambers, or followed by complete arrest of the whole organ, or by arrest limited either to the auricles or to the ventricles. I have also seen a delirium cordis entirely uninfluenced by, or entirely suspended during, vagus stimulation.

ments in the electrometer were arrested, showing that they were due to ventricular, and not to auricular, contraction.

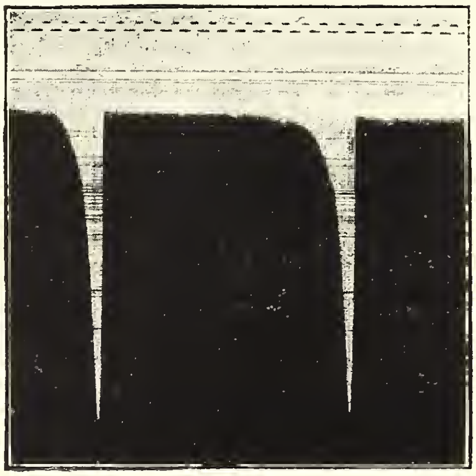
Experiment 9 is, in two respects, a typical one: it is representative of many others which I have made, in which the heart is led off from the mouth or from an eye, and from the rectum or from an inferior extremity; electrical variations are thus unfailingly demonstrated of a character which is illustrated in photo. 2.

Photo. 2.



Electrical variations of Cat's heart, with body intact, and led off from mouth to Hg, from rectum to H_2SO_4.

Photo. 3.



Cat's heart, exposed immediately after death by chloroform, and injured at the apex by crushing. Led off from auricle to Hg, and from apex of ventricle to H_2SO_4. Variations *s*S*sss*S (read from right to left; $s =$ negativity of auricle, $S =$ negativity of base of ventricles).

It also shows that the variation observed with peripheral leading-off points is exclusively ventricular. I have since repeatedly observed (with special facility when the auricles and ventricles have happened to beat at different rates) that, for the demonstration of any electrical change accompanying auricular contraction, it is necessary that one of the electrodes should be in actual contact with the auricles, and that, as soon as it is shifted to a distance, the auricular variation is lost. Photo. 3, taken with one electrode in contact with the auricle while the other was applied to the apex of the ventricle, shows the comparative effects of auricular and ventricular events; it will not be surprising that, if both electrodes be applied to the periphery, only the latter event should be manifested.

With respect to the distribution of cardiac electrical potentials ascertained by the determination of "favourable" and of "unfavourable" leading-off points of the body, this, although it properly belongs to Part I., will be more conveniently considered in conjunction with the study of cardiac potentials on Man.

Part II.

Electrical Variations of the Heart on Man.

I now pass to the more important series of observations, to which those described in Part I. were the experimental preface.

It should first be recalled that, of the various points established in this preface, four in particular have a special bearing upon the due interpretation of the observations about to be described.

1. *The normal variation of the Mammalian ventricles is diphasic.*
2. *The variation can be observed on the intact animal by leading off from points of the body remote from the heart.*
3. *Under these circumstances the auricular contraction gives no electrical indication.*
4. *A lead-off from the mouth is equivalent to a lead-off from the base of the ventricles; a lead-off from the rectum or from a posterior extremity is equivalent to a lead-off from the apex.*

An investigation made last year upon my own person gave the following results* :—

Leading off from the surface of the body by the several limbs and from the mouth, I found that some combinations were favourable, while others were unfavourable,† to the demonstration of the cardiac variation. The favourable combinations were the following :—

Front of chest and back of chest.
Left hand and right hand.
Right hand and right foot.
Right hand and left foot.
Mouth and left hand.
Mouth and right foot.
Mouth and left foot.

---

* 'Journal of Physiology,' vol. 8, p. 229.

† I use the terms "favourable" and "unfavourable" for the following reason :—With a moderately sensitive electrometer no variation is seen with an unfavourable combination, and a small variation is seen with a favourable combination; with a very sensitive electrometer a small variation is seen with an unfavourable, and a comparatively large variation with a favourable, combination

The unfavourable combinations were :—

Left hand and left foot.
Left hand and right foot.
Right foot and left foot.
Mouth and right hand.

At that time I could not see the reason of this difference, and was surprised to find it so. There was, for instance, no apparent reason why a lead-off from mouth and right hand should be ineffectual, while a lead-off from mouth and left hand should be attended with a marked variation. And it was the most common and easily verified case. One electrode kept in the mouth while the other dips into a basin of salt solution, into which first the left hand then the right hand is plunged, yields a ready demonstration of a favourable in contrast with an unfavourable lead-off. Another illustrative contrast is furnished by leading off from hands and feet. If the right hand and either of the two feet be led off, a marked electrical variation is manifested at each pulsation of the heart; if now the left be substituted for the right hand, no variation is apparent, or at most a slight one.

Deferring the further enumeration of cases, I may at once offer the explanation of these apparently anomalous results.

The contraction of the ventricles is not simultaneous throughout the mass, but traverses it as a wave (at the present stage the direction of the wave of contraction is immaterial). Inequalities of potential, at different parts of the mass, are consequently established at the beginning and at the end of each systole. Or, to reverse the order of statement, the inequalities in question are proof of the passage of a wave of excitation. The distribution of these inequalities of potential is represented diagrammatically in fig. 5.

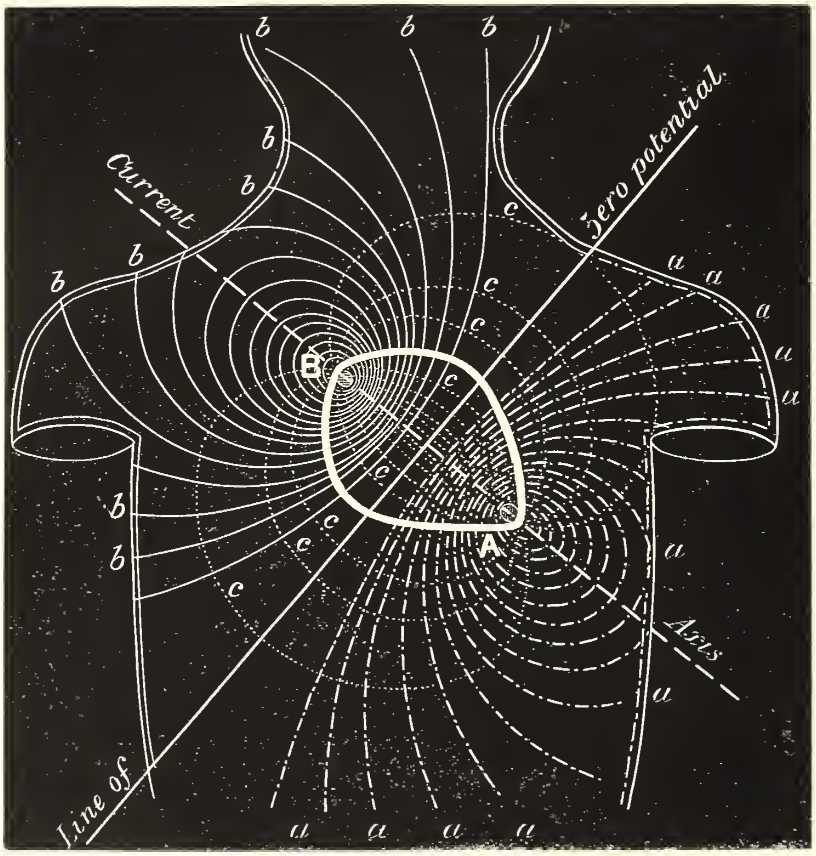
These data being transferred to the entire body, as in fig. 6, we have the dark portion a, a, a . . . as the area in which the potential of A is distributed, and the light portion b, b, b . . . as the area in which the potential of B is distributed.

Electrical variations will be manifested when any two points a and b are led off; no electrical variations will occur when any two points a and a, or b and b, on the same equipotential line, are led off; small electrical variations will be obtained when two points a and a, or b and b, on different equipotential lines, are led off.

This is precisely what has been demonstrated in the experiments given above.

Points a, a, a . . . are represented by the left arm, the left leg, the right leg, the front of the body, and by the rectum, &c. Points b, b, b . . . are represented by the mouth, the eye, the right arm, and the back of the chest. And, if the reader will refer to the results given above, he will notice that variations have been observed when two dissimilar points (a and b) have been connected with the electrometer, while variations have been absent or faint when two similar points (a and a, or b and b) have been explored. The difference of result, when the mouth and the right hand or the mouth and the left hand are joined to the electrometer, is now of obvious significance. Mouth and right hand are similar points *b*, *b*, mouth and left hand are dissimilar points *b*, *a*. The same remark applies to the contrasting results of exploration of one or other hand with either leg. The right hand and either leg are points *b*, *a*; the left hand and either leg are points *a*, *a*.

Fig. 5.



A and B are two points of apex and base respectively.

A straight line between A and B represents the axis of current between A and B if any inequality of potential should arise between the two points.

The dotted lines *c*, *c*, *c* . . . represent lines of current diffusion.

A straight line at right angles to the current axis represents the line of zero potential.

The broken lines *a*, *a*, *a* . . . represent equipotential lines surrounding the point A.

The continuous lines *b*, *b*, *b* . . . represent equipotential lines surrounding the point B.

To these results I may now add those obtained when one of the leading-off electrodes is in the rectum.

Favourable combinations are :—

Rectum and mouth.
Rectum and right hand.

Unfavourable combinations are :—

Rectum and left hand.
Rectum and right foot.
Rectum and left foot.

Thus we see that whereas in combination with a lead-off from the mouth the only unfavourable extremity is the right arm, this is the only favourable one in combination with a lead-off from the rectum; likewise, any one of the three other extremities constitutes a favourable combination with a lead-off from the mouth, and an unfavourable combination with a lead-off from the rectum. The reason is obvious: the mouth is a point *b*, the rectum is a point *a*; the mouth and rectum series of combinations with other points *a* or *b* are consequently the counterparts of each other.

The logical completeness of these experimental facts is further borne out by :—

1. Observations on Quadrupeds.
2. Observations upon two cases of *situs viscerum inversus*.

1. *Observations on Quadrupeds.*—The asymmetry in the distribution of potential in the Human body originating from the heart is not found in Quadrupeds so far as I have examined them. On Cats, for instance (*post mortem*, but of course only during the continuance of cardiac contractions), the following combinations were found to be favourable :—

Mouth and either posterior extremity;
Eye and either posterior extremity;
Rectum and either anterior extremity;
Rectum and eye or mouth;
Either anterior extremity and either posterior extremity;

whereas the following were found to be unfavourable combinations :—

Mouth or eye and either anterior extremity;
Rectum and either posterior extremity;
The two anterior extremities;
The two posterior extremities.

These results show that the distribution of potential from the heart occupying an approximately median position behind the sternum is in accordance with fig. 7.

2. Having ascertained the mode of distribution of potential on the normal Human body with the heart tilted to the left, I at once sought for a case of *situs viscerum inversus* with the heart tilted to the right. By the kindness of Dr. Cheadle and of Dr. Lewis I obtained the opportunity of examining two such cases (one male and one female), and found the differences from the normal exactly as expected, viz., the favourable combinations :—

Mouth and right hand;
Left hand and left foot;
Left hand and right foot;

and the unfavourable combinations :—

Mouth and left hand;
Right hand and right foot;
Right hand and left foot.

The above results are diametrically opposed to those obtained on the normal subject. Those obtained with the combinations indicated below were identical* in the two cases, favourable combinations in both being :—

Mouth and right foot;
Mouth and left foot;
Left hand and right hand;*

while in both we find the unfavourable combination :—

Left foot and right foot.

Detailed comment is needless; the complete harmony of the facts stated will be clearly recognised in the tabular summary on p. 191 with the assistance of the appropriate diagrams (figs. 6, 7, 8).

In each instance of the entire series a favourable combination is formed when the electrometer is connected with two heteronymous points a and b, while an unfavourable combination is formed when the connection is made with two homonymous points a, a or b, b.

The above observations supply abundant proof that in the contraction of the heart there is a time during which the apex and the base are not iso-electric.

From our knowledge of the diphasic variation of the hearts of Animals, we are further assured that on Man the inequality shall be of a double character; the part which first becomes negative is also the part which first ceases to be negative, the part which last becomes negative is also the part which last ceases to be negative, so that a diphasic change will occur in consequence of—

1. Unbalanced negativity where action commences.
2. Unbalanced negativity where action lasts longest.

What are the character and direction of the electrical changes observed on Man with the beat of the heart?

In answer to this question, I shall select the most favourable cases for examination, *i.e.*, those in which the variation has presented itself in a well-marked and readable form. I have followed two channels of information, each presenting special advantages and disadvantages. Simple inspection of the capillary under a $\frac{1}{4}$ objective is the ready and convenient means of investigating a series of combinations. Photography of the oscillating column of mercury with simultaneous photography of cardiac move-

* The *direction* of the variation with the two hands is opposite in the two cases, as will presently be noticed.

ments is the difficult but indispensable means for the close investigation and determination of special features in the rapid oscillations of the mercurial column.

By simple inspection I ascertained that *each beat of the heart is accompanied by a double movement of the mercury*, but I was unable to completely determine the character of this double movement. The whole movement consists of a comparatively large, prolonged portion, preceded by a small and extremely brief portion. There is no difficulty in determining the direction of movement as regards the second or major phase: the difficulty affects only the first or minor phase, which is so small and rapid as to appear with some instruments as a preliminary tremor, with others less sensitive I have failed to see it; but even with the most sensitive instruments which I have used I have failed to assure myself of its direction. I may, however, state at once that, as regards the second or major phase, I have always found its direction such as to indicate that any point *b* became negative to any point *a*. Simultaneous photographs of the double movement and of the heart's impulse show that the electrical precedes the mechanical event at whatever distance from the heart we choose to explore any two points *a* and *b*; they show, further, that in direction the first minor phase is opposed to the second major phase, being such as to indicate that any point *a* becomes negative to any point *b*.

By what has preceded, it has been defined that points designated *a, a, a* are in the region of apex potential and that points designated *b, b, b* are in the region of base potential.

The diphasic variation is, therefore, composed of a first phase indicative of *negativity of apex*, followed by a second phase indicative of *negativity of base*; this signifies that the excitatory process commences at the apex and lasts longest at the base, or, expressed in terms of mechanical action, that the contraction by which the ventricle discharges its contents commences at the apex and closes at the base.

Photo. 4.

c c

e e

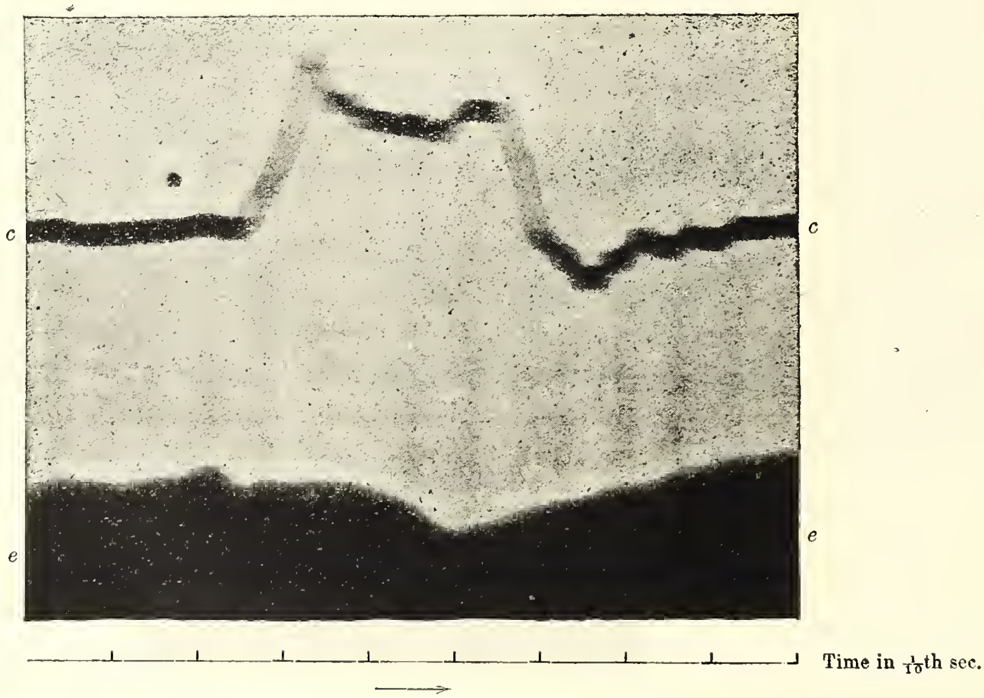
Heart of Man. Led off to Hg from mouth, to H_2SO_4 from left foot, the variation is *n*S. *c, c* = cardiogram; *e, e* = electrometer line.

NOTE.—The rate of propagation of the excitatory state in the Human heart may be deduced from the time of culmination of the 1st phase, but it is obvious that an estimate thus derived can under the circumstances be no more than an approximation. The interval between the initial point and the maximum of the first phase is about $\frac{1}{50}$ second; taking the length of the ventricles at 10 cm., this gives for the rate at which the excitatory state travels a value of 5 metres per second, on the supposition that it passes in a straight line from end to end of the organ. I do not, however, attach much importance to this estimate, having regard to the nature of the data and suppositions involved. The initial point of the second phase, which is presumably the indication of declining negativity of the apex, occurs about $\frac{2}{10}$ second after the initial point of the first phase. The interval between the initial point and the maximum of the second phase is about $\frac{1}{10}$ second.

I regard these time relations as an indication that the contraction, while beginning at the apex, lasts longest at the base.

It has been suggested to me, as possible, that the contraction of the entire heart, commencing at the venous orifices of the auricles, is propagated thence by the auriculo-ventricular curtains and musculi papillares to the apical vortex, and thence upwards to the base of the ventricles. This is a speculation for or against which I can at present see no positive evidence.

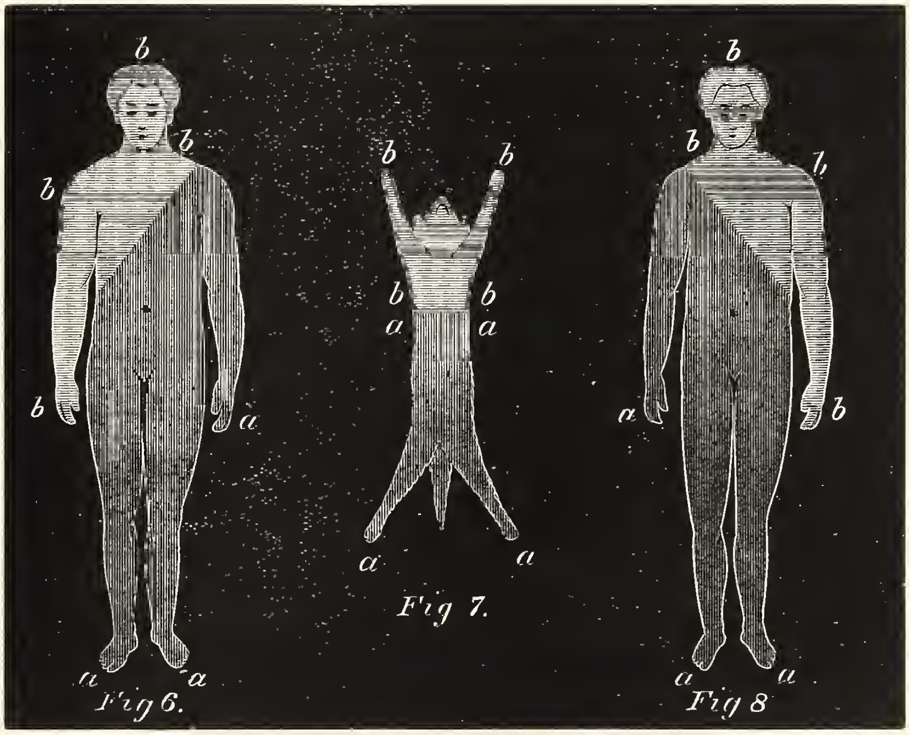
Photo. 5.



Enlargement (× 6) of a single systole of photo. 4, to show :—
I. First phase n (apex negative). II. Second phase S (base negative).
c, c = cardiogram ; e, e = electrometer line.

			Normal. (Cons. fig. 6.)	Reversed. (Cons. fig. 8.)
1	Mouth to H_2SO_4	Left hand to Hg	(*s*) N	—
2	,, ,,	Right hand to Hg	—	(*s*) N
3	,, ,,	Left foot ,,	(*s*) N	(*s*) N
4	,, ,,	Right foot ,,	(*s*) N	(*s*) N
5	Left hand ,,	Right hand ,,	(*n*) S	(*s*) N
6	,, ,,	Left foot ,,	—	(*s*) N
7	,, ,,	Right foot ,,	—	(*s*) N
8	Right hand ,,	Left foot ,,	(*s*) N	—
9	,, ,,	Right foot ,,	(*s*) N	—
10	Left foot ,,	,, ,,	0	0

Figs. 6, 7, 8.



The capital letter N, or S, gives the direction of the second phase; that of the first phase, which by inspection could not be determined in each case, is represented by the small letters *n*, *s*, in parentheses.

The direction of the second phase is in every case such as to indicate B negative to A.

On reversal of the connections, as given above, the electrometer movement was in every case opposite to that indicated in the Table.

In one instance only (No. 5) the directions in the normal and reversed subjects are opposite; a reference to the figures 6 and 8 shows that it is the only instance in which two points, *a* and *b*, are simply transposed.

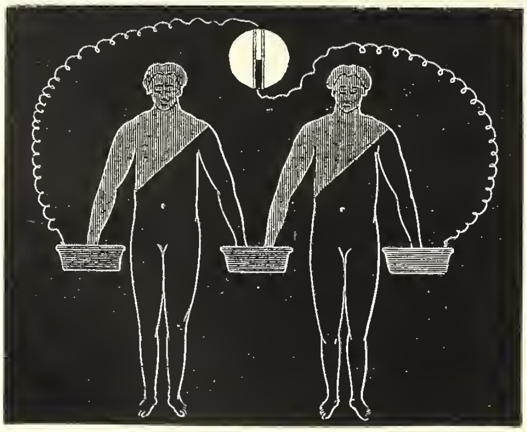
Postscript.

(Added February 7, 1889.)

The observations recorded in Part I., § 3, can be carried out on Animals at liberty. I have done so on Dogs and on a Horse, with the result that any two anterior or posterior extremities constitute an "unfavourable lead-off," and that any one anterior in conjunction with any one posterior extremity forms a "favourable" combination. For purposes of demonstration I give the observations the following form :—A large Dog, trained to stand still with his feet in vessels of salt solution, is made to do so with a favourable and an unfavourable pair of extremities in connection with the electrometer, a commutator being interposed so that either pair can be switched on to the electrometer without delay or disturbance. The mercury pulsates distinctly or not at all according as connection is made with a favourable or with an unfavourable combination.

To the observations recorded in Part II. I have added the following :—If two persons are connected with the electrometer as shown in fig. 9, their contracting hearts form battery when they are synchronous, and the normal variations are seen reinforced in degree; when, on the other hand, the two hearts are alternating in action there is interference of their electrical variations ; during this interference the movement of the mercury may be quite illegible, or the rhythm of each heart may be separately legible by following their separate pulses, the event depending upon the rates at which the two hearts are beating.

Fig. 9.



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V. *On the Organisation of the Fossil Plants of the Coal-Measures.*—Part XVI.

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[Plates 5–8.]

During the last twenty years many single examples of vegetable forms from Carboniferous rocks have come into my possession, which were obviously different from any hitherto described. But I have carefully abstained from publishing such specimens until examples of each multiplied in my cabinet, enabling me to determine how far their apparently distinctive features were constant, and not merely individual, variations. Many such imperfectly known forms still occupy a drawer in my cabinet; but in the present Memoir I propose to describe several of which examples have accumulated so far as to enable me to speak with reasonable certainty as to their specific distinctiveness.

In several of my previous memoirs I have from time to time called attention to a curious development of a medulla in the centre of the axial vascular bundle, especially of the Lepidodendra. This was especially done in the Memoir, Part III., when describing the Burntisland *Lepidodendron*, to which, as was also the case with the Arran form (Part X.), I have not yet ventured to give a specific name.

In the case of the Burntisland plant I showed, in figs. 3, 4, 5, 8, and 11, a medulla, *a*, which, at first of very minute dimensions, gradually enlarged, *pari passu* with the increase not only in the diameter of, but also in the number of the vessels composing the non-exogenous vascular cylinder—the "étui médullaire" of Brongniart. In this example traces of primordial medullary cells, however minute and few in number, could be detected in the youngest twigs.

The Arran plant (Memoir, Part X.) presented different features. The very young leafy twigs, found in great numbers in the Laggan Bay deposit, had an axial vascular bundle, which consisted wholly of tracheids, in the interior of which no traces of cellular parenchyma could be found (*loc. cit.*, figs. 1 and 2), whilst at a more advanced stage of growth such a medulla began to make its appearance (*loc. cit.*, fig. 3), which ultimately attained to a considerable size (*loc. cit.*, figs. 6, *d*, and 6A, *d*). Though unsuccessful in my search for an example in which the earliest traces of such medullary cells could be discovered, I had little hesitation in concluding that the small twigs devoid of medulla and the larger ones, in which such a medulla was very conspicuous, belonged to the same plant. This question of the development of a medulla in a manner so different from what is seen amongst living Exogens has long required to have more light thrown upon it; and I propose, in the present Memoir, to record some additional observations that I have made on the subject. But before doing so I would call attention to the existence of two distinct modes of ramification amongst these Carboniferous Lycopodiaceous plants. In one group, illustrated in Memoir Part III., Plate 43, figs. 19 and 20, and also in the *stems* of the Arran *Lepidodendron*, the vascular cylinder (étui médullaire) presents a dichotomous ramification, in which the cylinder divides into two virtually equal horseshoe-like halves. But in other instances only a small vascular segment separates from the cylinder. In my Memoir, Part II., p. 224, I showed that in *Halonia* segments only of the medullary vascular ring were detached to supply vascular bundles to the tubercles so characteristic of the genus, and which I showed (*loc. cit.*, pp. 224–5) were merely branches that had undergone an arrested development at an early stage of their growth. In the Memoir, Part XII., Plate 32, figs. 22, 23, 24, and 25, I showed that a similar mode of ramification existed in a Halonial (*i.e.*, fruiting) branch of the Arran *Lepidodendron*.

In the classic species of *Lepidodendron*, *L. Harcourtii*, I have not as yet succeeded in discovering any example of the first of the above modes of dichotomisation. But my young friend, Mr. LOMAX, of Radcliffe, brought me a fine branch of this plant in which the second type was conspicuous. This specimen gave us eleven transverse sections, which successively showed the progress of a bundle from its first separation from the medullary vascular cylinder to its existence as the vascular axis of a branch—Plate 5, fig. 1, represents a portion of the medullary vascular cylinder, as seen in the lowermost of the eleven sections. At a we have part of the large parenchymatous medulla. At b, b we see parts of the continuous ring of the medullary vascular cylinder. At b' we discover a segment of that cylinder becoming detached from the remainder at the points b'', b''. In fig. 2 we have the portions a and b, b of fig. 1, but the segment b' is now completely separated, as represented in fig. 3. The vascular portion is but little altered at b', b', whilst a small portion of the medullary parenchyma, a, of fig. 1, coheres to the detached vessels at fig. 3, a. In fig. 5 we find the segment changing its form. Its two points b', b' are curving inwards towards one another, but traces of the medullary cells are still seen at a. In fig. 4 the convergence of the two points b', b' has advanced so far that they now virtually touch one another, whilst scarcely any traces remain of the medullary cells at a. They seem to have undergone absorption. In fig. 6, which now represents the transverse section of the vascular bundle as seen in a separating branch, the coalescence of the points b', b' of the previous figures is complete. The bundle has now attained the form of a symmetrical cylinder, wholly composed, apparently, of scalariform vessels. Having thus become the vascular bundle of an ordinary branch of a stem of which a large cellular medulla is a conspicuous feature, such a medulla would, there can be little doubt, be developed in its interior, though no traces of cells can yet be detected in its central portion, where such an axial parenchyma should ultimately make its appearance.

Plate 6 and Plate 5, fig. 15, represent various stages of growth of a small species of *Lepidodendron*, from Halifax, of which I have now several examples, and to which I propose to give the name of *Lepidodendron mundum*. Its characteristic feature resides in its medullary vascular cylinder, *b*, which mainly consists, in young stems, of one, rather irregular, line of comparatively large and conspicuous scalariform vessels or tracheids, but which sometimes develops a second such series on the larger stems or branches. In both these cases this circle of conspicuous vessels is surrounded closely by a fringe composed of numerous, very much smaller, ones, from which the foliar bundles are solely derived.

Fig. 7 represents a transverse section of the youngest example of *L. mundum* I have yet obtained. In it the outer cortex, *c*, consists of a parenchyma, the diameters of the cells of which become smaller from within outwards, assuming, as they do so, the form of a coarse prosenchyma with somewhat thickened walls. The middle bark has disappeared from this, as also from all my other examples of this plant. At the centre, *b*, we have the vascular bundle, which is represented, still further enlarged, in fig. 8. It appears to consist wholly of a central mass of large tracheids, *b*, surrounded by a fringe of smaller ones, *b′*. I can detect no traces of cells amongst these tracheids.

In fig. 9 this solid axial cylinder has developed into a medullary vascular ring, *b*, *b′*, enclosing a few parenchymatous cells, *a*, forming a small but distinct medulla. In fig. 10, whilst the general conditions are similar to those of fig. 9, not only the tracheids of the vascular cylinder, *b*, *b′*, have become more numerous, but the medullary cells, *a*, have done the same. In this instance the cells, *a*, are crowded, thin-walled, and of irregular form, as if they had recently passed through a meristemic stage of multiplication. In fig. 11 this general enlargement and multiplication has not only gone still further, but the individual medullary cells have now assumed the hexagonal form of a very regular parenchyma. Fig. 12 represents a specimen in which these progressive developments of the medulla, *a*, and of the medullary vascular cylinder, *b*, *b′*, have attained to the maximum extension seen in any of the specimens in my cabinet. We also see in this specimen the well-preserved innermost cortex, *c*, which consists of a parenchyma the cells of which are uniformly of small size and shape, their mean diameters being about one-thousandth of an inch.

Fig. 13 represents a vertical section through the vasculo-medullary axis of a specimen of the same plant, the entire diameter of which, including its cortex, is about four-tenths of an inch. Here we have the medulla at *a* composed of cells with square or very slightly inclined transverse septa. The barred vessels* of the medullary

* The transverse bars of these vessels are connected vertically, as shown on Plate 5, figs. 14A and 14B, by delicate threads like those seen in the Arran plant (see Memoir X., fig. 4*).

vascular zone appear at *b*, whilst the inner cortex, *c*, of fig. 12 is now seen to consist of vertically elongated cells, amongst which those with rectangular and oblique septa are intermingled, whilst the thin-walled innermost ones, *c*′, present an obvious procambial aspect.

In fig. 14 we find the medullary vascular cylinder dividing in the regularly dichotomous manner seen among the larger forms of Lepidodendra. In fig. 15, on Plate 5, we have a section of my only example of this species in which the medullary vascular cylinder, *a*, *b*, is invested by a secondary, exogenously-developed zone, *d*.

The plant just described again illustrates the gradual development of a medulla within the interior of a vascular bundle where, in the youngest state of the bundle, no traces of cellular structure could be discovered, but the germs of which must necessarily have been furnished by the procambium from which, in the youngest twigs, the entire bundle originated. The probable philosophy of these facts may be considered after describing some additional undescribed types.

Figs. 16, 17, and 18 represent sections of a *Lepidodendron* from Halifax, for which I propose the name of *L. intermedium*, and which is remarkably distinct from any other form with which I am acquainted. Its vasculo-medullary axis, *a*, *b*, differs from all other known Lepidodendra, with one exception. The true medulla, *a*, consists of well-defined parenchymatous cells. The medullary vascular cylinder, *b*, is composed of numerous large barred tracheids. The external boundary of this cylinder is sharply and regularly defined, but its inner border is very irregular, some of its largest vessels being detached from it and isolated amongst even the most central cells of the medulla.* In addition to this characteristic feature, we have an exogenous zone, *d*, which is equally characteristic. The entire thickness of this zone, from its medullary to its cortical boundary, is only about the one-hundredth of an inch; yet many of its radial lines of tracheids consist of twenty-four distinct vessels. Measured separately, we find these tracheids ranging between the thirteen-hundredth and the twenty-six-hundredth of an inch in diameter. On examining these vessels under high powers, as they appear in tangential sections (fig. 17), we find them, *d*′, ascending and descending in extremely tortuous courses. They are here largely intermingled with cells, *d*″, *d*″, the diameters of which are equal to those of the vessels. These cells unmistakably correspond to those which constitute the medullary rays of the larger forms of Lepidodendra. (See 'Phil. Trans.,' 1872; Memoir, Part III., Tab. 42, fig. 13, *f*.) Fig. 18 is a vertical section made through the centre of the stem. At *a* we have the large vessels of the medullary vascular cylinder, intermingled irregularly at certain points with the medullary cells, as shown in fig. 16. At *d* we have the exogenous layer on one side of the central medulla and its vascular cylinder. In this zone we find the vessels of minute sizes, and again copiously intermingled with cells, many of which are arranged in radial mural lines like medullary

* In this respect the plant resembles *Lepidodendron selaginoides*; but it differs in the entire absence from its medulla of the barred medullary cells so characteristic of the latter type.

rays. At *e* we have the innermost zone of the cortex, as at fig. 16, *e*. The external cortex consists of a coarser parenchyma.

The next example to be described has a marked individuality. I propose giving to it the name of *Lepidodendron Spenceri*, after my friend Mr. J. SPENCER, of Halifax, who has so long been one of the most active of my several auxiliary collectors, to whom I have been so much indebted.

Fig. 19 is a transverse section of a stem or branch of this plant. The central structure, *a*, exhibits in this section no indication of medullary cells, but appears to be a solid rod of scalariform tracheids. It is surrounded at *e* by a narrow zone of the inner cortical parenchyma, the greater part of which has disappeared. At *f* is the middle cortex, characterised by an undulating outline, the outward prominences of which are frequently prolonged into points, f'. Fig. 20 represents one of these points further enlarged. In it, at *e*, we again have the zone of the inner cortex ; at *f* is the middle cortex, and at f' we find the barred vessels of a foliar bundle passing outwards. The cells of which the middle cortex, *f*, is composed are not uniform in structure ; dark, dense masses of thick-walled cells (fig. 19, f'') alternate with lighter groups with thinner cell-walls, f'''. In the centre of most of the darker groups of several of my specimens the tissue has become decayed, as at fig. 19, f''. This variation in the structure of the middle cortex appears to be a very characteristic feature of the species. None of my sections retain any traces of the outermost cortex and its foliar organs.

In fig. 21 we have a radial section of the above cortical zones ; at *a* are two or three of the outermost vessels of the central bundle, giving off at a' a foliar bundle passing upwards and outwards through the middle cortex. This cortex chiefly consists of strongly-defined prosenchymatous cells. In fig. 22 we have a vertical section through the centre of the vascular axis, *a*, of fig. 19. We see some prosenchymatous but thin-walled cells belonging to the inner cortex at *e*, through which a foliar vascular bundle is again escaping at a'.

On turning to the section through the vascular axial tissue (fig. 22), we find some interesting features. Its exterior, *a*, *a*, consists of developed and strongly-defined barred tracheids ; but the centre of this structure, a'', consists of thin-walled, unbarred, and much-elongated fusiform cells, whilst we have some very thin-walled, barred tracheids, but in which the bars are so thin as to be almost invisible. It is obvious that we have here a procambial string which is undergoing centripetal development into a vascular bundle, the more external tracheids, *a*, of which have undergone perfect lignification. Those indicated in fig. 22 by a''', a''' are but partially lignified, whilst in the central part, a'', we have procambial elements which the process of lignification has not yet reached. This is the only specimen in which I have personally seen a Carboniferous example of a centripetally-developed vascular bundle. What appears to have been a somewhat similar one is referred to by M. RENAULT as occurring in his *Lepidodendron Rhodumnense*.*

* 'Cours de Botanique Fossile,' Deuxième année, p. 23.

The next species, which I propose to designate *Lepidodendron parvulum*, is the smallest of the Lepidodendroid family that I have hitherto met with. I have a considerable number of examples of it in my cabinet, but there is a remarkable similarity of shape, size, and structure throughout the entire series, the mean diameter of the transverse sections, including the leaves, being only about one-tenth of an inch.

Though so small, every example in my cabinet obviously possessed a cellular medulla, Plate 8, fig. 23, *a*. This was surrounded by a medullary vascular cylinder, *b*, from two to three tracheids in thickness, the tracheids being fairly uniform in size, though, as usual, the outermost are rather less than those occupying the inner margin of the ring, which they combine to form. The inner cortex has disappeared. The middle cortex (fig. 23, *c*) is always an unbroken circle of parenchymatous cells. Still more uniform in size and shape are the parenchymatous cells of the outermost cortex, *d*, as well as of the leaves. But between these two zones of the cortex, *c* and *d*, we have in most of my specimens the verticils of empty areas (figs. 23, *e*, and 24, *e*). These spaces are separated by the radial bands of cortical parenchyma (fig. 24, *e′*). The empty areolæ, *e*, were long unintelligible to me, but at length I obtained specimens in which, as in fig. 25, *e*, I found them to be normally occupied by a thin-walled parenchyma very distinct from that of which the remainder of the cortex, including the leaves, was composed. The latter is represented in fig. 26; the former in fig. 27. What the functions of these vertical bands, *e*, of specialised tissue may have been I can form no opinion. I have seen nothing like them in any other Lepidodendroid stem. The vascular bundle, *b*, of fig. 25, is subdividing in the usual Lepidodendroid manner when the branch is about to dichotomise. I have an ascending series of five sections, from the lowest, in which the vascular ring is unbroken, to the highest, in which each of its two halves has not only almost reconstructed its perfect cylinder, but is enclosed in a distinct cortex of its own, just about to separate into two distinct branches; each of these branches retains the features characteristic of the species. I have obtained the plant both from Oldham and from Moorside, in Lancashire.

A few remarks on the general relations of the objects described in the previous pages may not be out of place.

The *form* of ramification illustrated in figs. 1–6 is not wholly new. In my Memoir, Part II. ('Phil. Trans.,' 1872, p. 224), I demonstrated that the tubercles characteristic of the genus *Halonia* were merely branches the development of which had been arrested, and that, as in the case of the plant (figs. 1–6) of the present Memoir, the vascular bundle supplying such branches was given off from the medullary vascular cylinder, in a manner intermediate between a perfect dichotomy of that cylinder, representing ordinary ramifications, and the detachment of a few small vessels from its periphery, constituting the bundles supplying the leaves. In Memoir, Part XII. ('Phil. Trans.,' 1883, p. 459), I further showed that the Halonial tubercular branches of my Arran *Lepidodendron* were supplied, as in fig. 1, by large segments cut off from the solid vascular bundle which, in the young branches of that plant, represents the medullary vascular cylinder of its older stem. I have also in my cabinet a section of the Burntisland *Lepidodendron* (cabinet number 502) which is giving off a similar branch. Various other specimens, of a like character, that have come under my notice, suggest the conclusion that the ordinary ramification of these Lepidodendra was dichotomous, as in fig. 25 of the present Memoir; but that, where a branch was of a special kind, characterised by an arrested development, then the mode of branching illustrated by figs. 1–5 was the normal one. It has long been certain that the scars of *Ulodendron* represent such arrested branches, which supported Lepidostrobi; and it is in the highest degree probable that the tubercles of *Halonia* are similar organs. A memoir by Mr. KIDSTON* clearly demonstrated that *Ulodendron* is not a *genus* of plants, but merely a *condition* of various Lepidodendroid genera; and the remark is equally applicable to the name *Halonia*, which, as is shown in Plate 34 of my Memoir, Part XII. ('Phil. Trans.,' Part. II., 1883), represents the extremities of an ordinary Lepidodendroid branch. A fine specimen of *Halonia regularis* in my cabinet shows that the ordinary branching of Halonial forms is dichotomous, as in fig. 14. It is therefore obvious that branches like figs. 1–5 represent some special ramification, distinct from the normal dichotomous one; and the only possible explanation that I can discover is that such arrested or subordinate branches were destined to support strobili.

It cannot now be doubted that the strobili of *Lepidodendron* were affixed to their sustaining branches in two ways. In one fine *Lepidostrobus* in my collection the base of the strobilus terminates the extremity of a *long*, slender pendulous twig. But in another very fine example, in the Museum of the Owens College, the large *Lepidostrobus* is planted laterally in an almost sessile manner upon a strong branch. In this instance the strobilus is evidently sustained by a short, arrested lateral twig, corresponding to one of the Halonial tubercles. The sections, figs. 1, 5, further establish another morphological fact, viz., that the young branches of a *Lepidodendron* may have a vascular bundle devoid of all visible traces of a cellular medulla, and yet such a medulla may be developed in its interior at a later stage of growth.

In some of my earlier memoirs I advanced the hypothesis that age produced other morphological changes, beyond mere growths, in these Lepidodendroid plants. To this M. RENAULT replied, speaking of *L. Harcourtii*, "On peut donc conclure que la différence de diamètre des rameaux de *Lepidodendron* n'apporte pas de changements dans la disposition générale des tissus qui les composent."†

My sections of *L. Harcourtii* demonstrate that such changes do occur; since, as the section, fig. 1, shows at *a*, the older and larger branches possess a very large medulla, which exists in like manner in all the older Halonial branches, which certainly

* "On the Relationships of *Ulodendron*," 'Ann. Mag. Nat. Hist.,' vol. 16, 1885.

† 'Cours de Botanique Fossile,' Deuxième année, p. 28.

continued to grow after the strobili had dropped off.* The illustration given above also tends to confirm statements made in my Memoir, Part X. ('Phil. Trans.,' 1880, p. 493), when describing the Arran *Lepidodendron.* I there gave my reasons for believing that, though no medulla was present in the centre of the vascular axis of any of the young twigs and branches of that branch, one had somehow or other been developed at a later stage of growth in those vascular bundles. Other conditions of these Arran specimens being considered, it now becomes an almost absolute certainty that such had been the case. The history of figs. 19, 20, and 21 of the present Memoir bears upon the same question, but little more can be said in reference to this latter plant until we obtain specimens of it in a more advanced state of growth. Though it and the Arran plant possess several features in common, I cannot identify them with sufficient definiteness to assign the same name to both. This, however, is of no consequence for the present, since I have not yet given any name to the Arran plant. Rather more important is the fact that M. RENAULT speaks of a stem having a solid vascular axis, like that of *Lepidodendron Spenceri,* to which stem he has given the name of *L. Rhodumnense.* The following description shows that it has some features in common with my plant :—La cavité centrale, due soit à un déchirement du tissu, soit à ce que le procambium n'a pas achevé sa lignification, est cylindrique, dans les échantillons non déformés, toujours de dimension extrêmement réduite, et ne présentant que des traces douteuses de tissu cellulaire.† Details in the structure of the cortex of *L. Rhodumnense* indicate a specific difference between it and my plant.

The *Lepidodendron intermedium,* figs. 16, 17, and 18, has a special interest when viewed in connection with the plant which I some time ago named *Lepidodendron fuliginosum.*‡ In my Memoir, Part XI.,§ in Plate 49, fig. 11, I represented a segment of a transverse section of the innermost cortex of this plant, in which a very rudimentary exogenous vascular zone is seen at h, and in p. 290 of the Memoir I called attention to the liability of these vascular elements to be diverted irregularly, and in an undulating manner, from their straight vertical course. Fig. 17 of the present Memoir, shows that though in the transverse section the vascular laminæ are arranged in regular radiating lines, as is also the case in the similar section of *L. fuliginosum* referred to above, vertical tangential sections of the same exogenous zone of *L. inter-*

* My meaning is not made sufficiently clear in this paragraph. In its transition from being a segment of a circle, as in fig. 3, to becoming a perfect cylinder, as in fig. 6, the vessels composing this bundle must have undergone precisely such changes of relative positions as M. RENAULT deems impossible. But the changes have not ended here. Supposing the bundle to have supplied an ordinary lateral branch, it must have become a hollow cylinder like that from which it sprang. If, on the other hand, it merely supplied an aborted Halonial tubercle which would be prolonged to form the axis of a Lepidostroboid fruit, it must have expanded in the axis of that fruit into a hollow cylinder, because all these strobili possess such hollow vascular cylinders, which enclose a true medulla.—September 3, 1889.

† 'Cours de Botanique Fossile,' Deuxième année, p. 23.

‡ 'Proceedings of the Royal Society,' vol. 42, p. 7.

§ 'Phil. Trans.,' 1881.

medium exhibit the same laminæ arranged, as in *L. fuliginosum*, in a very irregular, undulating manner. In both these cases this irregularity is due to the excessive development, amongst the vascular laminæ, of a cellular parenchyma. This feature, common to the two plants, is suggestive in both of a rudimentary type of exogenous growth; one, however, in which the *L. intermedium* has attained to a more advanced stage than *L. fuliginosum* has done. In the types in which the exogenous zone has reached a yet higher condition the number of the disturbing cells has been very much reduced, such only remaining as could be utilised as muriform medullary rays. How far a yet lower Lepidodendroid state has existed, in which no form of exogenous growth was developed at any period of life, cannot yet be determined. Thus far, however, we have obtained no specimen of *L. Harcourtii* which possesses such a zone, though I have a stem of that plant which is $3\frac{1}{2}$ inches in diameter. Nevertheless, that still larger stems may yet be discovered, showing exogenous growths of xylem, is suggested by the Arran plant, of which I have sections fully 3 inches in diameter, in which no such growth has yet made its appearance; whilst other stems of very much larger dimensions have the exogenous cylinder fully an inch in thickness between medulla and cortex. It now becomes more than probable that at one stage or another of their development all the Carboniferous Lepidodendroid stems grew exogenously. In some cases, as in fig. 15 of the present Memoir, such a growth took place when the medullary vascular cylinder of a branch was not more than the one-fortieth of an inch in diameter, whilst in others, as in the Arran plant represented in Plate 14, fig. 5, of my Tenth Memoir, the exogenous growth, though present, had made very small progress when the medullary vascular cylinder was fully an inch and a half in diameter.

The various instances in which I have now been able to trace the development of a true medulla in the Lepidodendra throws, I think, some light upon the physiological character of that development, as well as upon its homologies amongst living plants. All botanists are aware, though many geologists may not be, that the medulla of an ordinary exogenous stem makes its appearance in a very different manner from that seen in the Lepidodendra. The tip of a growing twig consists of a mass of what is termed "primitive tissue," viz., of undifferentiated parenchymatous cells. Almost, though not quite, simultaneously, a ring of vascular bundles is formed, which separates the lower portion of that primitive tissue into an inner mass, the medulla, or pith, and an outer ring of cortex. The medulla thus formed, though of small size, is merely a downward prolongation of the mass of apical cells, the two being absolutely continuous. As we trace this medulla downwards into the lower part of a shoot of the first year we find that the medulla increases in size up to a certain point, the distance of which point from the growing tip varies in different species of plants. As we thus descend we find that the increase in size is due to an increase partly in the number of the cells and partly in the diameter of the individual cells. Still lower down the diameter of the medulla either remains constant or even diminishes, so that in many old stems but feeble traces of it can be discovered.*

This process is a very different one from what we see in figs. 7–12 of the present Memoir. In fig. 7 the growth of the twig in length and diameter has not only made considerable progress, but its tissues have developed into a definite central vascular

* In illustration of this point I select a few examples from a number of measurements which I have made from transverse sections of growing stems, the measurements being recorded in decimal parts of an inch.

The plant.	Transverse diameter at apex of medulla.		Mean diameter of the medullary cells.	Diameter at successive points lower down the stem measured from apex of medulla.			
	Of branch.	Of medulla.		Distance.	Stem.	Medulla.	Medullary cells.
Æschylus Hippocastaneum	.085	.04	.0007				
,, ,, .	. .	. .	. .	6 inches	.25	.175	.0014
,, ,, .	. .	. .	. .	18 ,,	.45	.175	.002
Tydæa	.1	.04	.0009				
,, ...	. .	. .	. .	$\frac{1}{4}$ inch	.11	.05	.0019
,, ...	. .	. .	. .	$\frac{1}{2}$,,	125	.175	.0019
,, ...	. .	. .	. .	21 inches	.25.	.2	.005
,, ...	. .	. .	. .	28 ,,	.22	.25	.006
Geranium	.06	.05	.0014				
,, ...	. .	. .	. .	1 inch	.225	.105	.0035
,, ...	. .	. .	. .	4 inches	.275	.225	.0057
,, ...	. .	. .	. .	Old stem	.4	.2	.0035
Elder	.050	.03	.0014				
,, ...	. .	. .	. .	4 inches	.17	.12	.0043
,, ...	. .	. .	. .	14 ,,	.25	.15	.0043
,, ...	. .	. .	. .	2 feet	.6	.35	.0057
,, ...	. .	. .	. .	3 ,,	.55	.25	.0057

These measurements show us that, approximately, whilst

In *Æschylus* the medulla enlarges transversely from .04 to .75,
,, the medullary cells enlarge from .0007 to .002.
In *Tydæa* the medulla enlarges transversely from .04 to .25,
,, the cells enlarge from .0009 to .006.
In *Geranium* the medulla enlarges transversely from .05 to .2,
,, the cells enlarge from .0014 to .0057.
In Elder the medulla enlarges transversely from .03 to .25,
,, the cells enlarge from .001 to .0057.

We are thus led to the approximate conclusion that—

In *Æschylus three-fourths* of the transverse enlargement of the medulla is due to the expansion of the primitive medullary cells.

In *Tydæa* all the transverse enlargement of the medulla is due to expansion of those primitive cells.

In *Geranium* all the transverse enlargement is due to the expansion of the primitive cells.

In Elder three-fourths of the transverse enlargement is due to the expansion of the primitive cells, and not to an increase in their number.

bundle, invested by at least two distinct zones, the outermost of which bore leaves before any traces of a medulla could be discovered. Thus we discover at the outset a difference between the history of the medulla of a *Lepidodendron* and that of an Elm or an Elder tree. Tracing yet further the development of the Lepidodendroid type we find that in it the medulla first appears as one or two individual cells formed in the centre of a bundle of tracheids or vessels; once existing, however produced, these cells multiply rapidly by the ordinary meristemic process of fission. So far as my specimens throw light upon the latter process, it exhibits some peculiarities. The meristemic internal subdivision of these cells was not going on continuously, but interruptedly; at certain periods the whole of the fully-developed cells of the medulla simultaneously underwent such a division. Fig. 26A represents a cluster of cells from the medulla of a branch of *Lepidodendron Harcourtii* undergoing this meristemic multiplication. Some of these *matured* cells are subdividing into four or five of the younger generation. At this stage the latter are all thin-walled, small in size, and irregular in form; but all these conditions gradually become changed. The walls become more strongly defined; the area of each cell enlarges from two to two-and-a-half times its original size; and its unsymmetrical form develops into that of the regular pentagon or hexagon seen in the primary, or *mother*, cells of fig. 26.

But the effect of these changes is not limited to that produced upon the medulla. They reach the vascular bundle within which the increase in the number of the medullary cells is taking place. The first result of the internal tension occasioned by these cellular expansions is to develop the solid mass of vessels into an annular ring, *b*, of increasing diameter. Fig. 8 becomes progressively converted into what is seen in figs. 9, 10, 11, and 12. But this vascular ring, *b*, not only increases in diameter, but the vessels composing it increase in number, and change their relative individual positions as they do so. At later periods this process of meristemic division and subsequent expansion of all the new cells appears to have been repeated from time to time, until the medulla and its surrounding medullary vascular ring attain to their ultimate magnitudes—a condition which was probably coincident with the first appearance of the more external exogenous zone. I was at one time inclined to think that some of the young medullary cells assumed a procambial form, and were converted into new vessels; and even now I am not sure that this is not so in some instances. But it appears to me now that, in such examples as figs. 7–14, the new vessels must be produced on the cortical side of the medullary vascular cylinder—*i.e.*, centrifugally rather than centripetally. However this may be, the enlargement of this cylinder is evidently effected mainly, if not wholly, through the internal tension occasioned by the subsequent multiplications and expansions of the medullary cells—a condition that has no existence amongst the exogenously-grown trees now living.

But a partially parallel state of things does exist amongst some living plants. In his learned work on the 'Comparative Anatomy of the Vegetative Organs of the Phanerogams and Ferns,' the late Dr. A. DE BARY observes: "In many Ferns the original axile bundle widens out, as the stem grows stronger, into a tube, which is for the most part closed all round, and has only at each node, below the insertion of the leaf, a relatively small slit, or *foliar gap*, through which the medullary parenchyma is connected with the cortex, and from the margin of which one or several bundles pass into the leaf."* I think there can be little question but that this widening out of an axile bundle which, as the same author observes, "extends itself, and forms a tube, which surrounds a parenchymatous cylinder of pith" (DE BARY, *loc. cit.*, p. 283), presents substantially an analogous condition to what is so general amongst the Carboniferous Lycopods. At the same time, though the two cases are identical in their general features, they present differences. I have not yet found any Fern in which a solid central bundle develops a medulla within its own component vessels. The medulla is existent *ab initio*, surrounded by a circle of such vessels. Thus, in *Aspidium filix mas* this medulla has a diameter of about a tenth of an inch close to the growing apex of the stem, and at its utmost development it rarely attains to more than four times that diameter; even then the interspaces between the bundles composing the entire vascular cylinder are very large. This is very different as to details from some examples found amongst the Lepidodendra, where a medulla developed from some invisible cell-germ has expanded to more than an inch in diameter.

Plate 8, fig. 28, represents a transverse section of a *Rachiopteris*, of which I have several sections. At the first glance its central vascular bundle, cut through transversely, has a Zygopteroid aspect, but it differs in the fact that, whilst its side *a* consists of a line of large vessels, its opposite side *b* is composed of very small ones: at the two points *c*, *c′* the bundle consists of a number of the small vessels resembling those at *b*, but here forming an irregular mass surrounding a vacant space at each end of the central portion, *a*, *b*; at *d* a cluster of these small vessels is detached in Zygopteroid fashion, as if going to supply some peripheral organ. The outer cortex is composed of an uniform cellular tissue. I propose the name of *Rachiopteris irregularis* for this very distinct organism, which is from Halifax.

* English Translation, p. 284.

Supplementary Observations.

Strong objections have been offered to the supposition contained in the foregoing paper that any translation of position in the elements constituting any *permanent* tissue was possible. Since the paper was written I have devoted much time to this matter, which has an important bearing upon the chief subject dealt with in the present Memoir. The further I carry my enquiries into the question of the origin and growth of the medulla, and the contemporaneous expansion of the investing vascular medullary cylinder (the "étui médullaire" of BRONGNIART), the more clear the evidence becomes that in these primæval vascular Cryptogams we are brought face to face with important histological and physiological phenomena to which no exact parallels are to be found amongst living plants. Yet these phenomena must be as capable of explanation as the many similar ones to which the present race of philosophic botanists have given so much attention; and, as the phenomena in question must have an important bearing upon the problem of evolution, they demand a similar amount of careful study.

Though, as already observed, living plants present no exact parallels to the conditions which I have discovered amongst the Carboniferous Cryptogams, we can scarcely suppose that those conditions are the results of vital agencies of which no traces have descended to living plants. Some further important conclusions arrived at by the late Professor DE BARY seem to me to have a practical bearing upon the subject. Speaking of the origin of intercellular spaces, he says, "These arise in two ways in the original masses of cells, which, at least when in the state of meristem, are always in uninterrupted connexion. First, by separation of *permanent** tissue elements, as the result of their unequal surface-growth in different directions, the original common walls splitting, while the common limiting layer, which was originally present is—perhaps always—dissolved. Secondly, by disorganisation, dissolving, or in many cases rupture of certain *transitory* cells, or groups of cells, which are surrounded by permanent tissues. We may call the first mode of origin *schizogenetic*, the second *lysigenetic*" ('Comparative Anatomy of the Vegetative Organs of the Phanerogams and Ferns,' p. 200).

In the above passages DE BARY had chiefly in view the formation of cavities destined to be occupied by gums, resins, and various other *unorganised* secretions; whereas, in the case of our fossil plants, though we have to deal with similarly enlarged cavities, these latter are destined to be occupied by an *organised* cellular parenchyma; but these differences in their ultimate purpose do not materially affect the way in which the newly-formed and expanding cavities are brought into existence. The essential points established by DE BARY are: first, that vertically elongated, more or less cylindrical canals can be developed even in *permanent* tissues where such hollow spaces had no previous existence; and, secondly, that such cavities may be

* The *italics* are Professor DE BARY's.

formed alike by the one or the other of the two processes, which he respectively names *schizogenetic* and *lysigenetic*. So far as I can see at present, we must choose between these two processes in seeking an explanation of the development of the medullary area and the coincident annular expansion of the vasculo-medullary cylinder, so characteristic of all the Carboniferous Lepidodendroid plants.

In the preceding Memoir I have once more shown (figs. 1–6) that a stem or branch, the transverse section of which reveals a large and conspicuous parenchymatous medulla surrounded by a tracheidal, vasculo-medullary cylinder, frequently can, and possibly always does, give off to a much younger and smaller twig a *solid* axial bundle, in the interior of which no traces of a medulla can be seen. I have also again demonstrated (figs. 7–15) that a very young twig, destined in the future to enlarge into a branch, but in which the axial bundle of tracheids or vessels (fig. 8) is solid, not a hollow cylinder, and in which no traces of parenchyma can be detected, undergoes changes as it grows older and larger; the axial vascular bundle becomes more and more hollow, whilst, in its expanding interior, a cellular medulla, at first very small, and consisting of but a few cells (fig. 9), becomes gradually larger (figs. 10, 11, 12), and its component cells more numerous, as explained in the Memoir. These are indisputable *facts*, whatever may be the explanation of them. In seeking such an explanation, I repeat, we are shut up to the two processes described by DE BARY, to account for the expansion of the solid axial vascular bundle into a hollow cylinder. Either the youngest, first formed tracheids were *pushed asunder* by the centrifugal pressure of the growing and multiplying cells of the young medulla developing in their midst (a *schizogenetic* process), or they were *absorbed* (*lysigenetically*) under the influence of the same pressure. In the first case, all the relations of contact and propinquity between the primary vessels or tracheids composing the bundle must have been subjected to a continued succession of changes; because not only had the *primary* vessels, &c., to enclose a larger area than previously, but they had to allow the intercalation of a succession of newer additional vessels supplied from the investing meristemic cortex. This latter necessity is demonstrated by the fact that, as the vascular ring increased in diameter, enclosing at the same time a growing medulla, the actual number of its component vessels likewise increased. My present conviction is that the schizogenetic hypothesis is most in harmony with the known facts; but, should further investigations fail to support this conclusion, which I scarcely conceive to be possible, we must then fall back upon the second hypothesis. In doing this we must conclude that *all* the vessels or tracheids seen in fig. 8 were doomed to undergo absorption, and thus make room for the young medulla of fig. 9, and that, in like manner, the successive vascular rings of figs. 10–12 had but a temporary existence; the only permanent vessels being those of fig. 15, *b*, after the formation of which the development of the exogenous zone, *d*, always appears to have arrested the further expansion alike of the medulla and of its investing vascular cylinder in these Lepidodendroid plants.* The objection, in the present case, to this

* A note on p. 466 of my Memoir XII. on this subject is, I fear, an erroneous one.

lysigenetic hypothesis is strong and definite. We never find that the tracheids forming the inner border of the ring *b*, in specimens like figs. 10–13, have their walls torn or disorganised, which must have been the case had these inner tracheids been subject to a continuous destructive action.

Professor Hugo de Vries, of Amsterdam, who at my request has given some little attention to this subject, has suggested to me the question—Are such sections as I have represented in figs. 7–15 really examples of branches of the same plant in various stages of development? About this I have no doubt whatever. The twig in these arborescent exogenous Lycopods, as in modern exogenous trees, is but the young state or precursor of the future branch. It is a material point, bearing upon this part of the subject, that the above arguments are not based upon some isolated example of these Lepidodendroid plants. My specimens show that the conditions to which I am once more calling attention are not isolated or rare. *They are characteristic of the entire Lepidodendroid family, whether arborescent or otherwise.* Throughout the entire group we know that the large dichotomous branches did not shoot into existence as such. They were all once slender twigs, and I am convinced that such series of sections as abound in my cabinet, corresponding to those now represented in my figures 7–15, are illustrations of twigs and branches of the same plant in successive stages of their growth.

I should also like to remark on the objection that in this and some of the preceding Memoirs I am in danger of establishing new *species* without sufficiently defining them. In fact, the establishment of *species* of Coal-plants in the strict sense of the word has not been my object. The difficulties in the way of doing this are, in my judgment, insuperable. In the earlier of this series of Memoirs I made no attempt to attach specific names to the objects which I described. I mainly sought to throw new light upon the morphology and histology of the Carboniferous plants. I soon found that, whilst one type of structure was common to the entire group of Lepidodendroid and Sigillarian plants, this type was subject to numerous remarkable modifications as regarded the details alike of structure and of growth. It became necessary, by some symbol, to facilitate reference to each of these modifications. There was no room for doubting that where such details were conspicuously *different* I was dealing with forms that were specifically distinct. But the converse was not necessarily true. It was quite possible, though incapable of demonstration, that identical modifications of vegetative structures might exist in plants in which the reproductive organs might have shown specific distinctions, as is so commonly the case amongst living Lycopodia and Selaginellæ. Brongniart, however, had already followed the example set by Witham of employing the Linnean binomial nomenclature under similar circumstances, in his descriptions of *Lepidodendron Harcourtii*, of *Sigillaria elegans*, and of *Sigillaria spinulosa*; and after some consideration I deemed it best to follow so distinguished a precedent. At the same time, I wish it to be distinctly understood that my specific names are intended to represent modifications of types of organisation rather than specific forms.—The Botanical Laboratories, Owens College, *July* 31, 1889.

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5	2	The portion *b*, *b*, *a*, of fig. 1, after the segment *b′* has become detached. × 30. Cabinet number, 1656	197, 201
5	3	The segment, fig. 1, *b′*, after its detachment from fig. 2; *a*, remains of the medullary cells. × 30. Cabinet number, 1656	197, 201
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VI. *Researches on the Structure, Organization, and Classification of the Fossil Reptilia.—*
VI. *On the Anomodont Reptilia and their Allies.*

By H. G. Seeley, *F.R.S., Professor of Geography in King's College, London.*

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[Plates 9–25.]

The Structure of the Skull in Anomodontia.

The chief contributions to a knowledge of the Anomodont skull have been made by Sir Richard Owen, Professor Huxley, and Professor Cope. When Sir R. Owen published his first description of several species of *Dicynodon*, in 1845,* and regarded that genus as indicating a new order of Saurians, an elaborate comparison was made to indicate the nature of its relation to existing orders of Reptiles, with the result that the skull was interpreted as essentially formed on the Lacertilian plan, though upon that plan structures are engrafted which are otherwise characteristic of Chelonians and Crocodiles. The Lizards with which it is chiefly compared are the fossil *Rhynchosaurus* of the Trias, and the existing *Hatteria*. The chief Lacertilian characters enumerated are :—(1) the single pre-maxillary bone and the double external nasal apertures, though the pre-maxillary is single in *Chelys*, and both these conditions are found in many Serpents and some Amphibians, though the great development of the pre-maxillary in Dicynodonts is thought to foreshadow its condition in Birds ; (2) few existing Lizards have the maxillary arch so strong or the maxillary bones so well developed ; (3) the zygomatic bone is continued from the lower border of the orbit to the upper end of the tympanic pedicle ; (4) the tympanic pedicle descends vertically from the junction of the zygomatic and mastoid, and is comparatively free ; (5) the flat anterior part of the parietal bone is perforated by a parietal foramen, and the posterior part of the bone bifurcates ; (6) the orbits are circular and midway in the length of the skull. In some respects the characters are said to show a blending of Chelonian and Lizard structures. Thus, the palate unites features of both those orders ; there is a bony floor to the orbit ; the ex-occipital and basi-occipital bones combine to form the tripartite occipital condyle.

Among the differences of *Dicynodon* from Lizards which were indicated, are:—(1) the

* 'Geol. Soc. Trans.,' vol. 7.

2.12.89

edentulous Turtle-like mandible and pre-maxillary; (2) the expanded vertical occipital plate, which is compared to that of Crocodiles; (3) the brain-case is only two-thirds the breadth of the inter-orbital space, and in its small size suggests the lowest Amphibians; while (4) the two tusk-like teeth are only paralleled among Mammals. One of the distinctive Dicynodont characteristics is the junction of the par-occipital and sphenoid with the tympanic, near to the broad slightly convex condyle.

The bones which are identified in the skull are :—the basi-occipital, ex-occipital, par-occipital, sur-occipital, basi-sphenoid, mastoid, parietal, post-frontal, mid-frontal, vomer, pre-frontal, nasal, lachrymal, inter-maxillary, maxillary; the malar was thought to be blended with the maxillary; the palatine, dentary, articular, splenial, angular, coronal, zygomatic, tympanic.

In 1859, Professor HUXLEY made an important contribution to knowledge of the Dicynodont skull in his memoir on the *Ptychognathus Murrayi.** By making transverse vertical sections of the skull, the remarkable median vertical longitudinal plate which extends forward from the brain-case was discovered. The pre-sphenoid is said to be united with the basi-sphenoid by an oblique suture. Anteriorly it becomes the inter-orbital septum, and passes into the ethmo-vomerine plate or nasal septum. This expands inferiorly and unites with the maxillary. The palatine bones are found to be attached to the pre-sphenoid below the anterior border of the orbits, and, passing forward into the maxillary, define, with the ethmo-vomerine septum, the two posterior nares. The nasal passages of Birds make a much closer approximation to the condition in *Dicynodon* than is found in the Monitors. And it is observed that the manner in which the palatines and pterygoids are connected with one another and with the pre-sphenoid is extremely Bird-like.

In 1859, Sir RICHARD OWEN made full descriptions† of species which indicated the existence of other genera, named *Ptychognathus* and *Oudenodon*, and elucidated, in a more perfect way, many characters of the skull which were indicated in the original figures and descriptions of *Dicynodon.* In the same memoir the genus *Galesaurus* was defined, which afterwards became the type of a distinct family, and eventually was placed in another order of Reptiles.

In the same year, Sir RICHARD OWEN contributed to the Reports of the British Association for the Advancement of Science, a memoir‡ "On the Orders of Recent and Fossil Reptilia and their Distribution in Time," in which the order Anomodontia is defined for the first time. It then comprised three families: Dicynodontia, founded on *Dicynodon* and *Ptychognathus;* Cryptodontia, founded on *Oudenodon;* and Gnathodontia, founded on *Rhynchosaurus.* The third family does not appear to have been sustained, for, in a later writing,§ *Rhynchosaurus* is grouped under the Cryptodontia.

* 'Geol. Soc. Quart. Journ.,' vol. 15, p. 649, on *Dicynodon Murrayi.*

† *Ibid.*, vol. 16, 1860, p. 49; and 'Ann. Nat. Hist.,' vol. 4, 1859, p. 77.

‡ 'Brit. Assoc. Report,' Aberdeen, 1859, p. 153.

§ 'Palæontology,' 2nd edition, 1861, p. 263.

The same author soon after published his 'Palæontology,' which gives in the second edition, in 1861, a summary of his researches on the Anomodontia,* with some new facts and new views. Thus, it is observed of the Dicynodontia: "The vertebræ, by the hollowness of the co-adapted articular surfaces, indicate these Reptiles to have been good swimmers, and probably to have habitually existed in water; but the construction of the bony passages of the nostrils proves that they must have come to the surface to breathe air. The pelvis consists of a sacrum composed of five confluent vertebræ, with very broad iliac bones, and thick and strong ischial and pubic bones. The bones of the limbs resemble those of the marine Chelonia, but are more expanded at the extremities." The par-occipital in *Ptychognathus* is said to have been connate with the ex-occipital, as in Crocodiles.† A similar observation had been made concerning the skull of *Dicynodon tigriceps*.‡ The bone in *D. lacerticeps* which was named par-occipital in the explanation of the plate was described as wedged between the basi-sphenoid and the quadrate. That bone I propose to interpret as the malleus. In 'Palæontology'§ a new family, named Cynodontia, is founded for the genera *Galesaurus* and *Cynochampsa*.

In 1862 Sir RICHARD OWEN contributed to the 'Philosophical Transactions' of the Royal Society, a memoir "On the Dicynodont Reptilia, with a description of some Fossil Remains brought by H.R.H. Prince Alfred from South Africa, November, 1860."‖ The same volume contains an account of the pelvis of *Dicynodon*,¶ in which the Mammalian character of the pubic symphysis is urged; and the sacrum would have been considered Mammalian, but for its resemblance to Dinosaurs.

Professor COPE in 1870 published in the 'Proceedings of the American Association for the Advancement of Science' a memoir "On the Homologies of some of the Cranial Bones of the Reptilia, and on the Systematic Arrangement of the Class," in which the cranium of the Anomodontia is described from new materials. The new skull is in many respects similar to *Ptychognathus*, but appears not to show the posterior bifurcation of the parietal bone, resembling in this respect types like *Dicynodon tigriceps*. It is referred to a new genus named *Lystrosaurus*. If this specimen justifies all the conclusions which the author draws from it, it should have been more fully figured, for it is a more perfect representative of the order than the materials previously described in this country. Like other writers, Professor COPE uses a nomenclature for the bones which depends upon his theoretical views of the structure of the skull and differs in some points from that already given by OWEN. The order is grouped with the Archosauria, a division of the Reptilia formed to

* *Loc. cit.*, p. 255.

† 'Geol. Soc. Quart. Journ.,' vol. 16, p. 50.

‡ 'Geol. Soc. Trans.,' vol. 7, p. 235.

§ *Loc. cit.*, p. 267.

‖ 'Phil. Trans.,' 1862, p. 455.

¶ *Loc. cit.*, p. 462.

include Anomodontia, Dinosauria, Crocodilia, and Ornithosauria. The immovable articulation of the squamosal throughout the length of the quadrate bone removes the Anomodontia from the Lacertilia. The withdrawal of the pro-otic and opisth-otic from supporting the quadrate bone places it nearest towards the Lacertilia. *Hatteria* has a similar development of the squamosal. In both types the posterior extremity of the pterygoid is much expanded, and supports a columella ; there is an osseous inter-orbital septum ; distinct (?) epi-otic bones, bi-concave vertebræ, and a parietal foramen. Lizards also agree with Anomodonts in wanting the quadrato-jugal arch, and in having the pre-maxillary bone usually single. The Chelonian characters are limited to the edentulous jaws, and co-ossified mandibular rami. The Crocodilian characters are :—the pre-sphenoid keel, the expansion of the pterygoid to unite with it, the mandibular foramen, and reduced size of the zygomatic bone. Resemblances to the Ichthyopterygia are seen in the parietal and quadrate branches of the squamosal, the sessile suspensorium of the quadrate, and the posterior flat opisth-otic. Resemblances to the Dinosauria are found in the elongate sacrum, the capitular and tubercular attachment for ribs on the neural arch and centrum respectively. This type of rib articulation is also spoken of as Mammalian. The ribs are continued to the sacrum. The author concludes that the Anomodontia are the most generalized order of Reptiles known.

Professor COPE's account of the pterygoid, epi-otic, pro-otic, columella, quadrate, pre-sphenoid, and other structures has hitherto only been supported by the evidence of diagrammatic woodcuts.

In 1876, the Trustees published a descriptive and illustrated 'Catalogue of the Fossil Reptilia of South Africa in the Collection of the British Museum,' by Sir RICHARD OWEN, F.R S., in which most of the figures published previously are reproduced, with representations of all the more important specimens in the collection.

The family Cynodontia of that author's 'Palæontology' is now raised to the rank of an order, and named Theriodontia, on account of the resemblance of its dentition to that of the Mammalia. In this group are arranged various species of the genera *Lycosaurus, Tigrisuchus, Cynodracon, Cynochampsa, Cynosuchus, Galesaurus, Nythosaurus, Scaloposaurus, Procolophon*, and *Gorgonops*. The last-named genus is the type of a family, Tectinarialia ; the other genera are classed as Binarialia or Mononarialia, according as the external nostrils are divided or single.

The Anomodontia are sub-divided into three families. The name Dicynodontia of the 'British Association Report,' is replaced by the term Bidentalia ; and in this family are placed species of the genera *Dicynodon* and *Ptychognathus*. The family Cryptodontia now includes the genera *Oudenodon*, *Theriognathus*, and *Kistecephalus*. A third family, named Endothiodontia, is formed for the genus *Endothiodon*, which has the teeth spread over the palate and absent from the alveolar borders.

In "*Dicynodon lacerticeps*" (*loc. cit.*, Plate XXIII., fig. 3, p. 30), the par-occipital (opisth-otic) was regarded as being confluent with the ex-occipital, as in the Crocodile. Its broad process is said to abut against both the mastoid (squamosal) and tympanic (quadrate). I have been unable to find any certain evidence of the presence of the opisthotic in this position. It is stated that the tympanic pedicle is formed by the mastoid, 8 (squamosal), squamosal, 27 (quadrato-jugal), and tympanic, 28 (quadrate); but neither in the text nor in the figures is the part taken by each bone defined. I recognize no evidence of the quadrato-jugal, and the bone on which the number 28 is placed, Plate XXIII., fig. 1, I regard as the squamosal, and this bone is also numbered 8 and 27. What I regard as the quadrate bone is very imperfectly exposed, and only appears as a slender ossification widening distally, placed in front of the distal end of the squamosal. It is neither described nor figured. Hence, the visible part of the so-called tympanic pedicle is formed by the squamosal bone, though, as will be subsequently proved in other species, the condylar surface is contributed to by the quadrate bone. In the "Description of *Dicynodon leoniceps*" (p. 32, Plates XXIV.–XXVI.), the author regards the occiput as having been crushed into a pair of plates meeting at a right angle. This basin-like occipital depression, also found in *D. pardiceps* and other species, seems to me to be natural; for, if pressure had materially approximated the squamosal bones in the way implied, it would have obliterated the groove between the parietal bones (Plate XXV.), and have otherwise distorted the skull. The form of the condyle of the quadrate bone is compared in this species to the distal end of the humerus of a Ruminant or the tibia of a Bird. Subsequently it may be shown that the form of the condyle varies with the species. The author states that in this species the squamosal descends to near the neck of the outer condyle, and that it extends behind the quadrate. The author states that the composition of the tympanic pedicle is clearly traceable, but the numbers 8 and 27, placed on its upper part, imply distinct elements, which I am unable to find.

The pair of "hypapophyses" below the occipital condyle is said to be formed by the basi-occipital and basi-sphenoid; they are compared to the descending basi-occipital process of Lizards, and are supposed to have given attachment to powerful muscles. In *D. lacerticeps* and many Dicynodonts each process is seen to be formed by the (?) ex-occipital, basi-occipital, and basi-sphenoid, and to give attachment to the malleus, which has not ascended to its position in the skull among Mammals, and extends transversely outward to the quadrate bone. The pterygoid, which rests partly on the basi-sphenoid, is said to send a process backward, which abuts against the quadrate; this character is regarded as Lacertilian. It will subsequently appear that the quadrate may also sometimes send a short pterygoid process inward to meet the pterygoid bone; and that the mode of junction of these bones shows distinctive features. The author then describes the long ovate palato-nasal vacuity, which is single, but apparently without recognizing the vomer at its anterior margin. Evidence will hereafter be given to show that in some other Dicynodonts there are three palatal vacuities—one posterior and median in the pterygoid bones, as Sir R. OWEN thinks possible, and two lateral vacuities divided by the vomer. There is reason to doubt whether the ecto-pterygoid (transverse) is found, and the lateral vacuities of the Crocodile's palate are not present. The author compares the inter-palatal vacuity to the condition in many Marsupials. The exposed temporal foss is said to give a Carnivorous Mammalian character to the skull.

On the right side of the type-specimen of *D. leoniceps* (OWEN), British Museum, No. 47,047, the base of the columella is exposed, rising from the posterior part of the pterygoid bone. It has a comparatively long basal attachment, and extends obliquely upward and forward. It is imperfectly indicated in 'Cat. South African Reptiles,' Plate XXIV., above the number 24, but is not described.

The specimen figured in this plate seems to me to give no support to the interpretation of the palate there given. The separation between what are interpreted as the pterygoid and palatine bones, as shown in the figure, has no existence. The bone is divided on one side but not on the other; and the division is probably due to fracture. If the anterior portion were really separate, it would be the transverse bone, and not the palatine; but no such division in the pterygoid is to be detected in any of the numerous specimens which display that bone. Hence, the pterygoids are commonly united below the sphenoid, in the median line (though apparently separate in No. 47,056), and they are constricted from side to side at their confluence. They send a process on each side backward and outward to the quadrate, and forward and slightly outward to the maxillary. The latter union takes place below the orbit, and excludes the palatine bone from the external border of the palatal arch. The palatine bone may be found on each side, in close squamous contact with the anterior bar of the pterygoid, along its inner side. It extends backward to the point where the inner diverging fork or plate of the pterygoid is given off (Brit. Mus., No. 47,047; and 'South African Catalogue,' Plate XXVI., fig. 1). Anteriorly the bone widens, and externally is wedged between the pterygoid and maxillary bones, and internally processes from the two sides converge forward to meet the vomer, which divides them, but is not drawn in Plate XXVI., fig. 1. If the median inter-palatine space had been excavated deeper, it is probable that the internal pterygoid processes between which the number 24 is placed on Plate XXVI. would have converged forward to form a median vertical pterygoid plate, which would have extended forward to meet the vomer, this being the usual relation of the bones in other specimens.

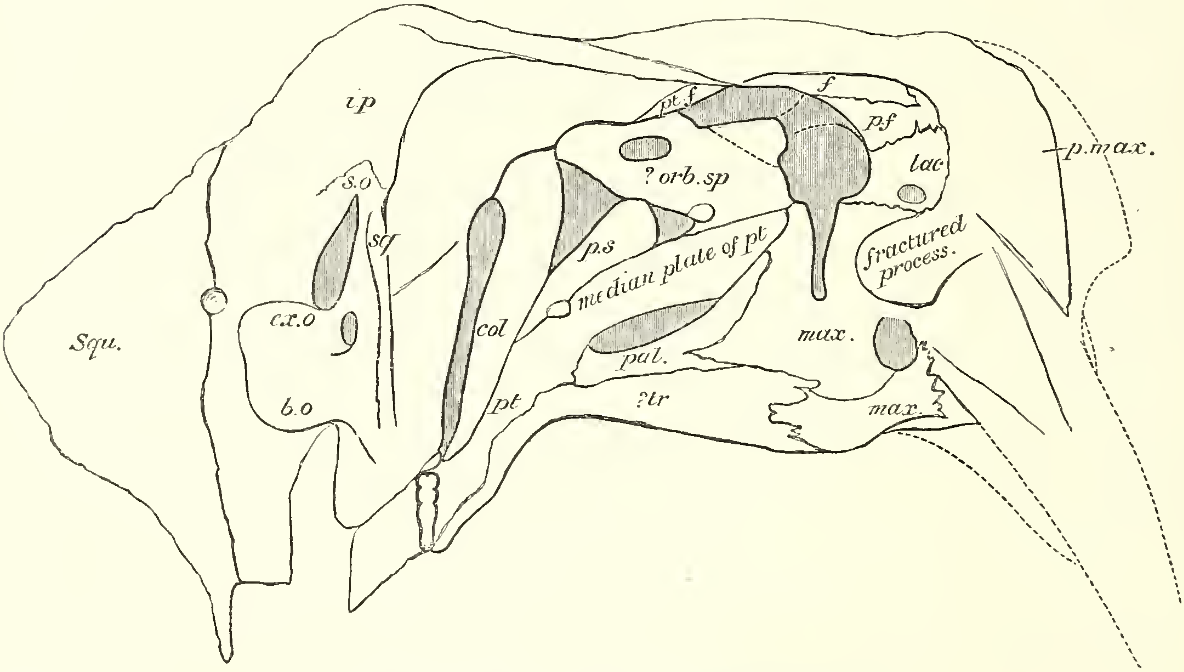
No other specimen which has been described shows so perfectly the form, size, and relations of the quadrate bone, though its individuality has been ignored in Plate XXIV., where only a broken mass of bone is indicated above the condyle, 28. The median descending broken mass, with a black anterior outline in the figure, is part of the squamosal, extending laterally downward over the quadrate bone. The dark oval space in the figure a little behind this bone, and 7 or 8 centims. above the condyle, is part of the proximal surface of the quadrate bone, laid bare by a piece of the squamosal bone being broken away from behind it, so that the bone is received into an arch in the squamosal, and its entire anterior extent is exposed looking obliquely forward and outward. The extreme height of the bone on its external border against the squamosal is 10 centims. from the base of the condyle. At that height there is no indication of its existence in the figure; but in the specimen the bone projects forward a little, and is well defined by a groove above it. Its inner border is convex, and the pterygoid unites with that border, just above the condyle, by an attachment which is 2 centims. deep. The attachment was not firm, for in *D. pardiceps* and other species the quadrate bones are lost from the otherwise perfect skull.

The *Dicynodon pardiceps* (OWEN), South African Cat., No. 70; Brit. Mus., No. 47,045, in many respects the most instructive of all the specimens, has never been adequately described. It shows details of structure, owing to the softness of the matrix, which manifest the union between many of the bones. The remarkably elongated zygomatic arch I find formed chiefly by the squamosal bone, which extends forward to the orbit. The maxillary bone also contributes to form it, extending below and behind the squamosal backward, almost to the descending process. The malar bone forms the lower border of the orbit. It rests externally upon the squamosal, and internally upon the maxillary; its posterior extremity supports the post-frontal bone, and its anterior extremity appears to extend forward to the lachrymal, and inward to meet what I take to be an outwardly directed process of the palatine. The post-frontal is a slender transverse bar, and meets the frontal by a well defined suture. The small parietal lies in front of the parietal foramen; but I do not feel certain that the long oblique posterior processes are rightly referred to that bone, and it would seem as though the analogous structure in Lizards had suggested an explanation which has not been questioned. These bones seem to me to diverge anteriorly to expose the parietal, and they diverge posteriorly to support the squamosals. If they are separate ossifications, they may represent external elements in the Amphibian skull which have remained after a deeper seated ossification was developed, just as in some types basi-temporals remain after the sphenoid is ossified.

In *Dicynodon pardiceps* a groove connects the upper posterior corner of the nasal aperture with the orbit, much as in *Pareiasaurus*, though the depression is but slightly marked. In *D. leoniceps* the region behind the nares is impressed over the depth of the apertures.

In *Dicynodon tigriceps* (OWEN) the configuration of the zygoma and temporal fossa is stated to be most nearly paralleled in *Chelydra*, though the difference is considerable. In another specimen referred to the same species, the inter-orbital space is said to be more completely ossified than in modern Crocodilia, Chelonia, or Lacertilia, and is an approximation to Mammalian structure. The upper anterior angle of the pterygoid is said to join the anterior extension of a cranial bone which may correspond with the pre-sphenoid of Crocodiles, or the orbito-sphenoid of Chelonians. The reference of this specimen to *D. tigriceps* is on several grounds open to question. But the bone which the author regards as pre-sphenoid (5) seems to me to be the median plate of the pterygoid. It is possible that a small ossification above this, which extends obliquely forward and upward, may be the pre-sphenoid, for it is in the position which the pre-sphenoid should occupy. The bone above it, at the back of the orbit, is pierced by a large foramen; a similar foramen appears to be present at the back of the corresponding inter-orbital bone in the specimen termed *Dicynodon Murrayi* (HUXLEY). This perforation I can only regard as the foramen for the olfactory nerve, and the bone as the orbito-sphenoid. A little further back, parallel to the cranial wall, but well separated from it, is a thin flat bone, which extends from the parietal region to the pterygoid; and I therefore identify it as the columella. There appears to be a distinct suture towards the quadrate process between an outer and an inner element of the pterygoid, and a line which might be fracture or suture extends forward to the angle where the posterior end of the palatine is wedged into the bone. This furnishes some evidence, though no proof, that the long external bone may be the transverse bone. In other specimens it appears to be blended with the median element, for no suture has been detected on its palatal aspect. The following figure shows the relations of the median bones of the skull according to the interpretation now given.

Fig. 1.



Skull regarded by OWEN as *Dicynodon tigriceps.*

Obliquely crushed Dicynodont skull, which has lost its external arches and shows the bones between the orbit and squamosal region. The shaded parts are vacuities or spaces occupied with matrix. The fractures produced by the crushing make the recognition of the sutures difficult. The figure now given may be compared with Plate xxxiv., fig. 1, 'South African Catalogue.'

i.p. Inter-parietal.
col. Columella.
p.s. Pre-sphenoid.
orb.sp. Orbito-sphenoid.
pt. Pterygoid.
?tr. Apparently distinct from pterygoid, and, possibly, the transverse bone.
pal. Palatine.
max. Maxillary.
lac. Lachrymal.
p.f. Pre-frontal.
f. Frontal.
pt.f. Post-frontal.

A large number of South African Reptiles were separated by Sir RICHARD OWEN from the Anomodontia, in 1876, to form the order Theriodontia. In several genera the dentition is of the Carnivorous type, and the teeth were regarded as making approximations towards those of Mammals. The order comprised *Procolophon*, which makes a striking resemblance to *Hatteria* in internal structure of the skull;* and in simple unvarying conical form of the teeth approximates to *Pareiasaurus*, though the teeth are never serrated, and never worn down. In most of the Theriodont genera little is preserved of the skull beyond the characters of the snout and the dentition, which are well seen in *Lycosaurus*, *Cynosuchus*, *Tigrisuchus*, *Cynodraco*, *Cynochampsa*, and *Galesaurus*. The chief character in the majority of these genera, which necessarily distinguishes them from *Procolophon*, is the development of a pair of tusks in the position of canine teeth. But this attribute has already been found to characterize the Dicynodontia, in which, however, there are no other teeth developed. The value of the character in classification is unknown, and Sir R. OWEN has suggested that the toothless animals named *Oudenodon* may possibly be the females of *Dicynodon*. The only difference of character in these teeth in Theriodonts is that their margins are serrate, and that the serration extended, in some degree, to all the teeth. But in *Pareiasaurus* and *Anthodon* the serration is well developed, without any indication of canine teeth; while in *Galesaurus* it is reduced to a lateral notch or two, dividing the crown in the molar region into denticles. In some genera the incisor teeth are large and the molar teeth very small. Other characters of the order have been defined by Sir R. OWEN in communications to the Geological Society. In a memoir on *Cynodraco*,† the canine teeth are compared to those of *Machairodus*, while the toothless interval which separates the canines from the lower incisors is found in the Marsupial genus *Didelphis*. In the humerus of *Cynodraco* there is a canal crossed by a bridge of bone at some distance above the distal condyle, on the internal and inferior aspect of the bone. This character is regarded as a characteristic of the Feline family of Carnivora. A similar canal is found in the humerus of Seals, of Insectivora, of Edentates, and Marsupials. This humerus (Brit. Mus., No. 47,910) is about 27 centims. long, and on its external border, at about 10 centims. from the distal end, I find evidence of a second foramen, much smaller than that upon the opposite side, which might be easily overlooked, since it has not been excavated by the Museum "masons." It is about 6 millims. in diameter, and appears to pass obliquely downward through the bone. I have no doubt that this second foramen is homologous with the similarly placed foramen in the humerus of *Hatteria*; but, while its occurrence parallels the humerus with the Rhynchocephalian type, the correspondence is not less close, in this respect, with the Edentate *Cyclothurus*. Humeral bones of Dicynodonts are often broken in the slender part of the shaft in which the foramina are present; and a fragment may

* "On new species of *Procolophon*, &c.," 'Geol. Soc. Quart. Journ.,' vol. 34.

† 'Geol. Soc. Quart. Journ.,' vol. 32, 1876.

show one foramen without indicating the other. There is no reason to suppose that all Anomodonts have a radial as well as an ulnar foramen, for the external foramen is certainly absent in *Galesaurus*; but, when only one foramen is seen, its direction appears to be transverse, and it passes obliquely from the ulnar to the radial side. On referring to drawings which I made in the Senckenberg Museum, at Frankfort-on-the-Main, in 1878, of some fragments of humeri which were described by VON MEYER, and have since been regarded as European Theriodonts by Sir R. OWEN, I find both foramina present, though the radial foramen is relatively small in *Brithopus*.

The ecto-pterygoid bone is stated by Sir R. OWEN to cease to exist in both Theriodonts and Dicynodonts; and this bone never reappears in the Mammalian series. But two specimens in the British Museum suggest doubt whether the bone is absent, or hidden by the pterygoid. Sir R. OWEN also finds in *Iguanodon*, *Scelidosaurus*, and *Pareiasaurus* dental characters which reappear in certain Mammalia, such as the Sloth and Kangaroo. He finds the number of incisors in these fossil Reptiles to be closely comparable with Marsupials. Thus, *Didelphis* and *Cynodraco* have the formula $i\frac{5.5}{4.4}$; and in *Thylacinus*, *Sarcophilus*, and *Cynochampsa* it is $i\frac{4.4}{3.3}$.

This memoir was succeeded by two papers* upon *Platypodosaurus robustus*. The vertebræ are said to differ from those of *Kistecephalus* and *Anthodon* in the less depth of the terminal concavities, in which character they approach *Dicynodon* and *Oudenodon;* but among the Plesiosauria this character is very variable, and seems to me a specific rather than a generic difference. The author compares this vertebral condition to that found in *Echidna*, but I do not find the resemblance close enough for comparison. An element of the sternum is recognized as the foremost sterneber, and identified with the first sternal element in *Ornithorhynchus* and the sternum of modern Lizards; and this bone is inferred to have been one of a series such as is present in Chameleons and Skinks. Its upper border is thought to have joined the coracoid, as in Monotremes, while its lower border may have given attachment to sternal ribs. The scapula is intermediate between that attributed to *Dicynodon* and the scapula of *Kistecephalus*. The nearest resemblance to the humerus is found in *Ornithorhynchus* and *Echidna*. On the digits it is remarked that the ungual phalanges, though relatively shorter, have more the proportions of those of *Echidna* than of *Ornithorhynchus*. What remains of the femur is compared with the proximal part of the bone in *Echidna*. The sacrum is said to be more Mammalian than that of *Dicynodon*, and to come nearer in shape to the Megatherioid Mammals. The Mammalian character is considered to be marked by the breadth of the iliac bones, and the extent of the confluence of the similarly expanded ischia and pubes, and by their confluence at the ischio-pubic symphysis.

* 'Geol. Soc. Quart. Journ.,' vol. 36, p. 414, 1880; and vol. 37, p. 266, 1881.

Subsequently,* Sir R. OWEN urged that the Theriodont dentition was monophyodont, and he proposed to include in the order the European genera *Brithopus*, *Orthopus*, *Rhopalodon*, *Deuterosaurus*, &c., which, so far as the dental characters are known, have very strong incisor teeth. But no evidence has yet been adduced, either in the skull or in the skeleton, that Theriodonts differ from Anomodonts as an ordinal group. For the value of the teeth as an ordinal character is small when so little specialized; and the skull shows few differences in plan.

The Structure of the Skull. (Plate 9, fig. 1.)

The only region of the skull which is at present undescribed is the brain-case. There are four specimens in the British Museum which contribute evidence as to its form and structure. There is also some reason to believe that in certain Dicynodonts the brain-case was very imperfectly ossified. A small skull divided vertically, which is partly figured by Sir R. OWEN ('South African Catalogue,' Plate XXVIII., fig. 4), gives no indication of a defined cerebral cavity. A similar skull, selected for its symmetry, and divided in the same way at my request, is equally free from evidence of a roof to the brain-case, though part of its floor is preserved. I am indebted to Dr. HENRY WOODWARD, F.R.S., for having these preparations made, and to Mr. HALL, the mason, for the skill with which the section was kept to the median line. The only bones shown in section (Plate 9, fig. 1) are the anchylosed basi-occipital and basi-sphenoid at the back of the head, and the pre-maxillary and dentary in front. The basi-occipital (*b.o.*) is much less deep in section than the basi-sphenoid (*b.s.*), which is perforated by a somewhat large carotid canal, extending downward and backward. Anterior to this, the posterior part of the bone sends a short process downward; and the middle part sends a short wide process upward. From its anterior corner a short curve of faint narrow marking, which is not bone, extends forward and upward. A faint oblong marking below this occupies most of the interval between the basi-sphenoid and pre-maxillary, and might correspond to the vomer. From the upper border of the foramen magnum a line extends inward parallel to the basi-occipital. It is succeeded by a large oval mass defined in the same way by a sharp line. Upon this, and partly in front of it, is another oval mass, from which a thickish band is prolonged towards the nasal aperture. These outlines are in the position which the brain should occupy; but other evidence of the form of the brain does not lead me to suppose that the brain substance has been preserved. There is no reason for believing that the brain had this form or extended so far forward in a Dicynodont, though it certainly extended obliquely upward in the same way as do these markings. In the other half of the specimen the united basi-occipital and basi-sphenoid are cut slightly on one side of the median line, and here the superior surface is nearly straight and the basi-sphenoid is rounded in front (Plate 9, fig. 2). There is no trace of the pterygoid, which in all other

* "On the Order Theriodontia," 'Geol. Soc. Quart. Journ.,' vol. 37, p. 261.

specimens is in contact with the basi-sphenoid. I am not aware of any circumstance which would account for the absence of the missing bones, except an original delicacy of texture or absence of ossification, which favoured their removal; but such conditions are not found in any larger skulls. It might, however, be characteristic of young individuals, and indicate this skull to belong to a young animal, but the form of the head is that of a new species.

The Occipital Plate. (Plate 10, figs. 1, 2.)

There exists in the British Museum a detached occipital plate from a Dicynodont skull, which is registered as $\frac{R}{1021}$, and appears to belong to an undescribed species. It is about 8 centims. wide by 6·5 centims. high, thin and rounded on the contour of the upper half, and thicker and notched on the lower half. The plate is flattened on the posterior aspect (fig. 1), but more convex on the anterior face (fig. 2). The greatest antero-posterior measurement through the occipital condyle is 3 centims. The condyle is remarkable for its large size, subquadrate form, slight posterior extension beyond the surrounding bone, and subcentral position upon the occipital plate. It measures 2·5 centims. wide, over the ex-occipital elements, which are subtriangular or subovate convexities, and make the wide upper part of the condyle, the lower part being made by the basi-occipital, which has an unusually large condylar surface, 2 centims. wide, and transversely ovate. The vertical depth of the condyle is 1·6 centim. Its contour is concave superiorly at the foramen magnum, with a parallel convex inferior margin, and small lateral concavities between the ex-occipital and basi-occipital elements. Its ex-occipital extension posteriorly does not exceed 6 millims., while that of the basi-occipital is only half as much. There is a saddle-shaped concavity on the inferior margin of the plate below the basi-occipital; it is concave from side to side, convex from behind forward and downward, and partly divides the two hyp-apophyses below the occipital region, which are here shorter than usual. These processes are convex from side to side, and 1·5 centim. wide. A delicate line descends down the middle of each process, coming from the outer side of the basi-occipital portion of the condyle, and this line I regard as the suture between the basi-occipital and ex-occipital bones. The width of the basi-occipital at the inferior termination of these diverging sutures is 2·7 centims. External to the ex-occipital element in the condyle, and hidden beneath its transverse expansion, is the usual perforation for the vagus nerve, which extends obliquely inward and upward.

The middle part of the occipital plate appears to be formed by the ex-occipital bones. The foramen magnum is 1·7 centim. high, and 1·1 centim. wide at the base, with the sides converging slightly upward, and arching together above. At 6 millims. above the floor of the foramen on each side, a delicate suture diverges outward and upward. I regard it as separating the supra-occipital and ex-occipital. The outward extremities of the sutures are 8 centims. apart, so that this is the width of the supra-occipital.

That bone forms the upper two-thirds of the foramen magnum, and its height above the foramen is 3 centims. At the middle of the superior border there is a median depression of the usual V-shape, but shallow and wide. On the anterior aspect (fig. 2) there are sutural lines somewhat undulating but nearly horizontal, and about 2 centims. below the superior margin of the plate, showing that the ex-occipital bones are overlapped externally by the supra-occipital; and this circumstance may account for the larger dimensions of the ex-occipital bones in the external surfaces figured by Sir R. OWEN.

A transverse suture appears to extend outward from the hyp-apophysis over the lateral notch external to it, separating an anterior plate of bone which rests upon the ex-occipital, and would meet the basi-occipital internally, forming the posterior wall of a canal which descends obliquely outward and downward from the sphenoidal region. This ossification enters into the transverse ex-occipital process termed by OWEN par-occipital, but I cannot trace it superiorly. In *Dicynodon lacerticeps* the left hyp-apophysis shows a tripartite structure, and the outer anterior element I regard as the same as the imperfectly indicated ossification just described. It is obviously in the position of the opisth-otic bone. It must meet the basi-sphenoid if it is not an extension of that bone; and its relations posteriorly with the ex-occipital, and internally with the basi-occipital, its combining with those bones in several other specimens to form an articular cup for the malleus, which bone extends transversely outward to the quadrate, favour its identification as an otic bone. It is possible that the thickened smooth convex surface which extends upward in this specimen from the sphenoidal region on each side, anteriorly to the foramen magnum, may be the squamous extension of the pro-otic bone. The body of the sphenoid is broken away, leaving a rough triangular surface with concave sides, which is less than 1 centim. wide superiorly, 3·3 centims. wide at the base, and 3 centims. in vertical depth.

Above this fracture the anterior aspect of the specimen shows a portion of the posterior walls of the brain-case, which has a high subtriangular outline. The foramen magnum is filled with matrix. In front of it the cerebral cavity expands vertically to a height of 3 centims.; and the transverse width increases, towards the base of the foramen, to about 2 centims.; but its lateral border is undefined in the middle for 1·5 centim., because the internal wall of the cerebral chamber rounds convexly into the lateral external surface of the ex-occipital boundary of the temporal vacuity in a way which indicates the absence of bone and the existence of a vertical vacuity in the cranial wall, which I regard as that of the fifth and optic nerves. The lateral walls of the cerebral chamber here exposed are flattened and oblique, so as to converge backward toward the foramen magnum, and upward. Superiorly the sides round into a slightly flattened convex surface, which is inclined downward and backward to the foramen magnum. At the base, the brain-case appears to sink into a depression in the line of the basi-occipital bone, but this may be the result of development with the chisel. Superiorly two diverging processes, like the forks of an inverted V, descend over the summit of the chamber and give attachment to a bone which, if the constituents of the occipital plate are correctly determined, should theoretically be the inter-parietal. At the inferior outer angle of the chamber there is a bony prominence on each side, which defines a notch like the outlet for a nerve which should give attachment to the ali-sphenoid bone.

A Specimen showing the Relative Height of the Cerebral Chamber. (Plate 10, fig. 3.)

Similar evidence as to the form of the back part of the cerebral chamber is seen in the British Museum specimen 47,056, described in the South African Catalogue, No. 80, as *Dicynodon leoniceps.* It may, perhaps, be another species, for, though it resembles that type in the form of the face, the head is relatively shorter, and the palatal characters, in so far as exposed, are more like those of *Dicynodon pardiceps.* In this skull, which is 32 centims. long as preserved, the bones of the occipital plate have been broken away, exposing a natural cast of a portion of the posterior aspect of the brain cavity, which is 8 centims. high. The mould is inclined obliquely forward, and its straight posterior contour makes an angle of 60° with the horizontal plane of the frontal region produced. The base of the foramen magnum is 2 centims. wide, and at about this height from its floor the mould of the cerebral chamber expands a little transversely, giving a convexity to the lower part of the side, and it contracts superiorly to less than half its width at the summit. The inclined sides diverge outward as they extend forward, and, as far as exposed, they are flattened. The straight posterior contour is rounded convexly from side to side, like the surface of a segment of a cone. In every respect the cerebral characters of this specimen are absolutely the same as in the occipital plate just described, only differing as do a seal and an impression from it.

Another Specimen, showing part of the Brain-case, and some characters of the Back of the Skull. (Plate 11.)

A specimen in the British Museum, numbered R. 868, apparently indicates a new species, distinguished by having the back of the head more than twice as wide as high, with the hyp-apophyses close together. The occipital condyle is hemispherical. The foramen magnum is less high than the condyle is deep. The so-called par-occipital process of the ex-occipital is wider than the rest of the bone. The sides of the narrow bones which form the temporal region are inclined to each other and parallel, and arch high above the descending plate of the squamosal. The back of the head is as wide as in *Ptychognathus latirostris,* but the form of the temporal region is that of *Dicynodon leoniceps.* It may be termed *Dicynodon microtrema.*

The brain-case is crushed a little obliquely downwards. Its base is formed by the anchylosed basi-occipital and basi-sphenoid, and the basi-sphenoid is broken trans versely, with the fracture passing through the internal carotid canal (fig. 2). This canal opens on the cerebral surface by a circular foramen, descends vertically, and forks in an inverted Y-shape, so as to have lateral external openings in front of the middle of the inferior descending hypapophyses. So much of the base of the brain-case as is seen is smooth and flat. It is contracted at the anterior corners by lateral tubercles, which may mark the limit of the cerebellum. Behind them the bones enclose a sub-hemispherical cavity, which is 1·9 centim. wide and about as high, though the height is probably reduced by crushing. The anterior border of this cerebral chamber is smooth, and indicates a vertical lateral vacuity in the skull. The ex-occipital bones appear to extend upward and forward in front of the vacuity, forming a concave roof to the back of the brain, but giving off on each side a lateral process which extends forward beneath the so-called supra-occipital bone. This bone (fig. 1) is small on the occipital surface, is narrow, divided by a vertical suture, and situate high above the foramen magnum, has only a linear longitudinal exposure on the median line of the roof of the skull (fig. 2), and extends forward beneath the spatulate bones, which have commonly been regarded as the parietals. The height of the brain-case where the ex-occipital bones terminate in front is 2·7 centims. And then the (?) supra-occipital bone comes into the roof of the cerebral chamber anteriorly, without increasing its width, so that the transverse measurement of the bones, with the median longitudinal interspace which divides them, remains 1·6 centim. The bone forms two distinct parallel plates, which are subtriangular, compressed laterally, and at the anterior fracture are 1·7 centim. high, with the internal surfaces vertical and parallel, and divided by a space 7 millims. wide, which is narrower at the superior border, where a strip of the supra-occipital, 2 millims. wide anteriorly, and 5 millims. wide posteriorly, is exposed. The width of the bones posteriorly at the summit of the occipital plate is about 1·5 centim., increasing suddenly below the median posterior groove to 2·5 centims. Their height above the occipital condyle, posteriorly, exceeds 3 centims. It is impossible not to recall the description of *Loxomma* given by Dr. Embleton and Mr Atthey* in relation to these bones. They remark, "The upper border of the occipital surface is also the posterior border of the middle part of the skull." "It is formed externally by the mastoids, and between them by a pair of bones corresponding to those which, in *Archegosaurus*, are called by von Meyer . . . supra-occipitals. Immediately below this border runs a transverse line of suture connecting the bones forming the border with those beneath it, namely, next the median line with the single, and, as we deem it, the true supra-occipital, and laterally with the ex-occipitals. The supra-occipital is of a subtriangular form, wider from side to side than from above downwards, and situated on the median line. It is doubtful whether or not the median suture passes through it. Below, it articulates with the ex-occipitals."

When the external surface of the specimen R. 1021 is compared with the corresponding portion of R. 868 there is a close resemblance, though the latter shows the

* 'Ann. Mag. Nat. Hist.,' July, 1874, p. 50.

squamosal bone on its external border, and two pairs of bones on its superior border. And since the sutures in this occipital plate are obliterated between the basi-occipital and ex-occipital bones, there is an *a priori* probability that they would be obliterated between the ex-occipitals and the supra-occipital, and, therefore, that the composition of the occipital plate in R. 868 may have originally been the same as in R. 1021, so that the supra-occipital would enter into its foramen magnum. Hence, there is some ground for supposing that the pair of distinct bones which surmounts the occipital plate is a pair of inter-parietal bones, with an immense antero-posterior extension. A pair of bones behind the parietals has been figured by Professor FRITSCH* in many genera of Labyrinthodontia, so that they would seem to characterize that order; but the small size of his specimens and their flattened condition would be unfavourable for the identification of a supra-occipital bone below them, if it exists, and these bones, if distinct from the supra-occipital, would also be inter-parietal.

The bone which flanks the inter-parietal in *Loxomma* is termed by Dr. EMBLETON, mastoid; and is the epi-otic of Dr. FRITSCH, though this determination has been questioned.

It is well seen at the side of the skull of R. 868 as a broad plate of bone, which extends between the inter-parietal and the squamosal, and rests upon the supra-occipital part of the occipital plate, so that in plan of construction of this region of the skull there is a close approximation to the Labyrinthodont type, which, in so far as I can judge from Mr. MAW's specimen of *Loxomma* in the British Museum, has a vertical occipital region.

The transverse extent of this (?) epi-otic bone is 4 centims. in R. 868; its position is oblique, and its breadth about 1·5 centim. In other species the position of this bone is different; in *Dicynodon leoniceps* it appears to descend obliquely downward, outward, and backward. In *Dicynodon tigriceps* its development appears to be greater upon the roof of the skull, where it seems to me to overlap the parietal bone, and to be defined by difference of colour of the bone, and a convex sutural border which allows the undivided parietal to extend back between the epi-otic bones.

The epi-otic bones are in contact with the pair of remarkable bones which form the roof of the temporal region of the skull. These bones are each about 1·8 centim. wide, somewhat inclined towards each other so as to look upward and outward, with the surface slightly convex from within outward, and the external margin, which projects well beyond the inter-parietal, is well rounded. The anterior fracture shows the thin blade-like substance of the bone. Its sides are sub-parallel, except that posteriorly it diverges outward and downward, so as to rest on each side upon the margin of the epi-otic. These bones have usually been regarded as the parietals. But they appear probably to be distinct plates, which are developed in the position where the muscles which work the lower jaw are attached. In *Dicynodon leoniceps* these bones, which have a similar smooth oblique surface, are 20 centims. long and 3 centims.

* 'Fauna der Gaskohle und der Kalksteine der Permformation Böhmens.'

wide. They diverge but little posteriorly, and on the inner side appear to show well defined sutures separating them from the bone beneath, which would necessarily be the inter-parietal. The bones diverge anteriorly to disclose the parietal foramen, and appear to show the parietal in front of them, as they open in a V shape. I regard the lateral suture as following the divarication anteriorly round the impressed muscular area to the post-frontal bone. In the genus indicated by *Dicynodon tigriceps* their development is different, because the roof of the skull is flat. They are narrow concave strips of bone which extend round the margins of the temporal vacuities, so as to display the parietal bones between them.

These bones seem to me to be called into existence by the muscular attachment, and they may correspond to the parietals of higher Vertebrates, where the single Reptilian parietal probably becomes absorbed.

Turning to the base of the specimen R. 868, the basi-occipital and basi-sphenoid are seen to be anchylosed together. The union is marked by a transverse ridge, behind which the basi-occipital extends for 1·7 centim. The combined bones are produced downward and outward into two strong processes, divided by a longitudinal median channel. The extremity of each process forms a large concave articular facet, which looks outward and downward, and is somewhat heart-shaped, and about 1·6 centim. wide. I regard this surface as having given attachment to a bone, the mastoid, which extended transversely outward to the squamosal and quadrate, as in *D. lacerticeps* and other specimens. A large hemispherical cavity which is opposite to it in the squamosal bone, is 2 centims. wide, and looks forward and downward, may have given partial attachment to its other end. The distance between these surfaces for the malleus is about 4·5 centims.

The squamosal bone is a large vertical plate, which forms the whole of the lateral expansion of the back of the head, external to the occipital plate (fig. 1). It is 6 centims. wide on the anterior aspect, and 5 centims. wide in the middle, posteriorly. It is 8 centims. high. The bone becomes compressed as it extends downward and outward, so as to form a support for the quadrate bone, which was placed in front of its distal end, as in *D. leoniceps* and other species, though the bone is lost from this example, as in *D. pardiceps*, *D. tigriceps*, and other species. An impressed surface which received it is but slightly indented, so that the attachment was loose and squamous. The main portion of the squamosal bone extends the plane of the ex-occipital outward, and its external border descends in a curve which is at first concave and then convex, so that the bone widens as it extends distally. Superiorly it sends a few sutural processes inward over the ex-occipital, and it extends in front of that bone anteriorly. Its transverse superior contour is concave. From the outer upper angle a greatly compressed and oblique bar is given off, which extends forward to form the external border of the temporal foss, though it is fractured (fig. 2), and the foss is not defined in this specimen.

It may be worth recording that the thickness of the occipital plate suddenly increases from 8 millims. to 2 centims. in passing inward to form the thickened wall of the cerebral chamber. There is some indication that this thickening may be due to another bone, the pro-otic, resting in front of the ex-occipital, with which it is now closely blended, for a transverse fracture on the right side appears to show a nearly obliterated suture, which extends upward from the articular cup in the hypapophysis over the lateral plate by an oblique channel which coincides with the thickening of the plate.

The Back of a Skull which shows the whole of Brain-case and the Relation of the Quadrate Bone to the Pterygoid and Squamosal. (Plate 12.)

The British Museum specimen (R. 866) comprises much of the back of a skull posterior to the parietal foramen, and indicates a new species. The squamosal bones are directed backward, so that the part of the squamosal and occipital plate which is anterior in the last specimen is lateral in this. The squamosal bone has its vertical contour convex posteriorly, and concave distally. The transverse extension of the bone extends vertically above the level of the post-parietal region of the brain-case, which is convex in length as well as transversely. The quadrate bone passes under an arch in the squamosal, so that its posterior and articular part is hidden under that bone. The pterygoid bone is vertically compressed posteriorly, so as to form a sharp ridge on the palate. The bones above the brain-case form a narrow vertical plate which expands transversely. In this species the atlas and axis are anchylosed. Every character separates it from the other described species. It may be named *Dicynodon* (*Tropidostoma*) *Dunnii.*

The squamosal plates, which diverge backward and outward, are remarkably convex from above downward. They approximate superiorly, so that the transverse width over the middle of the concavities in which they terminate is a little over 5 centims. Posteriorly their mutually inclined surfaces are separated by a wedge-shaped vacuity, which is $2\frac{1}{2}$ centims. wide superiorly, and extends forward between the roof bones of the skull for more than 5 centims. (fig. 4), becoming a mere groove in front (fig. 1) which can be traced along the median suture. The transverse width posteriorly in the middle height of the squamosals is over 11 centims., while the measurement over the articulation for the lower jaw is 7 centims. Hence the squamosal bones enclose an oval basin-shaped excavation at the back of the head, where the atlas and axis and succeeding cervical vertebræ are attached. The specimen gives no indication of *post-mortem* compression. The condition of the quadrate bone is unusual. It commonly lies in front of the distal end of the squamosal bone, and forms a flattened wedge, convex on its superior border, concave posteriorly; here it contracts distally to form the long narrow ovate convex condyle, which is directed obliquely forward, and crossed by a slight longitudinal groove. Each condyle is about 3 centims. long (though the left appears to be slightly shorter), and anteriorly measures 1·3 centim. transversely. The condyles are entirely hidden from side view by the squamosal bones. A deep groove separates these two bones; and, on the inner side of the condyle, a second concavity separates the articulation from the small malleus. The external lateral aspect of the quadrate is defined from the squamosal by a deep horse-shoe-shaped groove, which marks posteriorly the limit at which the squamosal overlaps it (fig. 2). The extreme height of this anterior part of the bone is 5 centims.; its extreme antero-posterior extent in front of the squamosal and above its short pterygoid process is 2·5 centims. The squamosal descends in front of it as a slender process, which does not reach down to the pterygoid process. The lateral aspect of the quadrate bone is divided into two areas by an oblique ridge, which extends forward and upward from the anterior termination of the condyle articulation. The superior posterior area is higher than wide, flattened, but slightly concave vertically. The inferior wedge-shaped area looks obliquely downward, outward, and forward. The pterygoid process is about 1·2 centim. deep, and 8 millims. long, and about 6 millims. thick, compressed towards the inferior and superior margins. It is continuous with the quadrate process of the pterygoid, from which it is divided by a vertical suture. Posteriorly the quadrate bone passes obliquely through the squamosal so as to occupy a large area on the posterior face of that bone. In this species the quadrate bone has a general resemblance to the quadrate of *Ichthyosaurus*, though that genus does not develop a pterygoid process, which I have seen in no other Dicynodont. In *Dicynodon leoniceps* the quadrate bone is of different form, and has no extension in front of its condyles, but above the trochlear end it rises in a quarter of a circle, vertical externally, and convex on the inner border, $5\frac{1}{2}$ centims. high, and as wide as high, just above the articulation. It is strong, very slightly convex, and makes an angle of nearly 45° with the longitudinal articulation. Its antero-posterior thickness above the condyle is $4\frac{1}{2}$ centims., and superiorly the posterior surface, which was contained within the squamosal bone, is convex from behind, upward and forward toward the sharp superior margin, which, as preserved, projects forward $1\frac{1}{2}$ centim. in advance of the squamosal bone. Its external margin appears to have been overlapped and hidden, as in all the other species, by a descending squamosal process.

On the palatal aspect (fig. 3) there is a deep saddle-shaped channel over the two downwardly directed processes, which appear to be formed by the basi-occipital and basi-sphenoid bones, which is very convex from behind forward, because the processes converge in front. Each process is $2\frac{1}{2}$ centims. long, and has a long ovate form. The transverse measurement over them is less than 3 centims. On the outer posterior side, wedged in between the basi-occipital part of the process and the quadrate bone, is a relatively small ossification, identical with that which I have recognized in other species and regard as the malleus. It extends obliquely outward and backward. Its surface is less than a centimetre square, and is obliquely convex from behind forward. External to the convex sphenoido-occipital processes the posterior quadrate bars of the pterygoid, which are 2·5 centims. long and about 1·5 deep, converge forward and inward. They do not extend further forward than the processes with which they are in contact, but merge in the median mass of the pterygoid, which is 3 centims. deep and 1·7 centim. wide, with convex sides, but vertically compressed to a sharp palatal ridge and a similar median ridge on its upper surface. The mass is fractured vertically in front, but shows no trace of a median suture (fig. 1). The median blunt longitudinal pterygoid keel distinguishes this species from all others, and nothing could be less like the expanded horizontal plates which the pterygoid bones form in other Anomodonts.

The squamosal bones appear to have a considerable lateral extension forward in the upper part of the wall of the temporal region in advance of the origin of the zygomatic process. The antero-posterior measurement, as preserved, from the middle of the convex posterior border is 10 centims. The zygomatic process, which is directed at first obliquely outward, and then parallel to the temporal region of the skull, is given off in the middle of the width of the bone by a vertical attachment 5 centims. high and fully a centimetre wide, which becomes narrower superiorly as the zygomatic process thins. Beneath this branch the lateral contour of the bone is convex vertically for $3\frac{1}{2}$ centims. down to the quadrate bone (fig. 2); and this convexity with the superior process defines the posterior vertical concavity in the bone, which increases in depth superiorly as this part of the bone becomes narrower. Inferiorly the concavity is lost. At the articulation for the lower jaw the squamosal bone appears to form the entire height of the back of the skull, which is about 12 centims.; but distally, where it rests upon the quadrate, it contracts to a width of 1·7 centim., and both the anterior and posterior borders of the descending process are concave for a length of about 3 centims. At 1 centim. from the distal end there is a transverse suture, and an ossification is displaced, which is 1 centim. deep, 2·5 centims. from back to front, and over a centimetre from within outward. Its distal surface appears to have been articular, and is convex from within outward and gently convex from front to back, as though it formed the outer half of the condylar articulation for the lower jaw. There is a similar transverse division on the left side, where the ossification is *in situ*, but the outer part of both bones is there broken away. In its relations to the quadrate and squamosal this ossification, if its distinctness is established, would correspond to the quadrato-jugal bone, which is not otherwise seen in Anomodonts.

The part of the squamosal bone which is anterior to the zygomatic process is about 4 centims. deep posteriorly, is sub-triangular, contracts in depth anteriorly. The post-parietal bones rest upon it superiorly, and it is in contact with other bones in front which converge forward and form the vertical median post-orbital plate. In front of the delicate process which extends above the superior border of the quadrate is a foramen from which a concave channel extends obliquely upward, over a sub-quadrate bone 3 centims. high and as wide, which lies in the wall of the brain case, widening it transversely above the quadrate process of the pterygoid, which is itself in close contact with the basi-sphenoid bone; so that its position is that of the ali-sphenoid bone, or pro-otic, and it may be compared with the thickened plate which flanks the brain-case in the previously-described specimen (Plate 10, fig. 2). The transverse width between the vertical anterior borders of these bones exceeds 2 centims. Anterior to them, and above them, the median vertical plate extends forward. It is 1·5 centim. wide at its base, which is concave from side to side, straight, and ascends forward and upward. At the anterior fracture the plate is over 3 centims. deep, and is formed of a pair of lamellar bones, in contact in the median line, with concave external sides, so that the transverse measurement in the middle is only 6 millims., but they widen again superiorly to 1·5 centim. before forming the transverse lateral expansions like the cross-piece of a capital T (fig. 1).

These bones, regarded as parietals, are in contact, inferiorly, with a median bone behind, which may be the inter-parietal, while the post-parietals, which appear to cover them superiorly and posteriorly, seem to terminate at about the anterior fracture; but fractures are numerous, and sutures so nearly obliterated, that it is difficult to determine the structure with certainty. This interpretation would bring the parietal bones into the roof of the cerebral chamber in advance of the inter-parietal, with a narrow concave surface, from the lateral borders of which bone appears to have extended downward, so as to define a vertical vacuity in the median plate, though the descending processes are broken away.

The superior surface of the temporal region is divided by a well-marked median suture, which becomes wider posteriorly, and its lateral margins are prolonged backward in a V form to the posterior extremity of the squamosal, though the level of this groove is much below that of the external zygomatic bar, from which it is separated by a deep narrow depression which extends downward and outward over the convexity of the anterior part of the squamosal bone. The postero-anterior convexity of the temporal region ascends in vertical position as it extends forward and diminishes in transverse breadth, till, at about 4 centims. behind the anterior fracture, it merges in the transverse ledge, which extends outward on each side of the median vertical plate, and as it curves forward it comes nearer to the superior surface of the skull. Seen from above, this temporal region has the sides sub-parallel; the transverse section posteriorly is half a circle, about $3\frac{1}{2}$ centims. wide. The width is scarcely diminished anteriorly, but the convexity diminishes, until at the anterior fracture it is replaced by a slight median concavity, and nothing remains of the convexity but the rounded margins of the transverse plate, which is less than half a centim. thick at the fracture, and has a transverse extension outward of 1 centim. beyond the vertical plate. From back to front this superior area is regularly convex, and the contour probably resembled that of *Dicynodon pardiceps*, but behind the convexity there is a slight concavity along the outwardly diverging surfaces. It is possible that the anterior extension of the inferior part of this brain-case was no greater than in the specimen previously described (Plate 10, fig. 2).

Within the post-occipital basin the remains are exposed of what I regard as an extremely thin plate of bone. Its vertical extent, from the side of the axis upward, is $5\frac{1}{2}$ centims. It is over 2 centims. wide, of a ribbon-like thickness, and curves convexly outward, like the squamosal bone, with which its anterior margin may be in contact. Its position is suggestive of part of the hyoid.

The cervical vertebræ of this specimen are described under the vertebral column.

On a New Skull of Dicynodon tigriceps (OWEN) *showing the Sutures of the Pre-parietal Surface.* (Plate 13.)

In the larger skulls of Anomodonts, it is rare to find the sutures between the bones distinctly shown, and I therefore give some notice of a specimen in the BAIN Collection, hitherto undetermined, which I regard as the anterior part of the skull of *Dicynodon tigriceps.* It is slightly distorted, and shows some differences of character in the greater depth of the maxillary bone, in the more anterior direction of the great tusk, and, apparently, in the greater prolongation forward of the pre-maxillary bones to form the cutting edge of the jaw. These characters are such as might be expected to characterize a species, but they are, I conceive, such as might vary with age; and Mr. BOULENGER has shown me such extraordinary examples of augmentation of size and variation of proportion in the skulls of some young and adult Frogs that, in view of the close correspondence of form and size in the bones of the pre-parietal region of the upper surface of the head, I do not feel justified, with only a distorted fragment to work upon, in separating the specimen from the type to which it is obviously closely allied.

The specimen shows the relations of the maxillary, pre-maxillary, sub-narial, nasal, pre-frontal, lachrymal, frontal, and post-frontal bones; while the nares are exceptionally well seen (fig. 2).

The length of the fragment is only about 30 centims. The pre-maxillary bone is shown by a transverse polished section to be single. It is inclined obliquely forward and downward, and is constricted posteriorly where it forms the anterior and superior borders of the nares. Here its transverse width is 8·5 centims., almost exactly the same as in the type skull of *D. tigriceps.* It extends backward in a rectangular wedge, and penetrates in the median line for some distance between the nasal bones. The length of the straight suture by which it joins each nasal bone is 6 centims. The length of the bone in the median line to the transverse polished section is 11 centims., and may have been several centimetres more: these measurements correspond with *D. tigriceps.* In front of the nares the lateral parts of the bone bend somewhat abruptly downward and outward for 4·5 centims. to the straight suture with the maxillary bone, which is parallel to the superior contour when seen from the side, and about 8 centims. long: its direction is also parallel to the median line of the skull when seen from above. The posterior lateral border of the bone is concavely notched out superiorly to form the front border of the narine, and inferiorly it slopes to form the floor on which the sub-narial bone rests.

The maxillary bone is flattened, oblong, and oblique in position, being inclined forward and downward. It contributes the inferior border to the narine, and supports the larger part of the sub-narial bone; but may be excluded from the external margin of the orbit by the malar reaching back to meet the lachrymal. It is 10 centims. deep and nearly 20 centims. long. A shallow depression or groove runs over the bone, parallel to the pre-maxillary suture and 3 or 4 centimetres below it, so as to define a superior convex ridge. The inferior border of the bone extends for 24 centims. in front of the lachrymal corner of the orbit; it is rounded, and contains the base of the tooth, which is sheathed in bone to its fractured extremity, where it measures 3 centims. in diameter.

A fracture shows the root of the tooth of the same size extending for 10 centims. through the surrounding sheath, and it probably extends nearly to the orbit. Its position is parallel to the suture between the pre-maxillary and maxillary, but 6·5 centims. behind it. It is, therefore, not in the place of the canine tooth of a Carnivorous Mammal, and cannot be so determined; but, being entirely in the middle of the maxillary bone, presents a character only found in the lower Mammalia.

The nasal bones are flattened above, and they extend transversely over the hinder part of the nares in a pair of thickened bulbous processes, like horns. The transverse width over the bones is about 19 centims. They meet by a well defined median suture, which is 5 or 6 centims. long; and the anterior margins of the frontal bones rise above them in a slight ridge. These straight sutures, 8 centims. long, converge backward, so that the nasal bones penetrate a little between the frontal bones, but to a less extent than the pre-maxillary bone penetrates them. The superior surface of each bone is convex from front to back, and concave from the median line outward towards the external horn. The external antero-posterior extent of the bone is about 7 centims., and this surface is chiefly occupied with the transverse convexity of the nasal horn, which is 4 centims. thick, though the thickness of the bone greatly diminishes towards its junction with the pre-maxillary. Inferiorly the nasal rests upon the lachrymal bone, which extends as a narrow strip into the narine, on the plan of *Ichthyosaurus*, so as to separate the nasal bone from the maxillary. A deep groove behind the nasal horn separates the nasal bone from the pre-frontal.

The sub-narial bone lies within the floor of the narine. This cavity is transversely ovate, 5 centims. long by 2 centims. deep internally, but 4·5 centims. deep to the outer limit of the sub-narial bone; and a centimetre below that there is a sharp ridge on the maxillary, which marks the lower border of the nasal aperture. The sub-narial bone is 5 centims. wide and 3·5 centims. deep, slightly concave from front to back, and inclined obliquely downward and outward. It is seen on both sides of the skull. When compared with the sub-narial bone in *Pareiasaurus*, this condition is interesting, as the bone is entirely withdrawn from the external suture on the face. And its position is such as to suggest that it may be the germ of the turbinal bones of the Mammalia.

The internal border of the orbit is flattened, and is limited externally by a somewhat thickened rounded edge, in the anterior corner of which lies the lachrymal bone. It is 4 centims. deep within the orbit, 2 centims. wide on the lateral border in front of the perforation for the canal, while the width to the extremity of the narrow tongue, which is prolonged superiorly into the narine, is 5 centims. The lachrymal perforation is 1·4 centim. long and half as wide, with a depressed rounded border.

Above the lachrymal bone, and behind the nasal bone, is the pre-frontal bone. It is a small ossification above the anterior corner of the orbit, where it projects outward in a second sharp but small pyramidal process, similar to the process of the nasal bone. The width over these processes is about the same as that across the nasal horns. But the pre-frontal bone is no larger than the lachrymal. It measures about 4 centims. in the vertical direction, and the transverse and antero-posterior measurements are about the same; so that its small size would suggest that the absence of this bone in the skulls of higher Vertebrates is probably a consequence of the frontal bone continuing to ossify at its expense. Its largest surface is within the orbital border; and the contour of the orbit causes it to narrow inferiorly in the lateral view to its junction with the lachrymal bone, above which the transverse spine or horn extends.

The frontal bones are a pair of large flattened horizontal ossifications which extend between the orbits, and meet in the median line in a slight ridge, which is only a little more elevated than the anterior margin which marks their union with the nasal bones, but there is a conspicuous though small depression where these bones meet in the median line of the skull. Their lateral margins are compressed and convex on the under side; but the lateral contours are concave from back to front, where they form the superior borders of the orbits. Posteriorly, the parietal bones are broken away, apparently at the suture, and on the right side the post-frontal bone is broken away in the line of suture. And these fractures show that the frontal bones extend backward in the median line in a broad Λ shape, which is rather more pronounced than the corresponding contour of the nasal bones; so that each frontal approximates to a rhombic form, with the sharp angles outward and forward, and made by the pre-frontal bones. The least transverse width between the orbits is about 14 centims. The antero-posterior length in the median suture, between the frontals, is fully 10 centims.; the length in the middle of each frontal, from the nasal to the post-frontal suture, is 8·5 centims., and on the orbital margin, between the post-frontal and pre-frontal, the measurement is about 5 centims., or 9 centims. to the deep notch between the pre-frontal and nasal. The length of the post-frontal suture on the posterior border is 5·5 centims., where it appears to be separated from the anterior end of the parietal by a ridge. The flattened superior surfaces of the frontal bones are somewhat concave, owing to the median ridge developed between them anteriorly, but where the ridge dies away posteriorly there is a slight transverse concavity in the middle parts of the bones. The similar depression in the suture at the posterior extremity of the united frontals, when compared to that seen in the type skull, makes it highly probable that the parietal foramen in this skull was similarly situate and conditioned.*

The palatal aspect shows the union of the palatine bone with the maxillary, the extension of the palatine on the internal border of the pterygoid, the median approximation of the pterygoids posteriorly against a compressed median plate, 2 centims. high, 1·4 centim. wide on the palate, and 9 millims. wide superiorly, which appears to be the pre-sphenoid; and a flattened bone descends on to the outer surface of the pterygoid, diverging outward and forward, which is in the position of the columella, but is only seen on the right side.

The Quadrate Bone. (Plate 10, figs. 4–6).

Though several Anomodont skulls in the British Museum have lost their quadrate bones, the collection contains but one specimen separated from the skull, and that bone, hitherto undetermined, is without record of the locality where found, beyond being kept with specimens presented by Mr. BAIN.

It is in a pale grey matrix, which has been completely removed, so that the external form of the bone is fully shown. It agrees substantially with the bones which are attached to the skulls in every character but one, and that is in the presence of a very large foramen or notch, which runs through the bone above the distal articulation. The bone indicates a larger skull than any which has been described, and may eventually be referred to a new genus.

The bone consists of a trochlear articulation, from which a strong wide wedge-shaped process ascends obliquely, and shows, on the middle of what I regard as the inner margin, a large vertical pit for the pterygoid bone (fig. 4).

The trochlear articulation (fig. 6) consists of two well defined condyles, which are convex from behind forward, and separated by a deep wide median channel. The inner condyle, as preserved, is the narrower, but this is probably due in part to a fracture, which has removed its inner margin. The transverse measurement from within outward is 7·5 centims.; and the antero-posterior measurement of the outer condyle is less than 6 centims., and of the inner condyle more than 6·5 centims. Both condyles project forward, and are well defined by the less development anteriorly of the bone which rises from the articulation; but the inner condyle appears to have worked in a well defined excavation, and is defined posteriorly by a transverse emargination or groove above the articular surface. The external condyle has the external margin convex from front to back; while the margins of the other condyle appear to be straight and parallel.

* In the fossil No. 36,235, the parietal foramen is an ovate perforation 1·3 centim. long and 9 millims. wide, with an elevated rounded rim, the transverse measurement over which is 2·3 centims., and the length about 2·8 centims. This border is less than half a centim. wide in front, but widens laterally and posteriorly; and all round it the bone is depressed.

The channel between the condyles is about 3 centims. wide, and fully 1·5 centim. deep, highly inclined towards the inner condyle, and less inclined on the surface which contributes to the outer condyle. This channel makes the anterior margin concave between the condyles, and causes a corresponding concave contour on the hinder border.

The greater length of the inner condyle appears to be correlated with the greater antero-posterior extent of the bone which rises above it. Above the outer condyle this is a small and comparatively slender vertical process, which is fractured, so that its height is not seen. It measures 3·3 centims. from front to back, and less than a centimetre and a half transversely where thickest. The convex external surface is somewhat irregular, as though it were impressed above the condyle by giving attachment to a bone, which may have been the quadrato-jugal or possibly the squamosal, since there is at present no conclusive evidence of the quadrato-jugal as a normal element in an Anomodont skull. The height from the base of the articulation to the fracture of this process is 5·7 centims. The process is separated from the strong bony wedge which rises above the inner condyle by the large notch or channel which is situate above the channel between the condyles, from which it is separated by a thickness of bone of about 2·2 centims. in front and 1·5 centim. behind. The base of the channel is concave from back to front, as well as from side to side; its external border was most produced anteriorly, and its inner border most produced posteriorly. The sides appear to have converged upward without meeting. The greatest width of the perforation in front is about 2·5 centims.

The plate or wedge of bone which rises above the inner condyle terminates superiorly in a thin compressed edge, convex from front to back, which is inclined towards the outer side in front, and towards the inner side behind, so as to obliquely cross the direction of the condyle. The height to this edge from the condylar surface is 10 centims.; its greatest antero-posterior extent is 9 centims. The wedge is somewhat cut into at the base, in front, by the median supra-condylar perforation, while the inner posterior side is excavated for a bony attachment. Otherwise the lateral surfaces are convex from front to back and converge from below upward, so as to meet in a sharp edge which extends from the back of the condyle to the front. On the posterior side the base of this wedge expands to the summit of the outer condyle. I have no doubt, from the evidence of the specimens described, that the larger part of this wedge was received into the squamosal bone. The large vacuity on the inner hinder border of the wedge, which is 4 centims. high and 1·5 centim. wide, is in the position in which the pterygoid bone commonly meets the quadrate; though in *Ichthyosaurus* a similar pit appears to be formed by the malleus.

On the whole, the bone approaches closely to the quadrate of *Ichthyosaurus*, notwithstanding the difference of the supra-condylar perforation; for, if the external ascending process of this South African quadrate were removed, its external surface would have the same concavity as the quadrate of *Ichthyosaurus* where it faces towards the quadrato-jugal bone. Moreover, it would correspond in plan to the quadrate of *Hatteria*, except that it rather resembles *Ichthyosaurus* in the absence of a pterygoid process, in the wedge-like supra-condylar mass, and its superior termination in a sharp margin. The divergence of character, in which it varies from the quadrate of other Anomodonts, and approximates to *Ichthyosaurus*, would warrant its reference to a new genus.

On a somewhat crushed and imperfect Skull of Dicynodon Copei, SEELEY. (See Plate 14.)

The skull numbered 47,074 is distorted, but indicates a new species of *Dicynodon*, which appears to resemble the type named by Professor COPE *Lystrosaurus*,* in which the face is vertical to the superior surface of the head. The nares approximate as closely as possible without being confluent, are circular, and inferior in position to the large circular orbits, which are posterior to the nares. The large teeth descend vertically.

The palate has been very fully excavated (fig. 3), and shows the wide smooth surface of the basi-sphenoid, slightly concave from side to side, and less convex from back to front. At each side, stretching between it and the quadrate bone, is the pair of bones with saddle-shaped surfaces, originally figured† in *Dicynodon lacerticeps* (OWEN), and already recognized in other Dicynodonts. In a line with their anterior margin the irregular transverse suture is seen, which marks the overlap of the pterygoid upon the basi-sphenoid. The lateral surfaces of this mass converge forward to make deep notches, external to which the quadrate processes of the pterygoid are prolonged outward and backward as thin oblique plates, which reach the external borders of the sub-quadrate saddle-shaped bones already referred to, which are regarded as the malleus. In front of these, the quadrate processes of the pterygoid bone are considerably constricted; and then an anterior pair of processes diverge forward and outward, so as to terminate behind the maxillary teeth, where a thin plate of the maxillary overlaps the pterygoid externally, but only on the upper part of the anterior process. In the constricted middle plate of the pterygoid is a long median vacuity, lanceolate behind, and tapering in front to a slender point. It is defined laterally by very slender plates, which converge inward and forward to form a single median plate, reaching forward to the maxillary region; and this plate may probably be identified as the vomer.

Stretching along the inner side of each anterior bar of the pterygoid is the palatine bone, which widens as it extends forward, so as to enclose with the slender V-shaped vomerine plate a pair of long oval vacuities in the palate, which I regard as the

* 'Amer. Phil. Soc. Proc.,' vol. 11, 1870, p. 419.

† 'Geol. Soc. Trans.,' 2nd Series, vol. 7.

palato-nares. The lateral excavation shows the palatine plate to extend upward and inward as in other specimens.

The distorted condition of the specimen makes detailed measurement of little value. The head is about 13 centims. long, and the height of the vertical face is about 8·5 centims. from the cutting margin of the jaw to the flattened frontal region. The circular nares are rather above the middle of this height; each is 2 centims. in diameter, and the bar which divides them, formed by the nasal bones above and the pre-maxillary bone below, is half a centim. wide. The pre-maxillary bone is extremely narrow (about 2 centims.), and may be divided by a median suture, though the state of preservation does not demonstrate this point, and the appearance may be delusive. The circular orbit is 4 centims. in diameter; the malar bone extends internally to the post-frontal at its hinder border, and both bones are overlapped externally by the squamosal, which at once rises to a level with the crown of the head as it extends backward. At the back of the head is a slender sigmoid bone, expanded at both ends, about 9·5 centims. long, which may be the clavicle.

These characters amply establish the distinctness of this species, and make its reference to a distinct genus not improbable; but I do not regard the vertical opposition of the pre-maxillary and mandible on which *Lystrosaurus* was founded as a sufficient definition of the genus.

The Skull of Hyorhynchus platyceps, SEELEY. (Plate 15, figs. 1–3.)

A skull, which is imperfect both in front and behind, registered in the British Museum as R. 872, received from Mr. THOMAS BAIN, is so remarkable in its form that I regard it as probably indicating a new genus. It is characterized by a slender angular Pig-like snout, relatively large orbits, and a narrow parietal region.

The upper surface of the head is flattened, slightly convex from front to back in the median line, with the superior borders of the orbits somewhat elevated, so as to make the frontal bones between them longitudinally concave. The post-orbital region has a length of 3 centims. as preserved, and is about 12 millims. wide at the posterior fracture, where a transverse section shows that it is the summit of a vertically ovate region of the brain-case. As in so many other allied forms, its lateral walls diverge outward as they extend forward to the posterior angle of the orbit, where the post-frontal bone extends transversely outward. The post-frontal is very slender. It is directed at first outward and downward, and then downward and forward, making the posterior boundary of the orbit. This aperture on each side of the head looks outward and a little upward, is 4 centims. long, by 3·3 centims. deep. The width of each frontal bone from the median suture to the orbital border is 1·6 centim. in the middle of the orbit. The sutures are badly defined. The parietal appears to be overlapped by the post-parietal bone, which extends forward to the post-frontal, and has the side flattened and obliquely inclined. At about 1·5 centim. behind the orbits is the long ovate parietal foramen, which appears to be 1 centim. long. At the anterior corners of the orbits are narrow pre-frontal bones, crossed by angular ridges which extend forward and separate the flat roof to the snout from the slightly inclined ant-orbital wall. The transverse width at the corners of the orbits is 3·5 centims. at 7 centims. in advance of the posterior fracture. The roof bones extend for 4 centims. forward in advance of this, converging to about 12 millims. wide at the anterior fracture, so that the angular ridge which borders the area laterally is slightly concave in length. The vertical height of the back of the skull in the post-frontal region is about 5·7 millims.; anteriorly the vertical depth to the maxillary plate of the palate is a little over 4 centims. The transverse width at the base of the hinder border of the orbit is 8 centims., while anteriorly at the maxillary plate it is 4 centims.

The maxillary bone is very imperfectly preserved, and is best seen on the right side, where a part of its inferior palatal border extends backward horizontally towards the orbit, but extends below it. Its rounded lateral border makes a considerable angle with the palatal surface of the bone, which is reflected inward and downward. In the part of the alveolar border which is preserved I can detect no indication of dentition. The form of the head would have suggested teeth of the Gennetotherioid type, in which the incissors are large and the molars small, but so much of the hinder margin of the maxillary bone as is preserved only demonstrates imperfectly a cutting margin, with doubtful indications of immature teeth buried in the substance of the bone.

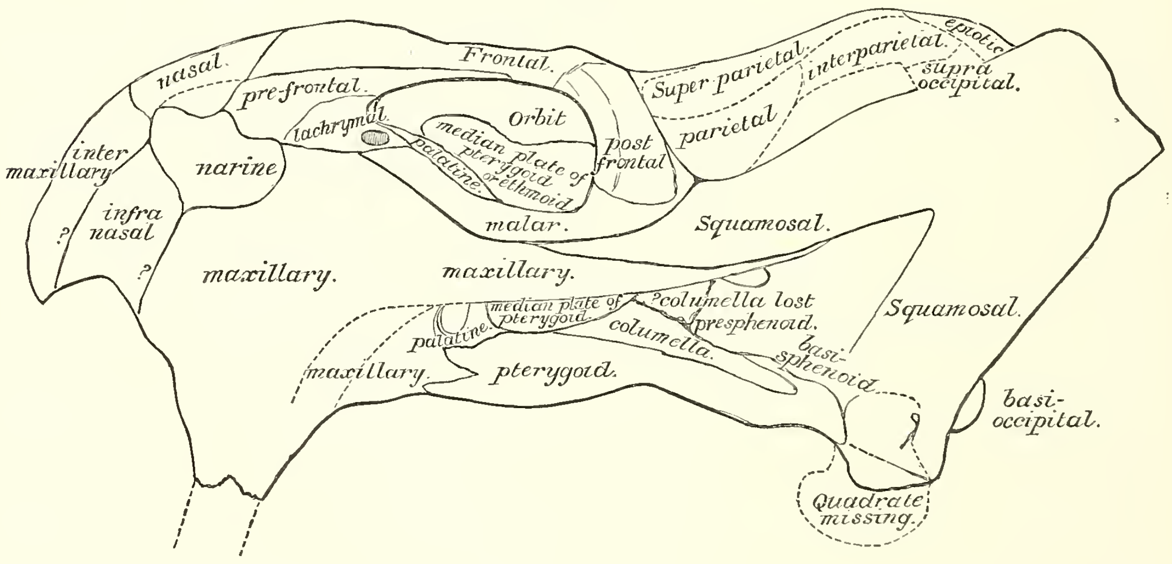
The palate in front is formed by two bones which meet in the median line by a close median suture at the bottom of a slight median concavity. These bones curve convexly upward in front, are in contact with the maxillary bones at the sides, and at their hinder outer corners meet the pterygoid bones, so that they appear to demonstrate the palatine as a transversely ovate plate consisting of two lateral portions. The palatine processes of the pterygoid bones are slender plates of almost rod-like form, which converge inward as they extend backward, but they descend to a lower level, apparently, than the maxillary bones. The exact mode of their union with the mass of the pterygoid bones is not evident. But the pterygoid bones unite in the median line, where they are unusually elongated, and inclined towards each other so as to meet inferiorly in a median keel. On the right side a process is given off which extends transversely outward and upward to the malar region of the base of the orbit, though the malar bone itself is clearly defined. This pterygoid process is therefore in the position of the transverse bone. The sphenoidal region of the palate ascends, and at the fracture is concave from side to side and shows the rounded form of the base of the brain-cavity. The inter-parietal bone is seen to extend obliquely over the cerebellar region in the usual way.

There is a general resemblance between this type of skull and *Ælurosaurus*, which shows that this genus belongs to the Gennetotherioid division of the Anomodont order; but Pig-like ridges on the snout and other features sufficiently distinguish it from Theriodont genera, and the palate is distinctive.

Summary of the Structure of the Dicynodont Skull.

The same plan of structure is found in all the skulls of Dicynodonts which I have been able to compare, although the proportions of the different parts of the head vary in the genera. The post-orbital region may be greatly elongated, as in *Dicynodon leoniceps*, when the parietal area is usually an angular crest, or the transverse expansion may be considerable, and the parietal region flattened and tabular, as in *Dicynodon tigriceps*. The nares may approach near to the extremity of the snout, as in some species of *Oudenodon*, or the pre-maxillaries may have a great anterior development, giving the nares a backward position, as in *Ptychognathus*. But, although the relative size and shape of every bone become modified in harmony with these modes of growth, the plan on which they are arranged never varies, so far as I have been able to ascertain. This plan consists in a solid jaw, from which a vertical longitudinal median plate is prolonged backward, where it divaricates to contain the small brain. This wedge is terminated posteriorly by a more or less vertical occipital plate. Inferiorly the back of the squamosal region is connected with the jaws by longitudinally extended slender bars, which form the palate. Laterally the squamosal extends towards the maxillary, forming a single lateral arch behind the orbit. Superiorly the anterior part of the head is more or less flat, and horizontally extended parallel to the palate.

Fig. 2.



Lateral plan: Skull of *Dicynodon*.

The occipital plate may either be transversely extended and vertical, or may have its lateral halves directed outward and backward. It includes a rounded occipital condyle, which is formed by the basi-occipital and the two ex-occipitals, which are usually developed backward somewhat further. The condyle is less prominent than in Chelonians. Above it is the foramen magnum, more or less extended vertically, with sub-parallel sides. The occipital plate consists of the usual four occipital elements, though the sutures between them may become obliterated. They are at first well defined, but much closer than the sutures which connect the occipital plate with the bones which occur above it and at its sides. The supra-occipital forms the upper part of the foramen magnum, the ex-occipitals make the lower parts of its sides, and the basi-occipital is below. The sutures between this bone and the ex-occipitals run down the descending processes which have been termed hypapophyses.

A large bone is situate above the supra-occipital, and extends the occipital plate vertically. It has an immense anterior extension, may apparently be single or double, enters into the brain-case, and I identify it as the inter-parietal, and regard it as homologous with the bone so named in Mammals and Lizards.

At the sides of the occipital plate are the large squamosals, elongated vertically and expanded laterally. Their connection is mainly with the ex-occipital bones. It is their reflection backward which forms the basin-shaped occipital surface found in *Dicynodon pardiceps* and other types. It is this bone which furnishes the Mammal-like zygomatic process from its anterior border, and it supports the quadrate bone on its base.

Two other bones are found at the back of the head, which form a pair, and lie between the inter-parietal, supra-occipital, and squamosal. They are, apparently, thin plates, which correspond with the similarly placed bones in Labyrinthodonts, which have been termed epi-otic bones.

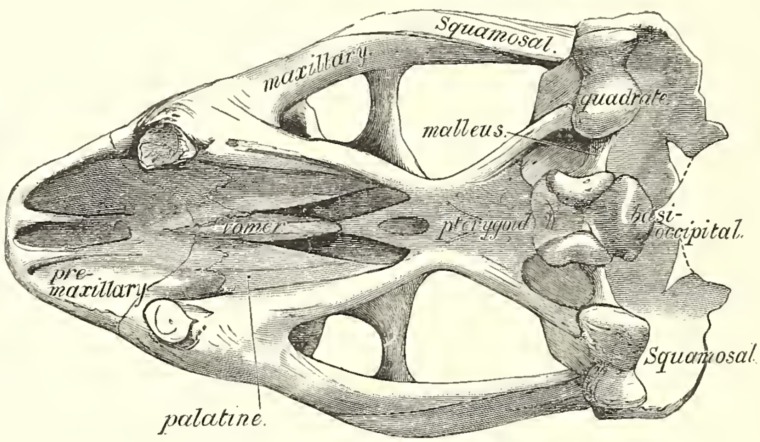
Between the ex-occipital and the squamosal is a large foramen, which may be auditory ; and at the sides of the ex-occipital elements of the condyle are foramina, which may be outlets for the vagus nerve.

There is no certain evidence of the basi-occipital being divisible by suture from the basi-sphenoid, but such a separation is probable (although a vertical section fails to show it), because the hypapophyses which are prolonged downward from the region of these bones sometimes show at their termination a tri-radiate groove, and it has been seen that the basi-occipital and ex-occipital contribute the posterior two of these three elements.

These sphenoido-occipital processes form, at their outer lateral termination on each side of the head, a remarkable crescentic concave articulation. A small bone articulates with it and extends to the squamosal, near to the quadrate or to the quadrate bone. The bone may be very small and sub-quadrate in form, or sub-cylindrical and constricted a little in the middle, with convex articular ends. These bones are much smaller than the pear-shaped bones in *Ichthyosaurus*, which have a large lateral attachment at the junction of the basi-occipital and basi-sphenoid, and which extend transversely outward, so that the small end is received into a pit in the quadrate bone, and they are much less slender than the bones which in some Liassic

Ornithosaurs extend from the sphenoid to the distal end of the quadrate, and less slender than the relatively long bones in the Bird's skull, which extend from the anterior inner angle of the quadrate articulation, and converge forward and inward to the sphenoid, which have been regarded as pterygoid bones. Nothing like these bones of Anomodonts has been recognized in existing Reptiles; and they are regarded as homologues of the malleus of the Mammalia on account of their relation to the surrounding bones.

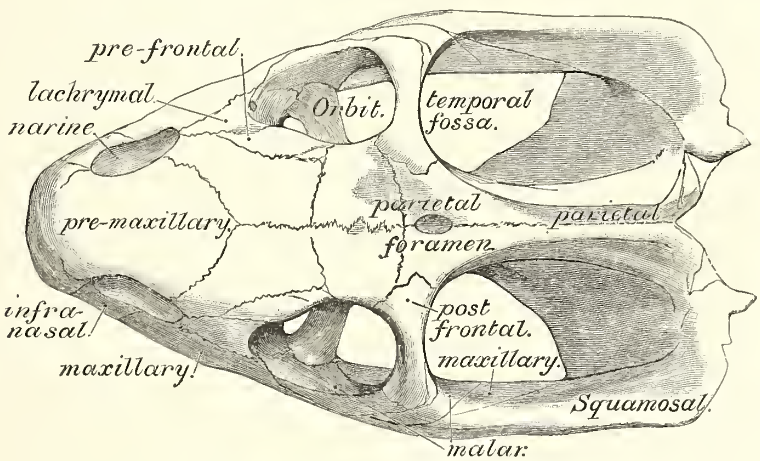
Fig. 3.



Plan of palate of *Dicynodon*.

The cavity which contained the brain is small, narrow, and high. It appears to have the basi-occipital and basi-sphenoid for its floor. The pre-sphenoid ascends obliquely in front, and is very narrow, and there is no certain evidence whether the olfactory nerve was prolonged above it. The superior covering of the brain was formed by the supra-occipital, inter-parietal, and parietal bones; there is no evidence that the brain extended forward to the region of the frontal. It has been seen that the ex-occipital bones enter into the foramen magnum, but on the anterior side the occipital plate thickens, and the thickening appears to be due to a bone, which loses its individuality at an early period, being super-imposed. I regard this bone as the opisth-otic. Its anterior margin is smooth, and formed the hinder wall of a vertical aperture in the brain-case through which a large nerve passed. It corresponds to the outlet for the fifth nerve, but other nerves also, probably, passed out in the same channel. Anterior to this aperture is a large vertical plate, which rises from the basi-sphenoid; and this I regard as the ali-sphenoid bone. It is very thin, and may not always have been ossified, but is well seen in *Ptychognathus*. Much further forward, above the pre-sphenoid, are the bones which appear to correspond to the orbito-sphenoid, placed at the back of the orbit, and perforated. Thus, the sides of the brain-case converge as they extend forward, till they merge in the vertical median septum which may be formed at its base by a flat median process of the pterygoid, on which is placed the pre-sphenoid, similarly compressed from side to side and elongated; above this bone succeed vertical plates of the parietal and frontal bones; and anteriorly these elements are prolonged by the ethmo-vomerine plates described by Professor HUXLEY.

Fig. 4.



Plan of the Upper Surface of the Skull of *Dicynodon*.

The palate in Dicynodonts is characterized by being formed mainly by the pterygoid bones. They are large horizontally extended plates, which meet in the median line and rest upon the basi-sphenoid, much as in Chelonians, but three processes appear to be given off, of which Chelonians show no evidence. First, a process is directed outward and backward to the quadrate bone, and is separated from the mass of the bone behind it by a notch. Secondly, from the anterior corner a long bar of bone is produced forward and outward to meet the maxillary bone behind the great tooth; and so as to make the external margin of the pterygoid concave. Between these anterior bars a pair of smaller processes is given off, which soon converge as they extend forward, and are developed into a vertical median plate which underlaps the pterygoid, and extends forward to meet the vomer, but soon rises so as to disappear from the horizontal plane of the palate. The palatine bones extend along the whole length of the anterior bars of the pterygoids. They are narrow, splint-like bones at first, on the inner sides of the maxillary bars of the pterygoid, and widen as they extend forward into the maxillary, where they converge toward the median line, but are commonly separated from each other by the vomer. A process from the palatine is directed upward and outward, and abuts against the lower border of the orbit, internal to the malar and the lachrymal.

The anterior part of the jaw is formed in Dicynodonts by the maxillary and pre-maxillary bones. The pre-maxillary elements appear to be always comparatively large, and apparently single, though they may be as small in Theriodonts as in any Mammal and divided in the usual way. In some types the suture between the pre-maxillary and maxillary bones appears to be overlapped throughout its length by the sub-narial bone, but in other species this bone is only seen on the floor of the external narine.

The apertures of the skull present nothing remarkable in the ways in which they are defined. The temporal vacuities may be extended transversely, or much extended longitudinally, and are always limited externally by a single zygomatic bar, into which the squamosal always enters, which may be underlapped by the maxillary in Dicynodonts, and is underlapped by the malar in Theriodonts. The orbit is circular or ovate, surrounded by the post-frontal, frontal, pre-frontal, lachrymal, and malar bones. The nares are usually anterior and divided, but their relative position in the head is influenced by the development anteriorly of the pre-maxillary bone. The nasal and frontal bones are double. The pre-frontal is distinct from the lachrymal. The latter bone is below the pre-frontal and above the maxillary; it is always perforated by the lachrymal canal, and extends from the orbit to the narine, as in *Ichthyosaurus*. The post-frontal forms a transverse bar at the back of the orbit, extending from the frontal to the malar, which rests upon the squamosal, and makes the lower border of the orbit. At the hinder margin of the frontal bones is the parietal, which shows no sign of median division, and contains the parietal foramen in its anterior part. External to it are plate-like bones, which margin the superior borders of the temporal fossæ. They appear to be distinct from the parietal, and to overlap that bone anteriorly and the inter-parietal posteriorly, for the inter-parietal succeeds the parietal as a single median roof-bone, which may sometimes be double. But, while it is probable that the post-parietal plates are anteriorly separate from the parietal, there is no specimen which establishes the separation beyond question.

The squamosal bone, by its varied development, greatly modifies the form of the skull. It always gives off a strong laterally compressed zygomatic process, and in Dicynodonts is more or less extended inferiorly and vertically below that process, where it forms an arch into which the vertical part of the comparatively small quadrate bone is received. This inferior process of the squamosal may become small, and in the Theriodont *Galesaurus* has no existence. The squamosal bone always forms the external lateral limit of the occipital area along its extent.

The Vertebral Column.

The only part of the vertebral column in which the number of vertebræ is imperfectly known is the dorsal region. A specimen in the British Museum, of which a portion has been figured by Sir R. Owen ('Cat. South African Reptilia,' Plate 52), indicates not fewer than fifteen vertebræ contained between the head of the humerus and the head of the femur, and in *Deuterosaurus* the number of dorsal vertebræ preserved is eleven, so that the vertebræ in the several regions of the body may be stated with some probability at seven or eight cervical, twelve or thirteen dorsal, one to six sacral, and about twenty caudal.

The forms of the vertebræ appear to differ a little in the several types, but they all show a remarkable approximation to Sauropterygian genera. There are the same biconcave articular surfaces to the centrum, only the mode of ossification of the intervertebral or proto-vertebral substance appears to be different, for the unossified part has a tendency in Anomodonts to contract to a tubular-conical form, while in Plesiosaurs and Nothosaurs the tendency is for the base of the cone to disappear, so that the conical excavation becomes shallower, till the articular surface is perfectly flat, and the cavity becomes obliterated.

The positions of the articulating surfaces for the ribs are more like that in Plesiosaurs than in other animals, since there is a double attachment in the cervical region, a single attachment in the dorsal region, which is entirely on the neural arch (except in the Pareiasauria), and a single caudal attachment, which descends again to the centrum. But, since the cervical diapophysis in Anomodonts is formed by the neural arch, the affinity is obviously closer with the Crocodilia, especially the Teleosauria, and in that group chevron bones are equally well developed ; so that, notwithstanding the general resemblance of these vertebræ to those of Plesiosaurs, the divergence in the cervical region is as absolute as in the sacral region. Yet the sacral vertebræ are quite unlike those of Crocodiles ; and the affinities indicated by these resemblances may be no more important than the affinities with *Hatteria*.

The Vertebræ of Dicynodonts. (Plate 12, figs. 2, 4.)

No specimen has hitherto been described which shows the actual association of the vertebræ attributed to *Dicynodon* with a skull ; for, although there is a strong probability that the vertebræ and limb-bones from the Gonzia River, attributed to *Dicynodon tigriceps*,* are parts of the same animal as the skull from the same locality, the specimens are separated. It is, therefore, interesting that the skull fragment already described in this memoir (Brit. Mus., R. 866) has the earlier cervical

* 'Catalogue South African Reptilia,' No. 66, p. 40, Plate 35.

vertebræ preserved in natural articulation in the basin formed by the expanded squamosal bones.

There is no reason to doubt that in this species the occipital condyle was small, and as short as in allied types, though it is not exposed. Attached to it, apparently, is a bony mass projecting backward, which is embedded in matrix, except at the base, and on part of the right side; and this I regard as being probably the anchylosed atlas and axis; not that there is any visible evidence of union between the bones, but the antero-posterior extent of the ossification is 2·3 centims., while the succeeding cervical centrums have an average length of 1·7 centim., and I know of no animal in which the atlas is longer than the succeeding vertebræ, while a similar increased length is found when atlas and axis are anchylosed. Its posterior articular face is less than 2 centims. deep. At its base is a large tubercle, which may be lateral, and is probably one of a pair; though the other, if present, is hidden by matrix. The height from this tubercle to the summit of the neural arch is 4 centims. The neural arch, which is imperfectly exposed, extends backward for a centimetre behind the articular face of the centrum. The succeeding vertebræ are dislocated, and turned round at a right angle. The matrix has been removed so as to expose both the left side and the base.

The first of the free vertebræ has many of the characters of an inter-centrum, for it has no neural arch, but the centrum has been partly chiselled away on the under side. Its antero-posterior extent, as preserved, is 1·7 centim., and, therefore, as long as the vertebræ which follow it. On its left upper anterior angle is a large ovate articular facet, slightly elevated, with a sharp border; it looks forward and outward; and I can only suppose that the neural arch of the axis may have rested upon it. The surface beneath it is deeply excavated. The neural arch of the following vertebra is in close contact with the superior part of its posterior border, and the neural spine of that vertebra extends forward, so that it must have been in contact with the neural spine of the axis. This affords, so far as I am aware, the first evidence of an inter-centrum among true Reptilia developed to the size of an ordinary centrum, as in *Diplovertebron.* There are no interspaces between the vertebræ; their neural arches interlock slightly, and the centrums are in close contact.

The next vertebra has the centrum 1·7 or 1·8 centim. long, flattened on the under side in front, but forming a median ridge behind. At the sides of this flattened part of the base is a pair of tubercles, moderately elevated, placed obliquely so as to look backward and upward; that on the right side is 6 millims. long. The side of the centrum is flattened, but slightly concave, inclined a little outward. It has no elevated anterior border, as though it were the vertebra to which the inter-centrum belongs. The vertical depth of the side of the centrum below the neuro-central suture is about 1·5 centim. The neural arch is large. It extends transversely beyond the centrum, and develops a strong diapophysis directed backward and outward. This process is fractured, and only its base is seen, convex above and flattened below; but it was probably as long as in succeeding vertebræ, where the measurement from the pre-zygapophysis to the articular diapophysial facet is 2·5 centims., and the process projects freely for more than a centimetre. The surface of the neural arch is flattened and inclined concavely. Anteriorly, at a height of 2·5 centims., there is a slight, horizontal, zygapophysial facet, which probably had no function in the skeleton. The posterior zygapophysis is strong, and is developed backward in the usual way. The front to back measurement over these facets is 2·5 centims.

The neural spine expands above into a hatchet shape, which is defined by a small concave notch above the pre-zygapophysis, and a larger concavity above the post-zygapophysis. Its superior border is concave from front to back, and 3 centims. long. It is fractured posteriorly, but is seen to have been a thick wedge of bone terminating in a point. Anteriorly it forms a blunt convexity more than a centimetre in transverse measurement. It makes some approximation in form to the neural arch of the fourth cervical of *Protorosaurus Speneri*,* though it differs in having the posterior process superior in position to the post-zygapophysis. From the base of the centrum to the summit of the neural spine is 4 centims., of which the neural arch measures 2·7 centims.

In the three succeeding vertebræ the characters are modified. The centrum is slightly shorter. The base of the fifth centrum shows a longitudinal median ridge, which is less elevated on the sixth and absent on the seventh, which is convex from side to side. The parapophysial tubercles are more anterior in position, being just behind the anterior articulation, but quite as low on the side of the centrum. The transverse measurement over them is 2·2 centims. Behind these eminences the centrum is concavely compressed. The pre-zygapophyses are strong, directed forward and upward, convex externally, with the facet directed inward and forward. Behind its rounded anterior margin, which is about 1 centim. wide, the process contracts a little. A blunt rounded ridge, which is concave in length, connects the zygapophyses. The neural spines incline a little forward as they extend upward ; they are about 1 centim. wide.

The posterior face of the last centrum shows a conical excavation, like the vertebra of an osseous Fish. The articular face is 2·1 centims. high and about 2·5 centims. wide.

The ribs remain in close contact with the vertebræ. Their articular ends fork, so that with the processes from the vertebra they each enclose a sub-rhomboid space. The forks are of equal length, and diverge like the forks of a Y, but the tubercular process is the wider and the more compressed. The ribs are sub-cylindrical, curved, and half a centimetre wide ; that attached to the fifth vertebra is preserved for the length of three centrums.

Dorsal Vertebræ. (Plate 16, fig. 1.)

A small slab from South Africa in the British Museum shows on the right side evidence of seven dorsal vertebræ, and on the left side portions of seven dorsal ribs.

* 'Phil. Trans.', Vol. 178, p. 208.

The locality from which the specimen was obtained is unknown, but the bones correspond in size with the cervical vertebræ already described. Three vertebræ in the middle of the series are fairly well preserved.

The centrum is 1·7 centim. long, very slightly concave from back to front, with the articular margins moderately elevated. From side to side the external surface is well rounded, and the transverse diameter is 2 centims.; towards the neuro-central suture the diameter contracts to about half as much in the middle. The vertical depth of the articular face exceeds 1·5 centim.; it is conically excavated.

The neural arch is defined from the centrum by a transverse suture. It is high, rises along the whole length of the centrum, extends obliquely outward, so that this concavity forms a continued depression with the upper part of the centrum. Both anterior and posterior surfaces rise steeply, so as to form at the interlocking of two neural arches a circular foramen for the inter-vertebral nerve, which is given off high above the centrum, on a level with the transverse processes, so that its summit is fully 1·6 centim. above the neuro-central suture, and its diameter about 6 millims. The anterior border is concave between the base and the middle of the pre-zygapophysis, which does not extend in advance of the centrum, and has its facet looking inward. The superior, inferior, anterior, and posterior sides of the lateral part of the neural arch converge outward in a pyramid, and the angularities between them are lost in the rounded transverse process, which extends outward and a little backward for 1·5 centim. beyond the pre-zygapophysis, and terminates in a flattened facet for the single-headed rib.

The neural spine is inclined backward in position, so as to extend above the inter-vertebral foramen. It is prolonged upward and a little backward; is 1 centim. wide from front to back, though the measurement may be slightly more towards the free end, where it is imperfect; but its height from the inter-vertebral foramen exceeds 3 centims. The spine is compressed from side to side, and convex from front to back, so that the anterior and posterior margins are sharp. The post-zygapophyses apparently have an articular facet posteriorly, as well as laterally; the former is immedidiately above the middle of the transverse process.

The fragments of ribs on the other side of the slab are about 12 centims. long, curved, more than half a centimetre in diameter, and cylindrical.

In the large slab already referred to, as indicating the extent of the dorsal region, there are only seven dorsal ribs exposed; and I infer, from the absence of ribs in the lumbar region, that three or four vertebræ anterior to the sacrum may in some specimens have been without ribs, though other larger fossils appear to have the ribs extending to the sacrum.

Caudal Vertebræ of Platypodosaurus. (Plate 17.)

The only known example of the tail of an Anomodont is the specimen described by Sir Richard Owen* as a mass of matrix, including part of the sacrum and pelvis, with ten anterior caudal vertebræ, probably a species of *Dicynodon*. It is from Fort Beaufort, a locality which yielded the skulls of *Dicynodon Bainii*, *D. pardiceps*, and *D. feliceps*, and it may not improbably be referred to one of these species. Unfortunately, the vertebræ have been separated from the sacrum and pelvic bones. The original description of the vertebræ is brief, and the figure unsatisfactory. The following notes contribute to a knowledge of this region of the vertebral column.

The specimen includes eleven vertebræ, and measures 31 centims. in length. The vertebræ progressively become smaller and shorter. All the earlier caudals have strong transverse processes, which are directed outward and very slightly downward, but the processes, which are separate ossifications, disappear from the later vertebræ. Where the transverse processes begin to decline, strong short chevron bones begin to be developed. These bones are massive, as though they supported the weight of the tail, and are in close contact with each other, but rapidly diminish in size, as though the tail only included about five more vertebræ; though the total number may have amounted to twenty.

In the first four vertebræ, each centrum is 3 centims. long. In the next three, each centrum is 2·6 centims. long, the eighth and ninth are each 2 centims., the tenth 1·9 centim., and the eleventh about 1·7 centim. long.

With this shortening in length, the whole aspect of the vertebræ gradually changes. At first the centrum is evenly convex from side to side below the transverse processes, and somewhat markedly concave from front to back. The margins of the centrum are sharp. Its depth is 6·3 centims., and its transverse width about 5·7 centims., and the transverse measurement below the bases of the transverse processes is 4 centims.

At the base of the fifth centrum, on the posterior border are two strong tubercles with a transverse measurement of nearly 3 centims., and a strong groove between them. On the sixth centrum the tubercle on the right side is elongated into a process directed downward and inward; on the left side the process is lost, being separated by suture from a large rounded boss. This is the beginning of the chevron bones. The chevron bones in the seventh and succeeding vertebræ are similar to each other. Each is a V-shaped bone which appears to articulate by two facets on the posterior face of the centrum. The transverse width over these processes is at first about 3·8 centims., and in the last vertebra preserved it is reduced to about 2·6 centims. In lateral view the processes are directed downward and backward; below the attachment their sides converge downward with a slight lateral concavity, and the anterior and posterior borders are strongly concave, so that the extremities of the bones are expanded from front to back. The length of the chevron bones is

* 'Cat. Foss. Rept. of South Africa,' 1876, p. 73.

in the first four about 3·5 centims.; the fifth is imperfectly preserved, but probably shorter. The antero-posterior extent of the basal expansion of each bone is about 2·4 centims., convex from front to back. It is also convex transversely, and wider behind than in front; the posterior transverse measurement in the second bone is 1·5 centim., the corresponding anterior measurement is about 1 centim., but in the later bones the surface appears to be becoming a rounded boss.

The neural arch is comparatively small, and is rapidly reduced in size in the later vertebræ. The strong pre-zygapophysial processes extend upward and forward as far as the middle of the centrum of the preceding vertebra; they are convex externally, with the articular facet vertical, and looking inward, so as to receive the post-zygapophysial wedge between them. This wedge in the earlier caudals is about 1·8 centim. wide; the bone above it rises into a neural spine, which is broken away, except for an indication that its moderately convex sides converged forward into a sharp anterior ridge which inclines backward as it ascends. The transverse width over the pre-zygapophyses in the earlier caudals was about 4 centims.

As the rounded pre-zygapophysial ridge descends it becomes constricted a little from side to side, and deeply concave on its anterior margin, to form the inter-vertebral foramen, and less concave on its posterior border, and then is directed outward, to form the base for the transverse process. This process is here a separate ossification or caudal rib which is attached high upon the side of the vertebra, partly on the neural arch and partly on the centrum; the depth of this attachment appears to increase to the fourth and fifth caudal vertebræ, where it exceeds 3 centims. The third and fourth processes are between 5 and 6 centims. long. The superior surface of the process is convex from front to back, and narrows as it extends downward and outward to the compressed free end, which is 1·5 centim. wide, and less than half a centimetre thick. Its anterior and posterior margins are sharp, with these ridges placed superiorly, so that the inferior surface is the more convex. In the later vertebræ the transverse processes appear to be inclined a little backward; they obviously have small basal attachments, but appear to have been lost from the last five vertebræ. The centrum was deeply concave in the early vertebræ, but there is an appearance like that seen in the vertebræ referred by Owen to *D. tigriceps* (Plate 16, fig. 2), only less marked, as though the substance of the notochord were in process of ossification, a condition which I regard as showing that relative depth or flatness of the articular face of the centrum can have no value as a generic character (Plate 16, fig. 3). The articular face of the last centrum preserved is only slightly concave, with a moderate central depression.

The neural canal is small, but it does not appear to decrease in size as it extends backward.

All record of the sacrum with which the specimen was associated had been lost. But, at my request, Mr. A. Smith Woodward, F.G.S., made inquiries from which it results that Mr. Barlow, the "mason" who developed the specimens, indentifies the sacrum and pelvis referred by Sir RICHARD OWEN to *Platypodosaurus robustus* as the missing fossil. I have attempted to fit the specimens together; but the caudal vertebræ are larger than might have been expected from the size of the sacrum, and a vertebra or two must be missing if the specimens are parts of one animal. On this point there appears to be no doubt, since Mr. WILLIAM DAVIES, F.G.S., who superintended the development, states that he remembers the association of this tail with the remains which were subsequently referred to *Platypodosaurus*. The generic difference of this pelvis from the forms which have been attributed to *Dicynodon* is obvious; but it is not improbable that a skull already referred to *Dicynodon* may be associated with the remains.

The Scapular Arch.

In all Anomodonts the scapular arch probably includes the same elements, which are:—an inter-clavicle, clavicle, scapula, pre-coracoid, and coracoid. In some types there is a sternum also, but there is no reason for supposing that this bone is always ossified.

In mode of grouping and arrangement of the bones there is a close resemblance to the Monotremata, the only group in which the pre-coracoid is similarly distinct. But in *Procolophon* it will be shown that there is a sutural union between pre-coracoid and coracoid; not unlike that which has been demonstrated in other Anomodonts, and this leads me to believe that the ordinary Reptilian type of coracoid, which is perforated in many types exactly as is the pre-coracoid, is probably the result of the obliteration of that suture, so that the coracoid in Reptiles may be held, especially when perforated, to comprise both bones; and, therefore, the persistence of the pre-coracoid suture in Anomodonts may rather indicate a line of descent than a direct affinity; and, judging from absence of the suture in some Amphibians, like *Cryptobranchus*, in which this part of the skeleton may be unossified, I am not disposed to regard division of this element into pre-coracoid and coracoid as conclusive against Reptilian affinity, or as showing affinity with Monotremes, which might at first have been surmised. The difference from both Reptile and Mammal is, however, of an ordinal kind; and, so far, the Anomodont characters help to show that only one more ordinal type is required to complete the gradation between these classes. The united pre-coracoid and coracoid in *Procolophon* make a bone elongated in the antero-posterior direction, which may be compared in length and form with the coracoid of Plesiosaurs and certain Ichthyosaurs, and among existing Reptiles with *Hatteria*.

On the Anomodont Scapula.

The scapula appears to be more variable than any other bone in the Anomodont skeleton. In *Keirognathus cordylus* I have described an elongated slender type, like the bone in *Kistecephalus*. The specimen No. 36,272 is imperfect at its junction with the coracoid (Plate 15, fig. 4), but shows a distinct constriction or neck, external to the articulation, because a moderate acromion process is developed on the anterior margin;

and the bone widens in a wedge-shape as it curves backward and inward. The external surface is concave from side to side, and the internal surface is convex in the same direction. The posterior margin is thickened, and the anterior margin compressed, and both these margins are concave in length. The free end of the bone appears to be convex from side to side.

The bone figured by Sir R. OWEN as the scapula of *Dicynodon leoniceps* ('South African Catalogue,' Plate 70, fig. 1) is essentially of the same type, but with the wedge-like blade relatively more expanded towards its extremity. The posterior border appears to be thickened, and the anterior margin is compressed and thin; but the acromion is not seen, probably because only the internal aspect of the bone is exposed, and the proximal part of the anterior border is invested in matrix. The internal or visceral aspect is bow-shaped in length, and the strong concavity may be supposed to correspond with the external curvature of the ribs. The bone is 28 centims. long. Its proximal end is thickened, and 11 centims. wide. It is divided by a short deep notch or groove into two articular parts, a posterior portion about 7 centims. wide, which gave attachment to the coracoid bone, which is not preserved, and an anterior part over 4 centims. wide, which articulated with the pre-coracoid, a bone represented in Sir R. OWEN's figure, but described as coracoid. It is stated that the coracoid exemplifies the broad and short type with the large "axillary" perforation (p. 35). But, although this specimen is described as the articular end of the right scapula with "the coracoid," it seems to me that the entire length of the scapula is preserved, though the proximal anterior corner of the bone is broken away. The pre-coracoid is imperfect, and too imperfectly preserved to give any idea of its shape. The anterior margin is very thin. Opposite the perforating notch in the scapula is a corresponding large U-shaped notch in the pre-coracoid, which appears to be what is commonly termed the coracoid foramen. It is about 2·5 centims. deep and 2 centims. wide.

Another specimen, No. 36,272, I regard as the left scapula and pre-coracoid, both very imperfect and exposing the external surface of both bones. The scapula is only preserved as far as the acromion process, which curves forward and downward, so making a flattened transverse surface for the clavicle to rest upon. It encloses a space between the clavicle and pre-coracoid. The scapular articulation is greatly thickened, semi-circular, transverse to the external surface; and the internal surface for the coracoid widens the bone into a large sub-triangular mass. There is no external indication of division between the pre-coracoid and coracoid surfaces of the bone, except that the pre-coracoid plate is obviously compressed and thin. The clavicle is sigmoid, expanded at the ends, which are at right angles to each other.

Coracoid Bones of an Anomodont. (Plate 15, figs. 5, 6.)

A specimen numbered 36,286 consists of a pair of coracoids. On the ventral surface, stretching in a line at right angles to the anterior margin of the left bone, are four dorsal vertebræ, a good deal flattened and distorted, which extend, as preserved, over a space of about 8 centims., but they lie about half a centimetre apart, each centrum being 1·5 centim. long.

I presume that the pre-coracoid joined the straight suture on the anterior margin; that the inter-clavicle joined the internal margin, which is exceedingly thin; and the thicker posterior margin joined the sternum. The bone has a thickened ovate external articular area, 3 centims. deep and 2 centims. wide, which gave attachment to the scapula by a large surface, and contributed with it to form the glenoid articulation for the humerus. In form it may be regarded as a segment of a circle, with the convex border facing toward the inter-clavicle, and the humeral articulation forming the narrow border toward the centre of the circle. The greatest transverse width of the bone is about 6·3 centims. The greatest antero-posterior extent is 7 centims. The internal border is regularly convex from front to back; it is thin, not more than 4 millims. thick in the middle, but becomes about a centimetre thick at the posterior extremity. This thickening is seen on both sides of the bone, and it helps to define the concave visceral surface of the internal aspect, as well as a slight concavity on the posterior part of the external aspect, besides giving rise to a flattened oblique posterior area a centimetre wide, which looks backward, outward, and upward. The extreme measurement from the posterior extremity of the bone to the humeral surface (in a straight line) is 5·5 centims., but the posterior outline of the bone is concave, forming a wide arch behind the articular surface for the humerus, with the contour straightening as it extends backward, and ultimately rounding convexly on to the internal margin of the bone.

The nearly straight anterior margin of the bone is less than 4 centims. long.

Immediately behind the articular surface is the pre-coracoid notch, about half a centimetre wide and nearly as deep, which contributed the posterior border to a foramen, part of the contour of which was formed by the pre-coracoid, and part by the scapula. The straight anterior sutural border is narrow, transverse to the axis of the bone, concave on the visceral surface, convex externally, 7 millims. thick. At this notch the extent of the bone from front to back is about 3 centims., and at the articular surface it measures a few millimetres more. The anterior and superior part of the surface gave attachment to the scapula, while the ovate postero-inferior surface was for the humerus.

The Left Pubic Bone of Titanosuchus ferox. (Plate 16, fig. 4.)

The forms of pubis among Anomodonts vary chiefly in the position and direction of the perforating foramen and extent of union with the ischium. Sir R. OWEN has catalogued one type* as "the coalesced humeral ends of the right scapula and coracoid of *Pareiasaurus bombidens*" from Vers Fontein; and has given the same osteological interpretation to a similar specimen referred to *Dicynodon leoniceps*, from Graaff-Reinet. In these specimens the obturator foramen is narrow and extended transversely; its direction is inward and upward.

Another type is seen in the specimen described in the same catalogue as the left os innominatum of *Dicynodon leoniceps*, Plate XXVIII. Here the obturator foramen of the pubis has a more anterior position, and is directed obliquely forward so as to emerge on the inner side of the anterior margin of the pubic bone (Plate XXVIII., fig. 2).

The bone now to be described (Brit. Mus. 49,367) approaches in general character to the latter type, but is larger, is pierced by the foramen for a greater antero-posterior distance, while the anterior opening of the foramen is hidden by a sharp anterior border to the bone. The narrow transverse median articular surface, about 2·5 centims. thick, by which the pubes met in the median line, would indicate that the position of the bone was ventral and horizontal, and nearly at right angles to the ilium.

The bone is greatly thickened at the acetabular end, where it unites with the ilium; but it is otherwise a moderately thin plate, slightly concave between the acetabulum and the median suture, measures 20 centims. wide, and is convex from front to back anterior to the obturator foramen. The anterior margin is strongly concave, but the concavity, which is 7 centims. deep proximally, becomes narrower distally, and twisted inward so as to merge in the visceral surface, which is saddle-shaped at the anterior end, and otherwise flattened, so that the two opposite sides converged downward in a broad V-shape.

The articular end, which is sub-triangular, with the short side in front, is about 20 centims. long, by 12 centims. deep. It is divided by a longitudinal angle into an inferior acetabular part, about 7 centims. deep, and a superior iliac surface of similar depth, which was longer.

The posterior or ischiac border of the bone is fractured, so that it is not possible to judge of the exact size of this transversely sub-ovate bone; but the anterior extent of the ischio-pubic symphysis is a distinction from the Dicynodont types, while the relatively small size of the pre-pubic tuberosity is a distinction from *Phocosaurus*. That tuberosity may be indicative of affinity; for, if it covered the whole anterior border of the bone, the Anomodont pubis would be more easily compared with that

* 'South African Catalogue,' p. 11, p. 35.

of a Plesiosaur, though I have as yet seen no Sauropterygian in which the obturator foramen passes through the pubis, or in which there is an extended sutural union between the pubis and ischium.

The Limbs of Eurycarpus Oweni (Seeley). (Plate 18.)

The only evidence of the relative size of the limbs as compared with the vertebræ, in addition to the specimen of *Keirognathus cordylus*,* is afforded by a sandstone slab 47 centims. long, which is a natural mould including the neural arches of the anterior part of a vertebral column with a number of dorsal ribs. It appears to show the femur, and the whole of the fore-limb. Isolated parts of this slab, showing the fore-limb and those vertebræ which have ribs attached, have already been figured by Sir R. Owen in the 'South African Catalogue,' Plate 52, pp. 53, 54, regarded as Dicynodont. When the digits from this specimen were reproduced in 1880, the figure was described as (?) *Dicynodon* ('Geol. Soc. Quart. Journ.,' vol. 36, Plate 17, fig. 5, p. 424). I am unable to detect evidence which would prove the animal to be Dicynodont, for the only data for comparison are the imperfectly displayed humerus and dorsal vertebræ; and in neither are the characters such as have been found in *Dicynodon* or its known allies. The humerus appears to have much in common with that of *Euchirosaurus*, but is of a different generic type, and may conform to the Anomodont plan. The vertebræ are too imperfectly exposed to show the relation of the centrum to the neural arch; and, although the neural spines are very short, and the transverse process short and stout, there is no vertebral character to show that the animal was not an Anomodont. An impression from the slab gives a deceptive appearance of lozenge-shaped dermal armour, which results from fracture of the stone. Although armour is characteristic of Labyrinthodonts, there can be no reason why it might not be present in an Anomodont. Some of the bones of the skeleton were incrusted with a concretionary film, and this has adhered to the ulna and radius, and part of the femur, and to a large sub-triangular bone anterior to the humerus and parallel to the cervical vertebræ, so that the casts of those bones are not sharp. The latter sub-triangular bone appears to resemble the inter-clavicle of *Pareiasaurus* in form, though its position is that of a scapula; and it is too imperfectly exposed to be determined with certainty. It is about 10 centims. long, and 4 centims. wide in the middle, with each of the three sides concave. Taken in association with the other characters, the bone may well be an inter-clavicle, and the fossil would probably be a Pareiasaurian. Partly overlapping the humerus are fragments of bones of the shoulder girdle, but too imperfect for determination.

The neural arches of six dorsal vertebræ extend over 10 centims. The neural spines, which are very imperfectly preserved, appear to have been short in the dorsal region, and longer and more compressed from side to side in the neck. In a few vertebræ,

* 'Phil. Trans.,' B, 1888.

between the neck and the back, metapophyses appear to be developed. The zygapophyses are strong, prominent, directed upward and outward; they are separated by a deep notch. Below this notch is a strong, short, massive transverse process which is rounded superiorly, and to these processes I suppose the ribs to have been attached, though they are now displaced to a lower level. There is no indication of the base of the centrum, but, as the vertebral column is nearly straight, and the vertebræ vertical, they may be presumed to have the base flattened. The seven dorsal ribs are each about 14 centims. long, rather slender, curved, contracting at the abdominal end to about half the diameter, rounded on the under side, flattened above with a slight ridge on the posterior margin, which makes the side of the rib slightly concave. The last rib but one shows, apparently, the expanded articular end. It is concave from above downward so as to overlap the rounded transverse process, and the superior process of the rib appears to be perforated by a foramen. The depth of the articular end is 7 mm.

The distal end of the femur apparently, with its (?) proximal end seen at one extremity of the slab, would show the bone to be 11 centims. long. The shaft expands towards the distal end, where it is about 3·5 centims. wide, convex transversely on the superior side, with the articulation moderately rounded downward and backward, though the distal end is imperfectly exposed. The left humerus is short, about 5·5 centims. long, and broadly expanded at both proximal and distal ends, which are nearly at right angles to each other. The proximal end is about 4·5 centims. wide, with the articular surface extended transversely as in most Anomodonts, and a small but prominent rounded tuberosity in the middle of the superior margin. The radial margin of the proximal end is produced outward as a thin process more proximal in position than is usual. At 2 centims. below the proximal articulation, and well above the middle of the bone, is a large foramen on the radial side in the cast, about 7 mm. in diameter, but it is caused by a boss of phosphatic matter which adheres to the slab. The distal expansion of the humerus begins at a little more than 1 centim. below the proximal articulation, and it evidently developed a strong process on the external side, which is broken away, for the fractured surface is nearly 3 centims. long and about half as wide. The distal end was evidently compressed laterally on the Anomodont plan, but the articular surface is imperfectly exposed.

The radius and ulna cross each other, so that the proximal end of the ulna is thrown behind the radius, which thus becomes partly exposed at its proximal end. The radius is the stronger bone, 2·3 centims. wide at the articulation, which is transversely truncated. Its dimensions become smaller as it extends distally.

The ulna is 8·2 centims. long. It appears to have a slight sigmoid flexure, inclining a little forward at the larger proximal end, which is fully 2 centims. wide, and slightly backward at the distal end, which is about 12 millims. wide. The external surface is somewhat flattened. There is a doubtful indication of an olecranon ossification. The distal end is rounded. The bone is much more slender than the large Dicynodont bones already described, and there is no evidence whether its proximal end received the radius in the same way in a groove as in the bones presently to be described.

The hand is folded back so as to expose the inferior aspect of part of the carpus, the meta-carpus, and the five digits. The width at the carpus is fully 4 centims., and the length of the meta-carpus and phalanges is 5 centims. The entire length of the fore-limb is about 18 centims.

The distal row of the carpus appears to include four bones.

The meta-carpal bones become more elongated from the first to the fourth, and the fifth is only a little shorter. At the same time the middle of the shaft, which at first is scarcely defined, gradually becomes slender, though the terminal ends, and especially the distal ends, do not become narrower. The first meta-carpal is 9 millims. long, and nearly quadrate; the fourth is 3 centims. long. In the first digit there are two phalanges, in the others three phalanges. These bones are short and broad, well ossified, with well defined articular ends and lateral constrictions, but the terminal conical claw phalange is relatively large, and in the middle digit, in which it is longest, measures 1·6 centim., and in every digit it is longer than any other phalange. The claws curve a little downward, are rather flattened on the under side in front, and compressed behind.

The specimen may be named *Eurycarpus Oweni*.

Femur of Titanosuchus ferox (Owen). (Plate 19.)

The limb bones which are marked in the British Museum Register as associated with the skull fragments named by Sir R. Owen *Titanosuchus ferox*, comprise, besides smaller pieces, a femur and a humerus. The remains of the femur have been put together by the British Museum masons with great skill, and I have no doubt their restoration exhibits accurately the complete form of an Anomodont femur more perfect than that attributed to *Dicynodon leoniceps*. The specimen is numbered 49,368. It is a straight stout bone, flattened in the vertical or antero-posterior direction, and only moderately expanded at the extremities, as compared with the humeri of *Dicynodon*. Its extreme length from the articular head to the outer condyle is 61·5 centims. The shaft is most constricted in its lower third, and the proximal end is more expanded transversely than the distal end, and twisted a little inward so as to be inclined to it at a slight angle.

The inferior aspect of the bone is flattened, but for the development of the condyles at the distal end. The transverse width over the condyles exceeds 20 centims. The rounded convexities of the inferior surface are worn away. The inner or tibial condyle had a vertical extent of 14 centims., and a transverse width in the middle of about 8 centims. The fibular condyle is developed so as to extend 2·5 centims. distally beyond the other condyle. The posterior surface of this condyle has a vertical measurement at 11 centims., and a transverse measurement of about 9 centims. The inter-condylar space is 3·5 centims. wide, and extends as a shallow concave channel between the distal parts of the condyles; it rounds convexly on to the broad shallow saddle of the middle of the distal articular surface. The posterior surface of bone between the upper parts of the condyles is a shallow concavity, and the surface, which becomes flatter as it extends proximally, is slightly concave in length, but gives to the eye the aspect of being flattened. It is about 30 centims. long, and 9 centims. wide in the middle, and widens a little proximally and distally. Its external border is the external margin of the bone, which is concave in length, and forms a sharp ridge proximally, and is more rounded distally towards the condyle. Its internal border is a slight ridge which descends from the inner margin of the obturator pit concavely towards the outer border of the tibial condyle, without quite reaching it; so that it appears to divide the proximal half of the hinder surface of the bone into an external area, which is slightly concave, and an internal area, which is strongly convex, rounding on to the superior or anterior surface. The thickness of the bone through the condyles is about 12·5 centims.; while the thickness between the condyles is 10 centims. On the anterior aspect the inner condyle has a sub-globate form, well rounded; while the external condyle is laterally compressed so as to rise into a blunt ridge, which is prolonged for a third of the length of the shaft, and then subsides into a slight ridge which extends outward towards the proximal trochanteric extremity of the bone. This ridge defines the external surface of the shaft, which looks outward and a little upward, widens distally, is concave in length, and somewhat convex in the direction of thickness of the shaft. The narrowness of the proximal part of the lateral area is coincident with vertical compression of the proximal part of the shaft.

On the internal side of the distal end of the shaft there is a blunt longitudinal ridge, which makes the superior front part of the area concave, and the inferior posterior part longitudinally convex. This ridge, as it descends, is inclined backward at an angle to the axis of the shaft. The proximal part of the shaft widens on both the inner and outer borders; but the articular head is inclined, as usual, upward and inward, forming a large sub-hemispherical convexity, which inflates the superior anterior aspect of the bone, making it convex in the transverse direction, and concave in length, but flattened, or even a little concave, on the outer side. Seen from the proximal extremity, the articular head is sub-reniform, being rather concave behind. It is worn, but appears to be 20 centims. wide, and 10 centims. from front to back, so that it is somewhat compressed from front to back, and flattened posteriorly. It becomes narrower as it extends outward, and retreats in a convex curve. The region of the external angle or great trochanter is broken away, but the compression of the bone does not suggest the presence of any strong trochanteroid process.

The obturator pit is a shallow depression which is imperfectly excavated, which lies external to the median longitudinal ridge already described on the posterior aspect of the bone, and which dies away at about 14 centims. from the proximal extremity of the bone. The pit is limited inferiorly by a slightly elevated ridge with a concave border, 17 centims. from the proximal extremity, and not less than 6 centims. wide. There are some indications of a second, but slightly impressed, muscular attachment extending distally below this ridge.

The differences of this femur from the corresponding bone in the giant Salamanders of Japan are that in the living type the proximal half of the bone is rotated upward at right angles to the distal end and that the condyles are not ossified; but the result is that the trochanteroid processes on the under side of the proximal end similarly define a shallow pit, comparable to that of the obturator pit in the fossil. The ridge which ascends from the internal condyle to the trochanter may be identical in both types, so that, allowing for the difference in ossification, there is more similarity than might have been expected between the two types. In *Hatteria* the bone is less compressed in the shaft, and has a much greater development of the infero-anterior proximal trochanter. Sir R. OWEN has already drawn attention to the Monotreme characters in the Anomodont femur.

Humerus of Titanosuchus ferox (OWEN). (Plate 20.)

The humerus is a strong bone approximating to the ordinary Dicynodont type, with both proximal and distal ends greatly expanded, but more nearly in the same plane than in smaller animals. It is 53 centims. long, nearly 30 centims. wide over the distal condyles, and slightly wider over the proximal end. The lateral outlines are concave, so that the transverse measurement over the middle of the shaft is reduced to 13 centims. The specimen exhibits a remarkable development of the distal condyles on the inferior aspect of the bone, which is unparalleled among other Reptilia, the vertical extension of the condyles in the middle of the shaft measuring about 18 centims., or fully a third of the length of the bone. On this postero-inferior aspect the outline of the condyles is sub-triangular, with the angles rounded and the superior border convex from side to side, and rising sharply from the shaft. The convexity of the condyle in this region is absent, and the proximal part of the surface, on which a film of matrix remains, appears to be flat. The thickness through the shaft in this region is about 14 centims.; and the measurement is scarcely less through the globate radial condyle. The ulnar condyle is comparatively compressed, and not more than 7 centims. thick, but well rounded on the distal surface; in the transverse direction a moderately wide and shallow concavity divides the smaller ulnar region from the large radial region. On the superior aspect of the bone this concavity is developed as a shallow triangular area, about 15 centims. broad and as high, which is defined towards the radial side by a blunt ridge. The area is gently concave; the surface external to the ridge is about 10 centims. wide and flattened; the corresponding surface of the distal part of the shaft on the ulnar side is rounded convexly towards the lateral margin. Both lateral margins are sharp for some 10 centims. above the condyles, but the ridge is narrower, sharper, and more convex in its lateral outline on the radial side. Above the radial condyle on the inferior aspect the shaft is oblique, concave from above downward, and becomes somewhat concave transversely, distally, as it approaches the condyle. On this area, at 18 centims. from the distal extremity of the bone, 5 or 6 centims. above the sharp proximal termination of the condyle, and 3·5 centims. from the radial margin, is the supra-condylar foramen. It is about 1·3 centim. wide, and descends obliquely downward. A wide notch on the margin of the condyle towards the lateral ridge may have carried the vessel which issued from this foramen. The foramen is situate substantially as in *Cynodraco*, *Brithopus*, and *Hatteria*.

On the ulnar side, the distal part of the shaft is similarly flattened, concave from within outward, with a deep oblique groove facing laterally, defined by a ridge of bone extending over the depression. At 22 centims. from the distal end, and about 3·5 centims. from the lateral margin, is a foramen which appears to be of the same size as the supra-condylar foramen on the radial side. Distally, by the side of the sharp lateral margin, above the condyle, is a moderate longitudinal groove, which may have carried the vessel issuing from the foramen. This foramen is present in Dicynodonts and is found in *Hatteria*.

The proximal end of the bone is only preserved on the inferior surface. It shows the articular head to be well rounded and directed inward, and defined from the radial side of the bone by a deep concavity which extends to the middle of the shaft. The extreme measurement from the head of the bone to the ulnar condyle is 45 centims. The radial crest is prolonged proximally far beyond the head, so that the measurement from the proximal border to the extremity of the radial condyle is 54 centims. In the transverse direction, the space between the head of the bone and the radial crest is concave. The proximal extremity of the radial crest is about 14 centims. in transverse width, convex from within outward, and well rounded on the posterior border. It is about 6·5 centims. thick, and is reflected downward and forward, widening the bone, down which it extends for more than half its length, measuring about 33 centims. in length. It appears to become narrower as it extends distally, but the distal development is imperfectly preserved. Its external or anterior surface is gently convex in the vertical direction, smooth, and has an unbroken concave contour from the rounded summit of the crest to the condyle at the distal end, where the concavity is more marked, and also developed transversely.

The humerus shows some general approximation of plan to that of the giant Salamanders in the expansion of the extremities, the thickening of the distal end of the shaft, the superior concavity between the condyles, and the development of the radial crest.

In *Hatteria* the resemblance of the contour of the humerus to that of an Anomodont is so close as to amount almost to identity of plan, the chief differences being that in *Hatteria* the radial crest is much less developed and that the extremities of the bone are less massive.

Fibula of Titanosuchus ferox. (Plate 21.)

Right fibula.—A bone which was broken in two and has been restored exhibits a flattened hour-glass-like form, with oblique articular ends, a concave internal contour which shows a flattened ovate surface proximally, presumably for contact with the tibia, though a similar surface exists on the proximal end of the radius in No. 36,259, named *Dicynodon tigriceps* (OWEN.) The external contour is so much less concave as to be almost straight. The extreme length of the bone is about 29 centims., so that it was less than half as long as the femur. This is exactly the proportion in the giant Salamanders of Japan, and establishes the Amphibian proportions of the limbs in *Titanosuchus ferox.*

The proximal end is 12·5 centims. broad, and may have been as thick as the distal end, but the external surface of the proximal end is broken away. It appears to have been transversely ovate and convexly rounded. The inferior surface of the shaft is flattened transversely, but still is a little convex, while it is markedly concave in length. The transverse measurement in the middle of the shaft is less than 9 centims., and the thickness is there reduced to 5·5 centims.; but, distally, the bone expands in both dimensions. Its transverse width at the distal surface, which is imperfect towards the tibia, does not appear to have exceeded 12 centims., while its thickness may have been 11 centims. Its outline is sub-ovate. The articular surface is flattened in the antero-posterior direction, and oblique and convex from the tibial margin downward and outward. There is some appearance of the surface towards the tibia being inclined more obliquely inward, as though it had helped to support a tarsal bone which was lodged between the tibia and fibula. The distal end is greatly thickened on the inner side of the shaft. All the surfaces of the shaft are well rounded in the transverse direction, and moderately concave in the vertical direction.

The Ulna. (Plates 22 and 23.)

Besides the ulna, No. 43,525, catalogued by Sir R. OWEN as the right ulna of *Pareiasaurus bombidens,* there are two smaller specimens in the British Museum, registered respectively as No. 36,249 and No. 49,389. Taken in connection with the other specimens, these isolated examples demonstrate the nature and development of the epiphyses, and prove these elements of the bone to have been quite as large and as well developed in Anomodonts as in Urodeles, as far as external contour is concerned, for there is no trace of the epiphysial element having penetrated the shaft. Proximally, the fully ossified ulna is prolonged in a massive olecranon process, and this ossification included the whole of the concave articular surface. In the specimen 49,389, which was forwarded by Mr. BAIN with many bones which were referred to *Dicynodon tigriceps,* the surface is seen from which the epiphysis has come away; and it proves to be convex, and so even that the extent of the epiphysis it carried could not have been suspected without the evidence from the specimen 43,525. The distal epiphysis was quite as singular, for it comes away, leaving a concave surface on the shaft (Plate 23, figs. 2, 3), which has a sharp margin on the superior surface, and allows the epiphysis to extend for some distance proximally on what may be termed the inferior aspect of the shaft. In their well ossified character and large size, transverse separation from the shaft, and union with it in the adult, these epiphyses are unparalleled among Reptilia, and in all these respects are comparable to the similar ossifications in the long bones of Mammals. From the differences of proportion and form, the specimens 36,249 and 49,389 may be regarded as different species of the same genus, and provisionally referred to *Dicynodon*; but No. 43,525 differs in ways which may well be generic.

The specimen is 32 centims. long, and is a massive bone which terminates proximally in an olecranon process (Plate 22, fig. 1), which is larger than the expanded distal end, and helps to form the large obliquely concave articular surface for the humerus, which extends forward so as to widen the proximal end of the bone to about 22 centims., while the width of the distal end is 13 centims., and the least width of the shaft, at 10 centims. from the distal end, is 9·5 centims.

The internal aspect of the bone is comparatively flat, being slightly concave in length and slightly convex transversely, with the proximal and distal borders slightly elevated. The impression on the upper part of the bone is the result of crushing.

The anterior border between the proximal and distal articulations is 17 centims. long, straight in the middle, and becomes curved forward at each end towards the proximal and distal articular surface.

The posterior contour of the shaft is concave at the distal end, but diverges backward as it extends proximally, and becomes a convex curve, which is continued on to the proximal surface of the olecranon.

The bone is compressed from side to side, but a rounded ridge extends longitudinally down the middle of the external aspect of the bone, commencing at the anterior external corner of the proximal articular surface, and running distally and a little inward, widening as it goes, so that distally it only makes the bone transversely convex, while proximally it divides the bone into two lateral portions, which meet each other at an angle. The posterior of these surfaces is about 26 centims. long, 11 centims. wide proximally, and 7 centims. wide distally; flattened, but slightly concave in length, and at the proximal end slightly concave transversely. A narrow posterior area separates this lateral surface from the internal surface, towards which it approximates as it extends outward; it is 5·5 centims. wide proximally, where it passes on to the proximal cartilaginous surface of the olecranon, but becomes narrower distally, and in the middle of the length the limiting angles of this surface, which looks obliquely outward, have disappeared, and the bone is transversely rounded. Distally, it is a groove margined by sharp short tubercles or ridges, defining a channel about 3 centims. wide. In *Dicynodon* this posterior area does not appear to exist.

The antero-external lateral area is much shorter, since it is limited proximally by the tranverse articulation. It is about 10 centims. wide, and deeply concave proximally, forming an excavation for the head of the radius ; but distally its width is reduced to one-half, and the bone, which is flattened transversely in the middle of the shaft, becomes convex transversely above the distal articulation. This surface meets the internal border of the bone in a sharp ridge.

The distal articulation (Plate 22, fig. 3) has a pear-shaped contour, narrow behind, and wide in front. It is oblique, extending distally several centimetres further on the inferior than on the superior border. Its extreme width is 13 centims., and extreme thickness 9 centims. It is convex from back to front, the convexity increasing anteriorly and inferiorly ; but the outer part of the bone is somewhat concave in the transverse direction.

The proximal extremity of the olecranon (Plate 22, fig. 2) is sub-quadrate, being 11 centims. wide by 9 centims. thick at the articulation, and 6 centims. thick at the posterior border. It is defined by four straight borders, is convex superiorly in both dimensions, and its rugose surface gives every appearance of being cartilaginous.

The proximal articulation is imperfectly freed from the matrix, but it consists of an internal part 14 centims. long, and an external part about 10 centims. long, so that internally the bone extends forward as an angular process 6 centims. wide, where it merges in the mass of the articulation. The inner part of the articulation extends further proximally than its external part, and on the middle of the posterior half of this surface is a strong sharp-rounded ridge, which was received into a corresponding groove on the distal end of the humerus.

There is something very Mammalian in the character of this proximal articular surface ; but it is perhaps the character of all others in which the Anomodont type approximates to a Dinosaur.

Small Bones of the Extremities of Titanosuchus ferox. (Plate 24, figs. 1, 2.)

Two small bones, numbered 49,367, were collected with the other remains of *Titanosuchus ferox* (Owen). Their forms are such that they may be phalanges, or carpal or tarsal bones, for in contour they resemble carpal bones of some Plesiosaurians, but are much thinner than might have been expected from the massive character of the larger limb bones. I regard the larger of the two bones as probably an external phalange, and the smaller as a middle phalange.

The larger bone is compressed, sub-quadrate, but broader than long, with concave lateral margins, long and narrow proximal and distal articular surfaces, which somewhat approximate towards one side of the bone. The external or superior surface is concave transversely, and gently concave in the vertical direction ; the inferior surface is flat.

What I suppose to be the proximal surface is 6·5 centims. long, transverse to the axis of the bone, and inclined obliquely in the transverse direction, so that the length of the bone towards what may be the external side is 6·7 centims., and towards the other side about 5 centims. This articular surface is smooth, flattened, slightly convex in transverse width, and about 2·5 centims. thick in the middle, becoming narrower towards each side.

The distal articular surface is 7·3 centims. wide, and more than 3 centims. thick. It is convexly rounded in the vertical direction as well as at its external corners, but so that the larger part of the articular surface lies towards what I regard as the inferior aspect. An impressed groove appears to margin the superior limit of the articulation, but the extremities of the bone are slightly worn or weathered. The external lateral border of the bone is a sharp ridge formed by the superior surface curving down concavely, so as to depress the inferior surface. The internal lateral border is relatively thick, and traversed by a longitudinal groove. This groove, like a small notch on the external side, I suppose to be for ligaments connecting the phalange, which moved on the sub-cylindrical convexity of the distal surface.

The second phalange is smaller. It also is compressed and sub-quadrate, with concave sides, and the proximal and distal surfaces expanded. The proximal surface is oblong, 5·8 centims. long, 2·6 centims. thick, with the ends rounded. It is transverse to the length, but irregular, so that it is convex transversely towards one side, and inclines to be concave in the same direction towards the other side. The extreme length of the bone is 5·7 centims. The superior surface is still covered with matrix, but was concave vertically, and slightly convex transversely. The inferior surface was similar, but flatter transversely. The distal end is thickened, having a depth of at least 3·5 centims., with the superior and inferior margins straight, and nearly parallel. It is 5 centims. wide. It is strongly convex transversely, with lateral impressed grooves towards the upper part of the articulation for ligamentous union with the adjacent phalange. The inferior surface of the articulation is oblique and flattened and extends proximally, so as to give the sub-trochlear facet a length of 2·5 centims. The superior border of the articulation is broken away. The sides of the bone are thickened, and appear to be vertically rounded.

The Tibia. (Plate 25.)

A specimen from JAN WILLEM's farm, registered as No. 43,525, determined by Sir R. OWEN as the right tibia, was referred to *Pareiasaurus bombidens.* The only other specimens from this locality are certain vertebræ, determined as dorsal and caudal ('South African Catalogue,' pp. 10, 11); unless the fossils described as *Tapinocephalus Atherstoni* (OWEN) from a locality "four miles from JAN WILLEM's Fontein," should be part of the same series. But, from the care with which localities are recorded, it is doubtful if there is any warrant for associating this bone with the remains of the skull of *Tapinocephalus.*

The right tibia is a short, massive bone, which shows a small lateral surface at the proximal end for contact with the fibula. Its extreme length from the anterior proximal process to the end of the internal distal talon is about 30 centims.; the least measurement between the proximal and distal articular surfaces at the posterior external angle is 15 or 16 centims.

The proximal surface is sub-triangular, with the posterior and external borders straight, each about 13 centims. long, and meeting at a right angle; with the external anterior border convex and rounding on to the adjacent sides. The transverse oblique measurement over this part of the proximal surface is nearly 20 centims. The articular surface is flattened, but crossed from front to back by a blunt ridge, at about 7 or 8 centims. from the outer border, which corresponds to the groove between the condyles of the femur, and makes a division of the proximal articular surface, which inclines the lateral surfaces to each other in a way seen in Mammals, with a slight anterior eminence extending between them.

The smaller external condylar surface may have been supplemented by the fibula. There is some indication of the large convexity which extends anterior to the articular surfaces being formed by a separate ossification, in which case it might correspond to the patella, but the indication is so obscure that no weight can be attached to it. The anterior proximal border is rounded, and the posterior border is sharp and prominent.

The shaft contracts so that its least measurements are below the middle, where the transverse width is 8 to 8·5 centims. The sides preserve their individuality fairly well; and they expand distally to form the remarkable distal articulation, which has a transverse sub-ovate contour, but develops an anterior process on the middle of the anterior margin. The inner half of the surface is an oblong convexity which is prolonged distally for 3 or 4 centims. below the flattened external half of the articulation. The transverse measurement of the distal articulation is about 15 centims., and the antero-posterior measurement about 13 centims. On the anterior border, a distinct notch in the middle reduces the antero-posterior measurement of the flattened part of the articulation to about 10 centims. Thus, a large process, comparable to the talon of the Mammalian tibia is well defined, and, taken in connection with the character of the proximal end, gives a Mammalian character to the tibia which is unparalleled among Reptiles, and is more remarkable than the Mammalian character of the femur.

There is no evidence of the genus to which this bone should be referred. It may be new or it may be *Pareiasaurus* or *Tapinocephalus*.

Procolophon trigoniceps (Owen). (Plate 9, figs. 7, 8, 9.)

No specimen has hitherto given an adequate idea of the structure of the skull in *Procolophon* or evidence of its systematic position; and, if I am able to improve upon previous knowledge, it is because the skill of Mr. Richard Hall, Mason in the

British Museum, in relieving Dr. EXTON's specimen from the matrix, has shown the characters of the fossil in a way which leaves nothing to be desired.

Procolophon differs widely from Dicynodontia, Gennetotheria, and Theriodontia in the structure of the skull, for it possesses no proper temporal fossæ. It approximates towards the Pareiasauria in features such as the expansion of the parietals roofing in the back of the skull and the elongation of the roof bones of the head; and is remarkable for the large size of the epi-otic and quadrato-jugal bones. But, on the other hand, the shoulder girdle is dissimilar. There is a large parietal foramen. The palate is very dissimilar in construction to that of a Dicynodont, and, apparently, unlike *Pareiasaurus* in details; so that the genus becomes the type of a new group, which is, in some respects, intermediate between the Pareiasauria and Dicynodontia, and cannot be placed in either sub-order. It is the type of the Procolophonia.

The skull is sub-triangular, 4·7 centims. long, with the transverse posterior outline straight, and measuring 3·5 centims. from one epi-otic horn to another. Anterior to these small posterior angles, the postero-lateral contour is a concave notch, owing to the extension outward of the squamosal and quadrato-jugal bone. This post-quadrate concavity is about half a circle, and its curve extends forward to a line with the back of the orbit, or the middle of the parietal foramen. The great lateral expansion anterior to this notch is made by the quadrato-jugal bone; and these bones widen the back of the skull to upwards of 5 centims., for the median measurement to the one side on which the preservation is perfect is 2·7 centims. Then the lateral contours converge forward, with a moderate constriction towards the front of the orbit, terminating in a blunt snout about 1·1 centim. wide below, but the anterior extremity containing the nares is lost.

Superiorly there is a slight bevelling toward the occipital surface, a horizontal flattening of the parietal and frontal region, a rounding of the nasal and pre-frontal area, and an oblique extension outward and downward of the bones below the orbit.

The circular parietal foramen is 0·4 millim. in diameter, and 9 millims. in advance of the back of the skull. The orbits are longitudinally ovate vacuities, 2 centims. long; 1·4 centim. wide in front, between the frontal and malar, but narrower behind. The least width of the inter-orbital space is rather less than 1 centim. The bones which surround the orbit are the malar, post-frontal, parietal, (?) supra-orbital, pre-frontal, and lachrymal.

The bones on the two sides of the head are not absolutely identical, partly owing to slight differences of form, and partly from differences of preservation.

The parietal bones are large and irregularly subquadrate, with a transverse angular bend separating the somewhat narrow, posterior inclined, occipital area from the flat, transversely extended, parietal area, which includes the parietal foramen, by narrow processes which extend convexly forward between the frontals to enclose it. The median suture between the part of the parietals behind the foramen is a zigzag of one angular bend to the right, followed by a similar angular bend to the left. The transverse width of each bone posteriorly is 1 centim. to the position where it is overlapped on the posterior corner by the epi-otic bone, but it becomes wider anteriorly, since it extends into the posterior corner of the orbital vacuity, where the width is half as much again. The extreme antero-posterior measurement in the median line is 1·4 centim.

There appears to be a small ossification between the posterior angle of these bones on the inclined occipital surface which may be the inter-parietal. Laterally, the parietal meets three bones, of which the most posterior is (1) the epi-otic, which rests upon the parietal; and (2) the squamosal, which is in contact with (3) the post-orbital. Anteriorly, the parietal is in contact with the frontal and post-frontal bones, and there is no evidence of division in the part of the parietal in front of the foramen.

The occipital surface of the skull is not seen.

The frontal bones are a pair of flat oblong bones, almost as long as the orbito-temporal vacuity, divaricating a little posteriorly, and narrower in front. The median suture between them is undulating, 1·7 centim. long. The lateral branches of the bones extend outward and backward above the orbits, about as far as the middle of the parietal foramen; their posterior contour is concave, and the transverse width over the posterior angles is 1·4 centim. The extreme length of the frontal is 2·1 centims. The width of the bones diminishes anteriorly, by the wavy external borders converging to half a centimetre. The width in the narrow space where their borders enter into the orbits is 9 millims.

External to the posterior border of the frontal and partly overlapping the parietal, is a long narrow bone, 1·1 centim. long, pointed in front, and widening to 2 millims. posteriorly, which I regard as the post-frontal. There is no indication that it has any other relations than with those two bones.

Extending along the anterior border of the frontal is a sub-triangular bone, which widens as it extends forward, which is the pre-frontal. It is 1 centim. long, extends as far forward as the frontal, and is 7 or 8 millims. wide, wedged between the frontal and lachrymal, and meeting the nasal. In front of the frontal and pre-frontal are the nasal bones, which, owing to the state of preservation, are imperfectly defined. The suture is seen, by which they unite laterally with the maxillary bones, and there are indications of the median suture, so that they cover the superior convex pre-orbital area. Each is more than 1 centim. wide posteriorly and (as the extremity of the snout is lost) fully 1·5 centim. long.

The post-orbital arch commences with the large sub-triangular bone whose extremity forms on each side the lateral posterior angle of the skull. The surface of the bone is convex from front to back, and its angular extremity is inclined downward, outward, and backward. It rests by squamous overlap upon the posterior border of the squamosal and the external surface of the parietal. Its relative size is comparable with the same bone in Labyrinthodonts, and is much greater than in *Pareiasaurus*, and altogether more conspicuous than in Dicynodonts, which usually have the bone chiefly on the occipital plate. It is about 8 millims. in length and width, with the external margin slightly concave, and the inner border slightly convex.

The bone which is anterior to the epi-otic is in contact with the external process of the parietal, and is hence named the squamosal. It is oblong, and inclined obliquely, downward, forward, and outward. Its inferior margin helps to define the posterior post-quadrate concavity of the skull. It is 1 centim. long and less than half as wide. Its superior anterior border helps to give attachment to a slender bone which extends from the external angle of the parietal to the malar, and forms the postero-inferior border of the orbit. That bone is about 1·2 centim. long and 2 millims. wide. Its inferior border is concave and helps to enclose a small oblong vacuity between it and the squamosal behind and the malar below. This vacuity might correspond with the position of the bone which I term supra-quadrate—the supra-temporal of OWEN and some authors. Below the squamosal is the quadrate, which has already been shown in other species to be compressed from front to back. It descends as a vertical pedicle, which lies below the hinder part of the orbit, and has its chief extension below and behind the malar. It is well seen on the inner side of the jaw, where it sends a strong process inward and upward to the pterygoid bone, and its rounded distal articular end is exposed externally by fracture on the left side; but it is otherwise completely hidden from view by the enormous quadrato-jugal bone, which is imperfect on the left side from being broken away. It is perfectly preserved on the right side, where it extends downward, outward, and backward, terminating posteriorly in a tuberosity which terminates the semi-circular contour of the post-orbital excavation. The form of the bone is obliquely sub-quadrate, 1·4 centim. high, and about 1 centim. wide, with the posterior border concave, and the anterior border convex. It is convex from above downward, where the lower extremity is reflected inward, but does not descend to the articulation so as completely to hide the quadrate. It is remarkable that the quadrato-jugal has a considerable extension behind the quadrate bone.

The malar bone, which forms the inferior border of the orbit, is an irregular crescentic bone, which is concavely constricted in the middle, and is in contact with the maxillary bone below in front, where it tapers away to meet the lachrymal at the anterior corner of the orbit. On the corresponding posterior surfaces it meets the quadrato-jugal below and the post-orbital behind. It contributes with the maxillary in front and the quadrato-jugal behind to define a concave and somewhat angular excavation of the contour of the head below the orbit. The malar extends behind the maxillary; its length is 1·7 centim.; its depth at the middle constriction is 1 or 2 millims.

The maxillary bone, as preserved, measures 2·1 centims. on the right side, but appears to be shorter on the left. The surface of the bone is concave from front to back, and convex from above downward, where it is 8 millims. deep, with a superior convex contour. It extends back somewhat beyond the alveolar border; and above the alveolar border it contains one or two comparatively large vascular foramina. There are six teeth in each maxillary bone. They differ in no way from similar teeth already described. Anteriorly they are 3 millims. long, posteriorly are a little shorter, are cylindrical, terminate in conical points, and have slightly expanded bases, which are in close union with the jaw. The interspaces between them are about a third the diameter of the teeth. They contain sub-cylindrical cavities. The summits, or crowns, of the lower jaw fit into the interspaces between the conical summits of the crowns of the maxillary region, and their form is not defined. The pre-maxillary bones are imperfectly preserved. They were short, as in other specimens, presumably divided the nares, were divided medially, and each contained three teeth, which were rather longer than those in the maxillary bone, and extend in front of the corresponding teeth of the lower jaw. The shortness of the pre-maxillaries gives a truncated appearance to the snout, since their teeth extend transversely.

The lower jaw is in natural articulation with the skull. The rami are loosely connected, 4·1 centims. long, and diverge backward, so that the transverse measurement at the quadrate region is equal to the length of a ramus. Each ramus is slightly curved, consequent on a slight convergence inward, to make the rounded narrow symphysial union; there is a slight inflection in the articular region, and a slight bulging outward below the malar region.

The lower jaw is comparatively stout, being half a centimetre thick, and it has a convex appearance on the external surface, and a flattened appearance on the internal surface. It is deepest (1 centim.) below the hinder end of the maxillary, where the dentary terminates in a slight coronoid elevation, in the middle of the length of the jaw, which divides it into an anterior slightly concave tooth-bearing border, and a posterior slightly convex area. From below this point the jaw decreases in depth, both anteriorly and posteriorly, to less than half its depth in the middle.

The jaw has lost some of the external bone substance, but appears to include five bones. First, on the external surface is the dentary, which forms the whole of the anterior tooth-bearing half. Posterior to this there is a long superior bone, extending back almost to the articulation, which I regard as the coronoid. It is divided by a longitudinal suture from an inferior bone, which extends much farther forward than the coronoid, and forms the sharp inferior ridge on the base of the hinder part of the jaw, which I regard as the angular. On the inner side of the jaw two other bones are seen. First, the articular, which is inflected inward to form a process like that seen in Birds and some of the lower Mammals. This bone extends forward so as to cover the hinder half of the jaw, barely reaching its base, for the angular is seen below it; and at its superior termination in front a small bone is seen above it, which I regard as the internal extension of the coronoid. The whole depth of the dentary appears to be covered internally by another bone, which I regard as the opercular. It meets the articular by an oblique suture, which extends downward and backward; and at its inferior termination there is a conspicuous ovate vascular foramen.

There is a better preserved example of a dentary bone of a new species of *Procolophon* in the British Museum, R. 514, from Kl. Vogelstruisfontein, in the district of

Bethulie, presented by Heer H. S. VILJOEN, which is deeper and relatively shorter than other known jaws, and has the anterior tooth or teeth remarkably long.

The Palate.—The base of the skull is occupied by a large oblong median ossification in the position of the basi-sphenoid, which is 13 millims. in length, is traversed by a deep wide median longitudinal channel, is slightly concave from front to back and concave at the sides, so that the width in the anterior part of the bone is half a centimetre and it terminates in a pair of strong prominent tubercles, directed outward and forward, which give attachment to the pterygoid bones. The transverse measurement over them is 9 millims., and there is a saddle-shaped anterior concavity between these tubercles.

The posterior end of the basi-sphenoid is not perfectly exposed, and there is a possibility that it includes in its posterior part a basi-occipital element, but, if so, the suture is obliterated by ossification, as in the skull section figured (Plate 9, fig. 2), and it terminates in a pair of articular processes. At the posterior extremity of the median groove there is a small ossification extending convexly backward, but this is probably a sub-vertebral wedge bone of the atlas, such as is common in Ichthyosaurs, Plesiosaurs, and other Reptiles. From this angle a bone, partly destroyed, which I do not identify, extends outward and upward.

In front of the basi-sphenoid is an indication of a slight pre-sphenoid ossification, extending upward and forward, seen in an almond-shaped vacuity, 7 millims. long and 4 millims. wide, pointed in front and rounded behind, the sides of which are formed by the pterygoid bones.

The pterygoid bones are large ossifications of tri-radiate form. Each sends a flattened process backward and outward to unite with the pterygoid process of the quadrate; this is 3 millims. wide and about 1 centim. long. There is a wide angular vacuity between it and the basi-sphenoid, which opens backward. Another angular vacuity opens laterally between it and the massive transversely extended part of the bone which descends so as to be almost in contact with the coronoid element of the lower jaw. The transverse width posteriorly over each of the processes is about 1 centim., and the antero-posterior extent may be as much. The external part of this process is crossed by a line which may indicate an external ossification which would be the transverse bone. Anteriorly to these processes the bones converge forward, and unite by suture in the median line for a length of about 3 millims. The extreme antero-posterior extent of the bone is about 2·2 centims. The transverse processes are directed strongly downward at their outer and posterior margins. On the rounded border of the almond-shaped pterygo-sphenoid vacuity there is a row of pterygoid teeth, so placed that they diverge backward in a V-shape. The teeth have all been broken away, but I count the basal attachments of seven on each side.

In front of the anterior parts of the pterygoids are a narrow pair of bones which are not completely exposed anteriorly and only seen for a length of 9 millims. The transverse width over both does not exceed 5 millims. They are in contact throughout their length, and extend forward and downward. I have no doubt that they are the vomera. On each of these bones there were two rows of teeth. In each outer row there were about six ; their bases are folded like the teeth of Labyrinthodonts.

At the back of the lateral palatal vacuities between the vomera and pterygoids is a pair of small oblong, obliquely placed ossifications, which I regard as the palatines. They have been injured in excavation, but are 7 millims. long ; their width is not shown.

At the back of the palate there is a pair of rod-shaped bones, constricted in the shaft, truncated at the ends, which are 2 millims. wide, and with the longer 1·2 centim. long, which I have previously regarded as the hyoid. Their anterior ends converge forward, and are 1·5 centim. apart. They are shifted from the median line towards the left side in harmony with the shoulder girdle.

The Shoulder Girdle of Procolophon. (Plate 9, fig. 9.)

The bones of the shoulder girdle are very little disturbed, though the scapula is not exposed and the clavicle is only imperfectly preserved. But the inter-clavicle, pre-coracoid, and coracoid are exceptionally well seen, and situate immediately behind the back of the skull.

The inter-clavicle is the key to the arch. Owing to its anterior convexity, the bone has the form of a pick-axe. It is shaped as in some Labyrinthodonts, as in *Ichthyosaurus*, and many Lizards, but it has the form of a more elongated and slender capital T than has the bone in Monotremes. The transverse bar is about 14 millims. long, and the median bar is nearly 4 centims. long. The anterior margin of the median bar is convex, and reflected a little downward, so as to make the posterior half of the bar appear impressed. The limbs narrow as they extend outward, being one-half as wide at the termination as at their origin. The transverse measurements are 4, 3, and 5 millims. The median staff has sub-parallel sides, being a little contracted in the middle, so that there is a tendency for the distal end to widen, though the widening is much less marked than in Monotremes.

An elevated median ridge runs down the length of the median bar, which is otherwise flat. It unites with the transverse bar at a right angle, though the junction is not notched out, but rounded.

The pre-coracoid and coracoid are in close sutural union with each other, and their antero-posterior length is about 2·6 centims. They are flattened, somewhat oblong, moderately thin plates ; but there is no evidence whether their straight inner margin rested upon the impressed lateral areas of the inter-clavicle, or whether that bone extended in front of them, as seems probable, after the manner of Monotremes and Ichthyosaurs. These bones appear both to contribute to the articulation for the humerus.

The pre-coracoid is not completely exposed on either side, but as shown it appears to be a longitudinally oblong bone 12 millims. long, with the anterior border probably straight and transverse, probably parallel to the transverse straight suture by which it unites with the coracoid. The internal margin was probably straight, as it appears to be in the specimen as exposed, but it is not quite free from matrix. Thus, there would be three straight sides of the oblong meeting each other at right angles. The width of the bone in the middle is not less than the length ; but the external border is notched, not unlike the external border of the coracoid in many Lizards. In the middle of the side there is a strong short process directed outward and backward towards the articulation, into which it may possibly enter. Anterior to this process the bone is concavely excavated on the margin by a notch which extends to its anterior angle. Posterior to this process is an oval foramen or notch obliquely placed, which extends towards the posterior angle near the suture with the coracoid, and appears to be homologous with the coracoid foramen in Dinosaurs and other Reptiles, which may thus become a pre-acetabular notch.

The coracoid is about 1·4 centim. long, and as wide as the pre-coracoid. Its inner side is straight, and its posterior end convexly rounded from within outward. The external border consists of two nearly equal parts : a thickened anterior articular surface which forms part of the glenoid cavity for the humerus, which looks outward and a little forward; and, secondly, a posterior concave area which contracts the width of the bone, but sends a small process outward and upward like that seen at the posterior margin of the bone in some Plesiosaurs. The length of the articulation as formed by the pre-coracoid and coracoid is about 1·4 centim.

No evidence of a scapula is seen ; and the clavicle is imperfect and only seen on the left side, where it extends, if correctly determined, backward and upward from the outer angle of the transverse bar of the inter-clavicle as a thin flat plate of bone 1·6 centim. long, and narrowing from 4 to 2 millims.

Posterior to the shoulder-girdle are some indications of ribs on the right side. They were sub-cylindrical and hollow. There are much smaller sternal ribs, but their relation to the ribs is not clear.

The Fore-limb.—Both humeri remain in natural contact with the bones of the shoulder girdle. The distal ends are missing on both sides. The bone may have been 3·5 centims. long. The proximal end was expanded, with a rounded condyle. The inferior surface was concave transversely, and slightly convex in length. The radial crest did not reach to the proximal end; it was of moderate length, and reflected downward. The ulnar tuberosity was extended backward; it appears to have been quite as large as the radial crest. The transverse width over these proximal processes, as preserved, is 1·6 centim. The diameter of the middle of the shaft hardly exceeds 3 millims.

The radius is a slender bone, 2·3 centims. in length. Its shaft is slightly twisted, a little convexly bent outward, with the ends moderately enlarged, and their articular surfaces convex.

The ulna is a much stouter bone, with its proximal articular surface obliquely truncated. Its extreme length is 2·5 centims. It is 7 millims. wide proximally and narrower distally, but most constricted in the middle of the shaft.

The carpus, as preserved, is somewhat displaced, but shows two sub-quadrate bones below the ulna—one between the ulna and radius, one below the radius. There appear to be one bone of the central series and four bones in the distal row.

I infer that there are five meta-carpals in contact with this carpus. Indications of three strong short bones are seen attached to the radial side. They are constricted in the middle, and each attached to a carpal. The fourth carpal gives attachment to two bones which appear to be more slender, and the fifth is shorter. In the first digit there is certainly one phalange besides the claw phalange, and I believe there is a second, but the state of preservation justifies some doubt on this point.

On Galesaurus. (Plate 9, figs. 3, 4, 5, 6.)

In 1859, Sir RICHARD OWEN described the South African genus *Galesaurus*, which became the type of a division of the Anomodontia, termed Cynodontia. Three skulls and some fragments referable to this genus are preserved in the British Museum, but no other parts of the skeleton have been recorded, so that its position in classification depends entirely upon evidence from the skull, which hitherto has neither been figured with accuracy nor described in detail.

In 1876, the same author instituted the Theriodontia, characterized as having dentition of the Carnivorous type, with incisors, defined and divided from the molars by a large laniariform canine. It was apparently suggested by the resemblances in number of incisors and molars to certain Mammals, rather than by any distinctive Mammalian attribute in the form of the molar teeth; but many genera were comprised in the group which are still imperfectly known. As there is no skull so perfect as that of *Galesaurus*, I believe that, both as the earliest known type and the only type available for comparison, *Galesaurus*, rather than *Lycosaurus*, which the author places first on his list, should be regarded as the representative genus of the group. I make this suggestion because the order was made to include animals which seem to me to have no near alliance with each other, and have a better claim to distinction from the other Anomodontia than either *Lycosaurus* or *Galesaurus*. Thus, *Procolophon* has been shown to be a type as distinct as any South African Reptile that is known. I am led to believe that *Lycosaurus*, *Aleurosaurus*, and the allied genera which have small pointed molar teeth, large canines, and large laterally compressed incisors form a division intermediate between the Dicynodontia and the Theriodontia, supposing that group to be accepted with *Galesaurus* for its type. The data for comparison between *Galesaurus* and *Lycosaurus* are of a slender kind. There can, however, be no doubt that *Galesaurus* is conveniently described as Theriodont, and that in form, proportion, and structure of the skull, it is the most Mammal-like of known Reptiles.

It approaches so nearly to Mammals like the Opossums and some of the larger Bats in form of the skull, that demonstration of the presence of typical Reptilian characters was needed to justify the placing of *Galesaurus* among Reptiles. I regard it as differing from the Dicynodontia, as represented by *Dicynodon*, in sub-ordinal characters; for, although the skull seems at first sight so dissimilar, yet in essential characters there is no structural difference which would constitute an ordinal group. Among the more striking characters by which *Galesaurus* differs from the Dicynodonts are:—

First, the possession of incisor teeth, and, secondly, by the development of cuspidate molar teeth. No Dicynodont is known in which teeth of either kind occur, and, therefore, the character is so far a good one; but, as it only extends to the dentition which was represented in *Dicynodon* by canines, I am unable to regard it as more than a sub-ordinal difference. Yet no existing group of Reptiles shows a sub-ordinal character of the same kind; but among Mammals a total absence of teeth in Ant-eaters only separates them as a sub-order from Armadilloes.

Secondly, the lower jaw has the coronoid process rising above the middle of the orbit, and is entirely Mammalian in form. The dentary bone appears to form the coronoid process, though it does not reach back to the articulation, and the lower jaw is certainly composite. As in Carnivorous Mammals, there is no heel prolonged beyond the articulation, and the articular process is only slightly inflected inward.

Thirdly, there is no descending tympanic process of the skull like that seen in Dicynodonts; and on this character depends the backward extension of the jugal arch by its squamosal element to the articulation for the lower jaw, which it contributes to form, though the quadrate bone still remains, though of small size, seen on the posterior aspect of the skull.

Fourthly, there is a manifest difference from Dicynodonts in the occipital articulation, though it is imperfectly exposed, for there appears to be no trace of a basi-occipital condyle.

Nythosaurus larvatus (OWEN) is a *Galesaurus*. It agrees with the type skull in size and form of the cerebral region. Its molar teeth show three or four denticles, and appear to be about eight in number. The dentary bone similarly extends far back, and forms the coronoid process. A perfect mould is preserved of the auditory region, and shows the vertical semicircular canal and the horizontal semicircular canal of the auditory region (Plate 9, figs. 5, 6). The former passes outward, backward, and downward, and from its base the latter extends horizontally forward. On the right side there is indication of a third canal, directed forward at right angles to the posterior vertical.

Relation of the European to the South African Anomodonts.

KUTORGA, FISCHER, EICHWALD, and VON MEYER have described and variously interpreted Reptiles from the Permian rocks of Orenburg, some of which have been classed by Sir R. OWEN as Theriodonts. KUTORGA, with only fragments of the humerus, recognized Mammalian characters, and regarded the animal to which they belonged as a Mammal. For this type, which FISCHER named *Eurosaurus* in 1841, KUTORGA adopted the name *Brithopus;* and I concur with GAUDRY in preferring the older name. EICHWALD's conception was, however, a remarkable one. He states ('Lethæa Rossica,' p. 1630) that *Eurosaurus* has the skull of a Labyrinthodont, with vertebræ and phalanges like those of *Mastodonsaurus,* and the femur, tibia, coracoid, and scapula like *Pelorosaurus* and *Hylæosaurus*; and he associated KUTORGA's genera *Brithopus* and *Orthopus* as the humerus of that animal. The foundation for this interpretation is partly in the skull which VON MEYER named *Melosaurus,* and regarded as Labyrinthodont; and, reviewing EICHWALD's work, that writer considered the association of bones so dissimilar in size and character in one animal to be improbable. It is impossible to form an independent opinion without studying the original materials at Moscow and St. Petersburgh; but the Labyrinthodont character of the skull of *Pareiasaurus*, and many other Labyrinthodont features in the vertebral column, in combination with Dicynodont characters in the pelvis, may justify a suspension of judgment on the conclusions adopted by EICHWALD. But whether the Orenburg fossils should prove to be allied to *Pareiasaurus,* or to some other Anomodont type, they are associated with remains named *Rhopalodon,* which VON MEYER compares in its teeth to *Galesaurus,* though the palate carries a row of small conical teeth on the hinder outer margin of the pterygoid; and the same beds yield *Deuterosaurus,* which VON MEYER compared with the *Bathygnathus* of LEIDY.

In this type there are eleven dorsal vertebræ at least, and, according to EICHWALD and OWEN, two sacral vertebræ, though VON MEYER inclines to think there may have been three, and that the strong transverse process of the eleventh dorsal contributed to support the ilium, although that vertebra was not united with the sacrum. Every dorsal rib unites with two transverse processes, in this respect probably rather approximating to the type of *Pareiasaurus* than to Dicynodonts. There can be no doubt that the pelvis was also substantially formed on the Dicynodont plan, though the antero-posterior processes of the ilium appear to have been much less developed even than in *Phocosaurus.* In 1866, VON MEYER described some additional remains from Orenburg, which he referred to *Eurosaurus verus,* and which may be accepted as making better known the skeleton of *Brithopus priscus* of KUTORGA. I have examined the figured bones preserved in the Senckenberg Museum. They comprise a fragment of a large tooth with a finely serrated border, comparable to canine teeth in many South African genera, described by Sir R. OWEN as Theriodont. VON MEYER remarks on the close resemblance of the posterior part of the skull to the corresponding region of *Dicynodon.* It is obviously formed on the same general plan, but the foramen magnum is triangular, and broader than high. The occipital condyle, which is also very wide, appears to be tripartite, as in Dicynodonts. There is a median excavation below the basi-occipital part of the condyle, though this is much narrower than in Dicynodonts, and rather suggests Placodonts. There is a vertical median ridge above the summit of the foramen magnum. On the other side of the specimen there is an impression of that ex-occipital process which extends transversely outward, and gives attachment to the lower part of the squamosal. The impression was evidently due to a squamous bone being lost from it, and I can only surmise that the missing element may have been the quadrate; but such a relation of the quadrate is never found in any Dicynodont. The impression of the lateral border of the cerebral cavity is seen, and appears to have been relatively wider than in South African Reptiles. The large foramen for the fifth and other nerves is partly defined, and the ali-sphenoid, which bounds it in front, ascends as a slender process. Between the ali-sphenoids the brain cavity contracts. There is a median excavation for its floor in the basi-sphenoid. Anteriorly the basi-sphenoid terminates in a large transverse sutural facet. There are thus conspicuous resemblances of this occipital plate to the bone figured in this memoir, R. 1021, and differences which show it to have belonged to another generic type of the same order. A single vertebra is figured by VON MEYER. Its neural arch rather suggests Nothosaurs, while the cupped form of the body is Plesiosaurian. The vertebra appears to be dorsal, and the rib articulates by two heads, but narrowly separated from each other, rather suggesting the Plesiosaurian than the Ichthyosaurian type, but making a transition from the double-headed anterior dorsal rib type of *Pareiasaurus* to the single-headed dorsal type of *Plesiosaurus*. The scapular arch is instructive, although the bones are imperfect. It was formed by the scapula and coracoid and pre-coracoid. The pre-coracoid is a comparatively large bone, which extends to the margin of the articular surface for the humerus. It is perforated by the usual foramen, which passes obliquely forward, so that on the internal surface it excavates the margin of the scapula, in the way seen in *Dicynodon*. Other specimens show the scapula as a strong compressed plate of somewhat Dinosaurian form, but not dissimilar to types of scapulæ from South Africa. The humerus has the radial crest moderately developed, and has a rather more slender shaft than is usual in the Dicynodont family, though more slender bones are known. The pelvis is remarkable for the way in which the ilium contracts above the acetabulum, and for the narrow superior facet in the acetabulum for articulation with the femur. It more suggests *Phocosaurus* than any Dicynodont; and the obturator foramen has a similar oblique passage through the bone, extending forward towards the pubic border. The narrowing of the superior mass of the ilium may be profitably compared with the spatulate condition of the attached end of the ilium in *Plesiosaurus* (as well as with the reduced dimensions of the bone in *Nothosaurus*). The expanded forms of the pubis and ischium are intermediate in character, as in mode of union, between the conditions of those bones in Plesiosaurs, Nothosaurs, and Ichthyosaurs. A proximal end of an ulna is figured by VON MEYER, which is interesting as showing not only similar form to that seen in South African Dicynodonts, but it also gives evidence that the bone developed an epiphysis or olecranon ossification, which has been lost. Other fragments of long bones appear to be referable to the fibula, but they are too imperfect for determination without re-examination of the specimens. There is, therefore, no doubt that *Brithopus*, if all these bones are correctly referred to it, belongs to the Anomodont order, and that its place is substantially that assigned to it by Sir R. OWEN. There is also good reason for accepting the conclusion that *Deuterosaurus* and *Rhopalodon* must be closely associated with it, and they contribute materially to a knowledge of the vertebral column and limb bones of the group. But I should place them in the sub-order Gennetotheria.

I have seen no evidence which establishes generic identity between the Anomodonts from the Triassic rocks of India and the Dicynodonts from South Africa.

Comparison with Placodus. (See Plate 24, figs. 5, 6.)

In *Placodus* the malar succeeds the maxillary, and behind the orbit overlaps the squamosal, which is equally deep and is prolonged backward, forming the outer bar of the temporal foss. The relation of the squamosal to the expanded plate of the back of the skull is that of an Anomodont, for the back of the skull in *Placodus* is a basin-shaped space. The quadrate bones descend below the squamosals at the outer limits of the basin, but, except at the margin of the condyle, which articulates with the lower jaw, the bone is not exposed in lateral view. Below the squamosal is a bone which is in the position of the supra-quadrate and quadrato-jugal, and it appears to be divided by an oblique suture which would separate the transverse supra-quadrate part from the vertical quadrate part or quadrato-jugal. There is a sub-circular excavation at the anterior angle where these parts meet, and this concavity, which is probably auditory, forms the posterior and inferior limit of the compressed vertical temporal arch.

The mode of union of the head with the vertebræ was remarkable. *Placodus* shows no sign of a basi-occipital condylar articulation, for the inferior margin of the foramen magnum is a thin film of bone. But, laterally, on each side of the middle of the foramen magnum, the ex-occipital bones are prolonged outward and backward, exactly like the posterior zygapophyses of a vertebra; and on the left side, which alone is disengaged from the matrix, this process shows on its inferior surface a transversely oblong flat facet, which looks downward. This is the occipital condyle; and thus *Placodus* has two occipital condyles, which closely approach to the Mammalian type, and the neural arch of the atlas, and not the centrum, articulates with the skull so far as the evidence goes. Therefore I am led to compare the Placodontia and Cotylosauria, and to infer that they are probably members of the same group of animals. Since the Mammalian atlas unites with the skull, by elements of the neural arch, and the centrum takes no part in the articulation, we seem to find in *Placodus* a Reptile in which the mode of union of the vertebral column and skull usual in Reptiles is lost, and that which characterizes Mammals is assumed.

On the Relation to the Anomodontia of the Fossil Animals termed Pelycosauria and Cotylosauria.

In his catalogue of the Permian Reptiles of North America, Professor COPE enumerates 15 genera and 39 species described by himself since 1877, which are referred to a group named Pelycosauria. This group is combined with the Anomodontia into an order named Theromorpha. Few of the American fossils have been figured, so that their exact relation to the Anomodontia is not easily determined; but, in so far as I can judge from the description, few of the characters relied upon to differentiate them sustain the author's estimate of their importance in classification, while their affinity to the Anomodontia is so close that I can realize no obstacle to grouping the Pelycosauria as a sub-order of Anomodonts. I base this conclusion on the following facts:—

(1) Professor COPE defines the Pelycosaurian scapular arch as consisting of scapula, coracoid, and epi-coracoid, blended like an os innominatum. But Sir R. OWEN, in 1876, in his 'South African Catalogue,' Plate 69, figs. 5, 6, figured a South African specimen which shows this condition, and he regarded the fossil as Dicynodont. Professor COPE remarks on the Mammalian character of the scapular arch, and states that in *Dimetrodon* the coracoid is smaller than the epi-coracoid, as in Monotremes.

(2) The author also affirms that the pelvic arch is identical in structure with that of the Anomodontia, and is considered to resemble *Echidna*.

(3) In the limb bones reference is made to the possible presence of epiphyses in Pelycosauria. I have found epiphyses to be well developed in the limb bones of Anomodonts. The humerus is said to resemble that of *Echidna*, but the nature of the resemblance is not stated.

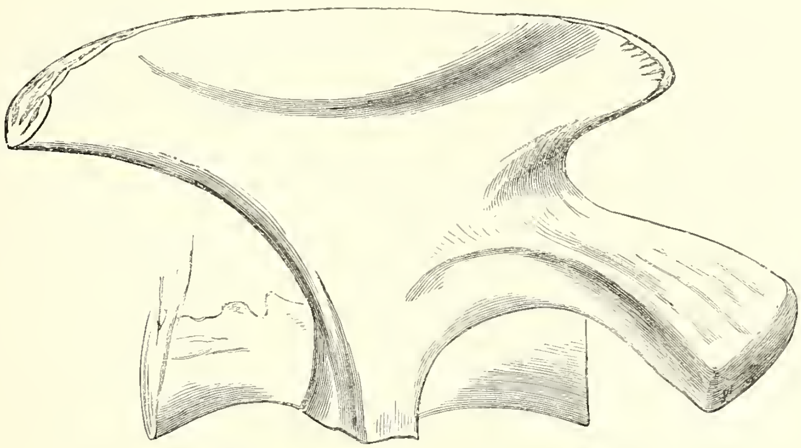
The Pelycosauria.

There are few data for judging of the systematic value of the Pelycosauria. But in view of the fact that the Anomodontia was originally made to include animals which are allied to the Pelycosauria, supposing that group to be well founded, it seems more in accordance with usage to class those animals among the Anomodonts than to adopt a new name like *Theromorpha* for a well-known ordinal type.

There is need, however, that the distinctness of the Pelycosauria should be established. The tibiale and centrale are said to unite to form an astragalus which has no movement on the tibia. One face of the astragalus receives the cuboid. Subsequently an entire tarsus was figured which has a very Mammalian aspect. It is regarded as referable to *Clepsydrops natalis* (Cope)*, and is classed as Pelycosaurian. A similar tarsus was subsequently referred with doubt to the genus *Theropleura*.† It is difficult to judge of its importance. Its characters appear to be more Mammalian than those of the Crocodilian tarsus, for the bones of the distal row are completely ossified. The tarsus is absolutely unknown in any Anomodont from Africa, Europe, or Asia ; and, therefore, there is no means of comparison with this American fossil.

The Pelycosauria are said to have two or three sacral vertebræ, a notochordal column, and inter-centra usually present. With the evidence that Dinosaurs may have as few as two sacral vertebræ, as well as a larger number than has been found

Fig. 5.



Ilium and Sacrum of *Zanclodon*.

in any Anomodont, this ground of ordinal distinction fails. Similarly, the mode of ossification of the inter-vertebral substance presents many types among Anomodonts, one of which already figured by Sir R. Owen might be regarded as notochordal. What the value of the inter-centra may be I am unable to say, as they have not been figured ; but, inter-centra, as I understand them, are not unknown among Anomodonts.

The remarkable vertebral column with vertically elongated neural spines referred to *Dimetrodon* is apparently unlike any known Anomodont, but the elongation of the neural spines in certain Wealden Reptiles, like (?) *Hylæosaurus*, is not considered to militate against their position in the group to which they belong. And it may be doubted if the more extraordinary neural spine of *Naosaurus* (*loc. cit.*, Plate III.), with its transverse branches, has any greater classificational value, since the transverse branches are the only character by which the author separates *Naosaurus* from *Dimetrodon*. In *Theropleura*, which is also described as having elevated neural spines, abdominal dermal rods are found. These appear to be of the same nature as

* 'Amer. Phil. Soc. Proc.,' August, 1884.

† 'Amer. Phil. Soc. Trans.,' vol. 16, Plate III.

the abdominal rods of *Protorosaurus*, and *Mesosaurus*, from South Africa ; but I shall, when dealing with the latter type be able to show that those rods are composite ribs comparable with the abdominal armature of Plesiosaurians. Professor COPE has also noticed abdominal rods in *Stereosternum*, and in a Batrachian genus *Ichthyacanthus*. No such structure is known in any Anomodont, but there is no evidence of its absence ; and *a priori* considerations suggest that it will be found.

Professor COPE's contributions to a knowledge of the skull are of great interest. The only genera which have been figured are *Empedias* and *Naosaurus*. The former is referred to the Diadectidæ, defined as Pelycosauria with transverse molar teeth. A cast of the brain cavity in this type has also been figured. The author describes the brain case as extending between the orbits, and in that family it is said to be completely closed in front, after the manner of Ophidians. Sir R. OWEN has described a Theriodont *Nythosaurus* (*Galesaurus*) ('S.A. Cat.,' Plate XXXIV., fig. 2) in which a similar condition appears to exist ; only there is no such enlargement of the cerebral epiphysis in the South African fossil, and the American fossil, although widening anterior to the epiphysis, expands in a much less marked manner. It may be remarked in passing that the vertical foramen for the fifth nerve, figured by Professor COPE in the cast of the brain case, is quite in harmony with the vertical foramen similarly placed in Anomodonts. In another genus, *Edaphosaurus*, Professor COPE describes a distinct element as connecting the basi-occipital on each side with the quadrate. This is not figured, but the description is suggestively indicative of the bone figured by Sir R. OWEN in 1845 in *Dicynodon lacerticeps*, which was then regarded as the par-occipital. This bone is found to be characteristic of the Dicynodontia. Professor COPE regards the skull in the Diadectidæ as possibly forming the type of a sub-order, for which the name Cotylosauria was suggested. There is a plain facet on each side of the foramen magnum, which expands largely below these facets. The bone which bounds the foramen inferiorly presents a vertical median posteriorly projecting process, on each side of which there is a transverse cotylus, much like those of an atlas which are applied to the occipital condyles of the Mammalian skull. These concavities are further said to occupy precisely the position of the Mammalian condyles. The bone in which they are excavated is said to have the form of the Mammalian basi-occipital and of the Reptilian sphenoid. The author afterwards expressed doubt as to whether this form of articulation might not be due to the loss of a loosely articulated basi-occipital bone. This is the most distinctive feature of the Pelycosauria, but it does not appear to extend beyond the family Diadectidæ. The occipital condyle is described as undivided in *Edaphosaurus*, and, therefore, the argument tends towards the conclusion that the Cotylosauria may be distinguished from the Dicynodontia, but not to sustain the Pelycosauria without further evidence. The nearest approximation to this condition of the occipital articulation with which I am acquainted is that seen in the Placodontia ; and, in so far as I can judge from the evidences given in the figures of the skull of *Empedias* ('Amer. Phil. Soc. Proc,' vol. 19, Plate V.), there are no ordinal characters to separate the Cotylosauria from that group. The pterygoid bones are similarly expanded; the vacuities of the skull which can be compared are similarly placed; the quadrate bone appears to be similarly excavated in the auditory region.

The description of the palate in *Empedias* is unintelligible when compared with the figures, for, although the pterygoid bones may be identified by their posterior position and by meeting the quadrate, as well as by the downward direction of their external borders, they are described as the palatines. The median bone in front of them is termed the vomer, and said to carry two rows of small conical teeth. This bone is stated to be separated from the maxillary by a groove, which is represented in the figure. Hence, the pterygoid bones and vomer are the only elements of the palate described, excepting the greatly expanded palatine plates of the maxillary; and I therefore infer that the palatine bones must have occupied the posterior part of the groove between the pterygoid, vomer, and maxillary bones. If the palatines are thus lost and absent, the skull would still have points in common with the Anomodont group, though the absence of a median vacuity, defined by the pterygoids and palatines, is a remarkable difference; but it is a character shared by the Placodontia, and by the Endothiodontia—supposing the latter group to be distinct from the former, which has yet to be established. The figure which Professor Cope has given of the skull of *Naosaurus* establishes a well developed maxillary dentition, but differs in remarkable characters from the Dicynodontia in the conditions and relations which are attributed to the quadrate and squamosal bones; but they do not differ from the Dicynodontia more than do the Pareiasauria, or the Placodontia, hardly more than the Theriodontia. On the evidence of the skull I am led to regard the Cotylosauria as intermediate between the Placodontia and the Theriodontia, and the Pelycosauria, in so far as it is possible to judge from the fragment of skull of *Naosaurus* representing it, is intermediate between the Gennëtotheria and the Placodontia.

Comparison of Anomodontia and Protorosauria.

There is no evidence of close affinity between these groups which would at present justify their association under one ordinal type, yet their relation to each other appears to be closer than has hitherto been supposed. The pelvis of *Protorosaurus* is essentially intermediate between that of Ornithosaurs and Anomodonts. The limb bones are more slender than in any known Anomodont, but the somewhat Mammalian character of the tarsus, if unknown in the Anomodontia, appears to be paralleled in the American animals which Professor Cope names Pelycosauria. Although the evidence is very imperfect and inconclusive, I am disposed, from a cast of the Freiberg specimen of *Protorosaurus* which Dr. Woodward has obtained for the British Museum, to think that the scapular arch in that specimen probably includes pre-coracoid elements, and may be constructed upon the Anomodont plan. The circumstance that teeth occur upon the bones of the palate in *Procolophon*, and that the vomera meet the pterygoid bones with the palatines external to them in the same relative positions as in my restoration of *Protorosaurus* ('Phil. Trans.,' B., vol. 178 (1887), p. 205), would prepare me to find other points of correspondence in the skulls of those types, while the fact that the Anomodonts occur in the Permian rocks of France and Russia makes an affinity with that type less improbable in the Thüringerwald Saurians.

Comparison of the Anomodontia with the Saurischia.

The skeleton is imperfectly known in the Saurischia in details of structure; but the following resemblances may have value as showing affinity.

The ilium in both types may be extended behind the acetabulum as well as in front of it; but in several genera there is a tendency for the anterior extension to be the more conspicuous. The pubis and ischium meet by a median vertical suture; but while these bones are thus united in known Anomodonts down to the median symphysis, there is in the Saurischia a more or less large ventral vacuity by which the median symphysis of the two pubic bones is separated from the corresponding union of the ischia.

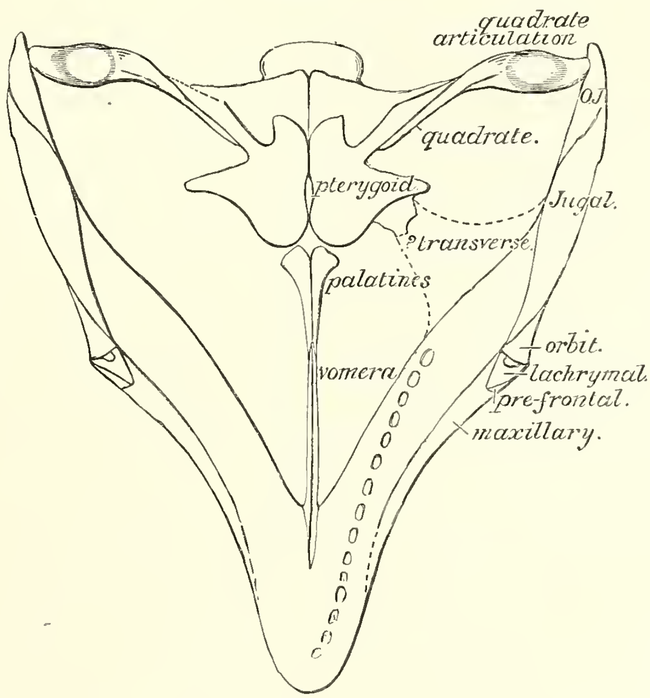
The larger limb bones may have much in common, and I am aware of no satisfactory characters by which the femur, tibia, fibula, ulna, and radius could be always differentiated, and the humerus has enough in common to make the distinction of type dependent upon the absence from the Saurischia of the foramen or foramina in the shaft. The divergence is conspicuous in the carpus and tarsus and the smaller bones of the foot; and the scapular arch appears to be formed on a totally different plan. In the vertebral column there is enough in common to have led Sir R. Owen to group *Pareiasaurus* and *Tapinocephalus* with the Dinosauria. The cervical vertebræ have the ribs articulated by two heads; but I am aware of no evidence that any Anomodonts, except Pareiasaurians, have this kind of articulation in the dorsal region, and therefore the vertebral characters will prove to be essentially Sauropterygian, with only such divergence as may be correlated with difference in the condition of existence. The resemblance which is found in the sacrum of some genera seems to me to be an induced resemblance, and not an inherited character. The tail is but imperfectly known in Anomodonts, but it is always short, and no examples of long chevron bones are at present known. The skull appears to be constructed upon a different plan, but it is only known among the Saurischia in *Compsognathus* and *Ceratosaurus*, and in neither type is the structure of the palate available for comparison.

Relation between Dicynodon *and* Scelidosaurus.

Scelidosaurus makes a certain approximation in some respects to the Dicynodont skull, but the resemblances are less important than might appear. Thus, though the quadrate bone is concealed, as in Dicynodonts, it is a long, comparatively slender bone, which is not in front of the squamosal, and not wedged into it, while the quadrato-jugal, which covers its distal end, is itself covered by the malar. Internally the quadrate of *Scelidosaurus* sends a long process inward, which laps in front of the quadrate process of the pterygoid. Hence the forms and relations of the quadrate bone in the two types are altogether dissimilar.

There is a certain resemblance in palatal structures, as may be seen from the accompanying restoration of the palate in *Scelidosaurus*. But the pterygoid bones of

Fig. 6.



Restoration of the palate of *Scelidosaurus*, from the specimen in the British Museum.

the Ornithischian are not anchylosed; and, although the bone has a similar posterior expansion in *Dicynodon*, and a similar pterygoid process, it possesses an external process which *Dicynodon* has not, and in front of that process there is a fragment which may be part of a transverse bone. If so, it was probably prolonged laterally to the malar, and not anteriorly, like the pterygoid of *Dicynodon*. Anterior to the pterygoid *Scelidosaurus* has a small ossification which appears to be a delicate palatine bone placed in the median line, and therefore unlike the lateral palatine of *Dicynodon*, while at the anterior fracture the double vomera make a difference from the vomer of *Dicynodon;* so that I regard the palate in the two groups as formed on different types.

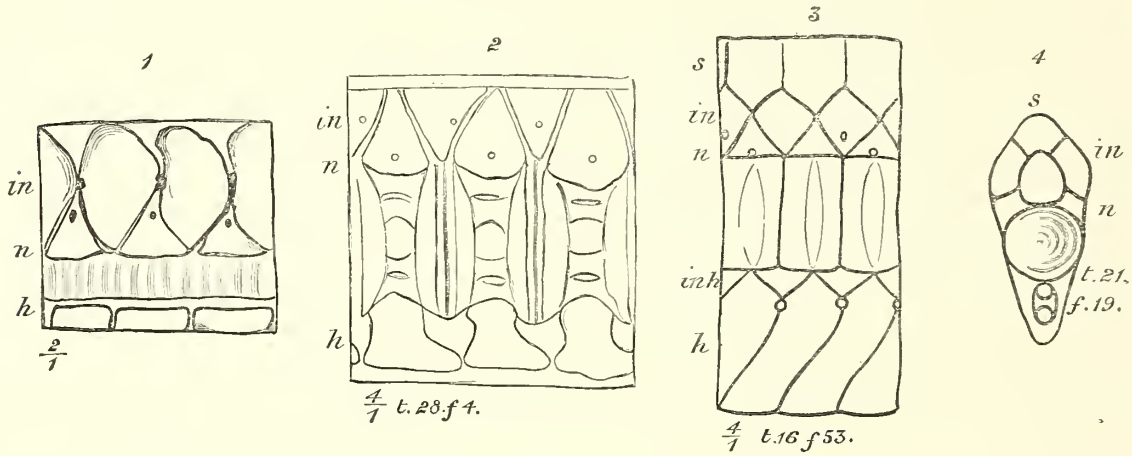
The Theory of the Anomodont Skull.

Only when it is established that the Dicynodontia, and therefore the allied Anomodonts, differ as sub-orders from the Pareiasauria, does it become possible to realize the magnitude of the changes which a skull may undergo in the same natural group of animals, and also how considerable is the gap which remains to be filled in before the most Mammalian type of Anomodont could be transformed, in so far as its skull is concerned, into a Mammal. We are at present ignorant of the modes of elaboration of such change, beyond knowing that certain bones have to be obliterated from the Reptilian skull to make it Mammalian. But whether that loss was brought about by the peculiarly Reptilian elements dwindling in size until they disappeared, while the peculiarly Mammalian elements augmented their growth in a corresponding way to take their places, or whether the Mammalian type implies such an osteological retrogression as the loss of some fundamental segmentations which divide bones from each other in ancestral types, cannot be determined, even with probability, without the aid of theory. I have already regarded the skull as a more primitive part of the skeleton than the vertebral column, less specialized; so that it preserves structures, which originally existed in the vertebral column, long after the vertebræ have lost them.* I have compared the roof bones of the skull to the roof bones of the vertebral column which exist in those plagiostomous Fishes in which there is a superior intercalary segment introduced between two adjacent neural arches. This seems to me to explain the presence in the skulls of lower Vertebrates of those bones which have been termed inter-parietal, post-frontal, and pre-frontal. The inter-parietal persists in some Mammals, and in some orders is absent. When it is absent it is manifestly blended with the supra-occipital. I see no reason for thinking that the inter-parietal gradually disappeared, and that the supra-occipital grew at its expense; but, just as the inter-centrum may become blended with the centrum, so these bones have blended with some associated elements in the skull. The argument in favour of this interpretation rests upon the fact that in those Fishes in which the intercalary neural elements are present the neural arches form an unbroken continuous tube, whereas in those animals in which they are not found there are more or less appreciable gaps between contiguous neural arches; and, if any element in the covering of the skull were absent, it seems more probable that a fontanelle would result than that the other bones would take its place. Similarly, in certain Chelonians, like *Podocnemis* and *Rhinochelys,* the pre-frontal bones retain the distinct individuality which characterizes them in other Reptilia; whereas in the majority of Chelonians this individuality is lost, and the pre-frontal bones are not differentiated

* "The History of the Skull," King's College Science Society, October, 1882.

from the nasal bones. Thus the suture becomes lost, not by the growth of the nasal at the expense of the pre-frontal, but by an absence of segmentation which causes the nasal region of such Chelonians to become Mammalian. In the same way I would interpret the loss of the post-frontal bone in Mammals. The common position of this bone is at the back of the orbital vacuity. It is manifest that in many Mammals there is no bone between the posterior border of the orbit and the jugal bone below, and in all such cases the bone may be lost through not being ossified. But in other types the post-orbital bar is present, and mainly formed by the frontal bone. It therefore would seem probable that the post-frontal had lost its individuality, because in the vertebral plan it was a portion of the arch formed by the frontal bones, just as the pre-frontals were portions of the arch formed by the nasal bones. The parietal bones in Lizards appear to show the accomplishment of a union of a similar kind. Theoretically there should be a pair of bones between the parietal and inter-parietal elements. These bones are not found, but the parietal is seen to bifurcate posteriorly, and the bifurcations have no obvious relation to the plan of the median element of the bone. There is some evidence, though very inconclusive, that these posterior arms of the parietals are separate ossifications in the Dicynodontia. They extend along the parietal crest, parallel to each other, overlapping the end of the parietal, and they appear to diverge forward, whereas the parietal bone of Dicynodonts is undivided. If this distinction should hereafter be established, it would contribute an element of symmetry in the theory of the skull, and would help to fortify the theoretical principles on which, in the matter of the bones referred to, a transition might be made from the Reptilian to the Mammalian type.

A more important difference between Reptiles and Mammals is found in the mode of union of the lower jaw with the skull. Theory has for a long time concerned itself with the fate of the quadrate bone. Sir RICHARD OWEN, following the school of CUVIER, termed the quadrate bone the tympanic, and taught that it becomes in Mammals the ring which supports the drum of the ear. This is a view which follows naturally enough from the study of the Chelonian skull; but I should never have seen my way to accept it without the evidence which Anomodont skulls give of the history of the quadrate bone, and its relation to the squamosal. Hitherto those bones have been imperfectly understood. The squamosal is of large size and sends a zygomatic process forward, which combines with the malar bone to form the zygomatic arch, and it sends a process downward, in which the quadrate bone is embedded. I have here figured several examples of this relation of the quadrate. The squamosal extends in front of it and hides it, and extends internal to it, so that the lower jaw comes to articulate with the squamosal bone apparently, as well as with the quadrate. In one example the quadrate bone is perforated by a large excavation; and I regard this excavation as auditory and comparable to the auditory notch or excavation in the Chelonian quadrate. In the Theriodont *Galesaurus* the form of the skull has become Mammalian; the inferior process of the squamosal is lost, and the zygomatic process forms the larger part of the bone, and it is only on the posterior aspect of the skull that what appears to be the small quadrate is seen, taking part with the squamosal in forming the articulation. Another step in the evidence of transition is needed, but I cannot doubt that when once a direct articulation is established between the lower jaw and the squamosal, with the articulation moved a little forward, and the small quadrate seen only behind, that the loss of work would lead to a diminished growth of that bone; and that no function is eventually left to the quadrate but to support the tympanic membrane and surround the auditory aperture. ALBRECHT, finding that occasionally a suture separates the articular part of the squamosal bone from the squamous portion in mammals, concludes that the zygomatic portion of that bone represents the quadrate bone, but, as we have seen that the squamosal in Anomodonts has the same relation to the skull, and to the lower jaw, as in Mammals, this interpretation has no support in the Anomodontia.



Lamna. *Pristis.* *Aëtobates.* *Chimæra.*

Showing the relation of the centrum to elements of the neural arch in Elasmobranch Fishes, after HASSE.

Professor COPE has described the quadrate bone of *Clepsydrops natalis* as having a horizontal ramus, which he affirms to be "nothing more than the zygomatic process of the squamosal bone of the Mammalia forming with the malar bone the zygomatic arch." But, from the fact that in the Dicynodontia and Theriodontia the squamosal bone always takes the development and function here attributed by COPE to the quadrate, there is an *a priori* improbability that a type so nearly allied to the Anomodontia should present a fundamentally different structure, when the external characters are described as similar. It is difficult to suppose there has been any error in the interpretation of the facts, since Professor COPE, in 1870, recognized the quadrate bone in a South African Anomodont skull; but in the absence of figures it is impossible to judge of the evidence on which the interpretation rests.

A feature which specially distinguishes the Dicynodont skull from the skulls of allied animals is the enormous development of the squamosal bone, and, although it is difficult to speak with confidence on a matter that is necessarily hypothetical, it seems to me probable that this development is the cause for the scattered positions of the otic bones, and that when the squamosal becomes smaller those bones come into closer relation.

The skull structure is especially suggestive in relation to elements in the Mammalian auditory region which are not found in Reptiles. In *Echidna* two elements are seen, one an imperfect circle, and another external and anterior to it. The former of these is in contact with the pterygoid, just as is the quadrate bone in Reptiles and Birds. The latter is extended between the squamosal and the pterygoid, and meets the tympanic ring. It is regarded as the malleus. This bone almost exactly corresponds in position with the bone in the Dicynodont skull which has been often referred to in my descriptions of the palate. The tympanic ring similarly corresponds to the quadrate bone; and the relations of malleus and tympanic in *Echidna* to each other, and to the surrounding skull bones, are almost exactly those of the quadrate and malleus in Anomodonts, though both bones are relatively much larger in the Reptile than in Mammals. Hence it seems to follow that when the squamosal came to extend outside the quadrate and in front of it, taking on itself part of the function of forming the articulation for the lower jaw, that the quadrate and malleus would be thrown inward and backward, and diminish in size at the same time. Some steps in this process of degeneration are seen in Anomodonts, and they are all approximations towards the Mammalian type.

The difficulty in harmonizing the composite structure of the Reptilian lower jaw with the simple Mammalian jaw is similar to the difficulty with the composite roof bones of the Reptilian skull. In the most Mammal-like of Reptiles, *Galesaurus*, the lower jaw remains as Reptilian as in a Chelonian or Crocodile. The Mammalian might be derived from the Reptilian mandible, in one of two ways. It may be supposed that the elements forming the lower jaw ceased to be segmented, as we have assumed in explanation of the roof bones of the skull, and, therefore, that the Mammalian jaw includes the same elements as the Reptilian jaw, but in an undifferentiated condition. In favour of this view it might be urged that a yet more improbable development of a like kind is seen in existing Chelonians, where the dentary elements of the opposite sides lose their individuality, and form a single dentary element which unites the rami. But, perhaps, it may be as well to remember, before following this speculation further, that the articular element of the lower jaw would necessarily undergo a certain change of function akin to those of the quadrate bone, by which it shares the articulation with the sur-angular element in the same way as the quadrate shares the articulation with the squamosal. And, if the articular bone ceases to make the joint with the quadrate, owing to the abstraction of the quadrate from such work in the skull, it should result that the articular bone ceases to be ossified, because the mechanical conditions which determined its ossification have disappeared. The lower jaw is distinctly formed about MECKEL'S cartilage; and, whereas the articular bone is an ossification at the terminal end of that cartilage, and the only part of it which is ossified, it is instructive to note that, in the Mammalian jaw, the foramen by which the cartilage leaves it is some distance in advance of the region in which the articulation is placed. This condition has always seemed to me conclusive against the articular element persisting in the Mammalian jaw. Secondly, the dentary bone attains a varying development. In *Galesaurus* it is very large, and apparently rises into a coronoid process as well developed as in any Mammal. But it seems inconceivable that it could ever come to form the articulation with the squamosal if that articulation was previously established with the sur-angular bone. There appears, therefore, to be a necessity for the preservation of parts which correspond to the dentary and sur-angular and angular elements. But I see no such necessity for the preservation of the splenial bone, which in Crocodiles is little more than a long scale on the inner side of the dentary, or of the coronoid bone which is internal in position to the coronoid process; so that I suppose the three successive bones on the inner side of the Reptilian lower jaws to become lost in the Mammal, and the three external bones to become united and preserved as one continuous ossification. It may be within the limits of possibility that, after the articular bone was lost, the angular bone on which it rests also disappeared from the changed mechanical conditions which affected its ossification, and that the dentary bone, extending backward at its expense, may have eventually invested the outlet for MECKEL's cartilage before its union with the sur-angular bone was obliterated. Therefore there are facts which seem to point to a loss of some elements from the Reptilian jaw by absence of ossification, and other facts which render the union of the remaining bones by a loss of segmentation highly probable.

Classification.

It would be premature at present to do more than recognize the larger groups into which the Anomodontia may be divided. Among such sub-ordinal divisions are the following:—

	Sub-order.	Example.
Basi-occipital articulation. No temporal vacuities. No median bar to inter-clavicle	PAREIASAURIA	*Pareiasaurus.*
Median bar to inter-clavicle. No temporal vacuities. Teeth on pterygoid and vomer	PROCOLOPHONIA	*Procolophon.*
Tripartite occipital condyle. Descending process of squamosal. Not more than one tooth in each maxillary	DICYNODONTIA	*Dicynodon.*
	GENNETOTHERIA	*Lycosaurus.*
Large, laterally compressed incisors, separated by canines from small pointed molars	PELYCOSAURIA (?)	*Clepsydrops.*
[Ex-occipital condyles.] No descending process to squamosal which articulates with lower jaw. Molar teeth with pointed cusps	THERIODONTIA	*Galesaurus.*
Ex-occipital condyles. Molar teeth transversely developed, with cusps	COTYLOSAURIA	*Empedias.*
Ex-occipital condyles. Crushing teeth on vomer, pterygoid, and maxillary	PLACODONTIA	*Placodus.*

This list does not exhaust the modifications which the Anomodont type assumes. It is rather a grade of organization than an order. Its affinities are of the widest kind. Its lowest group connects Reptiles with Labyrinthodonts and Amphibians; its intermediate groups have affinities with all the extinct orders of Reptiles; and its highest groups make approximations to Mammals which go some way towards demonstrating their Reptilian origin.

I would express my grateful thanks to Dr. HENRY WOODWARD, F.R.S., for the many facilities afforded me in making this examination of the Anomodont Reptilia in the Geological Department of the British Museum.

EXPLANATION OF PLATES 9–25.

PLATE 9.

Galesaurus *and* Procolophon.

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VII. *On some Variations of* Cardium edule *apparently Correlated to the Conditions of Life.*

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[Plate 26.]

Introduction.

The following paper forms part of an investigation of the relation between the variations of animals and the conditions under which they live. It appears to me necessary that any investigation of this problem should be begun by the examination of cases in which difference in environment is known to exist, and that variations should then be sought for among the forms of life subjected to these conditions. If by this examination any variations can be shown to occur regularly with the change of conditions, or in any way in proportion to their intensity, it is so far evidence that there is a relation of cause and effect between them.

By thus first approaching the question from the point of view of the conditions, many difficulties are obviated which occur in any attempt which begins by ascertaining the variations in the animal, in the hope of afterwards finding an environmental change to which they may be traced. Such attempts to trace back variations to some environmental cause have often been made, and have, in general, been unsuccessful. In the case of species which have varied in isolated situations not apparently differing from each other, the failure to find points of environmental difference has been held to be evidence that the variations in question did not arise from such causes at all. This appears likely, and is probably true of the variations in question; but it must be borne in mind that the fact that no palpable difference can be found between the conditions in the several localities is no proof that they do not exist. While these differences in condition are usually evasive and hard to detect, it is best to begin to investigate their relation to variations in animals by selecting cases in which the change in conditions is unequivocal, and proceed from this starting point to seek for correlated variation in the forms of life subjected to them.

It appears that a particularly favourable opportunity for investigating this question is offered by the fauna of isolated lakes of various composition and of different degrees of salinity, and the following observations were made in accordance with this view. They are chiefly interesting owing to the great scarcity of any systematic observations of the relations between variation and the condition of life and to the rare occurrence of opportunities for investigating them.

While it has been held by some persons that the conditions of life are without definite effect in producing variations in animals, others, on the contrary, regard their production as an obvious consequence. The result of my investigations is to show that the whole relation between variation and conditions is much more complicated than it would be in accordance with either of these views; and that, while one animal may be profoundly and uniformly modified in every case by a certain change of conditions, yet these same changes produce no palpable effect on an allied animal of a different sort. For example, particulars will be given of the constant modification of *Cardium edule* consequent upon the drying up of the lakes in which they were, while *Dreissena polymorpha* and *Hydrobia ulvæ* do not appear to have been affected. It may be here remarked that the general variability of a form, as *Dreissena polymorpha*, does not appear to predispose it to assume a new form for a given change of condition.

In view of the fact that definite variations have been shown to be produced in *Cardium edule* by change in the composition of the water, it next becomes desirable to know to what extent these changes would be maintained if the conditions were altered back again to their original state. Upon this point I have no evidence; but that the animals would, if they lived and propagated, ultimately regain their former structure appears probable; for, since it can be shown that certain variations are constantly produced by water of certain constitution, it practically follows that the maintenance of these variations depends also on the same cause. It would, however, be of the greatest interest to ascertain the length of time and the number of generations necessary to effect these changes.

The specimens forming the subject of this paper were collected in the district of the Aral Sea and in Egypt.

In 1886 and 1887 I made a journey to some of the lakes of Western Central Asia, for the purpose of making observations on their fauna. As the waters of these lakes are of very various composition, being salt, alkaline, bitter, or fresh in differing degrees, I looked forward to an opportunity of investigating the question whether these diverse environmental conditions produce any correlated changes in the structure of the animals which are exposed to them. The collections made with this object consist chiefly of Crustacea, of which an account will be published hereafter.

In the course of the journey thus undertaken, I visited the northern shores of the Aral Sea and the sandy region called Kara-Kum, over a part of which, at least, the Aral Sea formerly extended, as is shown by the quantities of shells of the Aral Sea Cockle which are strewn on it. The area from which the Aral Sea has thus receded is not a level tract, but contains three considerable depressions, called respectively Shumish Kul, Jaksi Klich, and Jaman Klich. When the level of the sea was changed these three depressions remained, for a time, as isolated lakes, each containing a separate sample of the fauna of the sea living in it. The lakes gradually dried up, becoming salter and salter; and it is the object of the present paper to investigate the changes which befell the animals inhabiting them during this process.

General Account of the Desiccation of the Aral Sea.

Before entering into a detailed account of these lakes, it may be well to describe briefly the present conditions of the Aral Sea itself, of which they once formed a part. As is well known, the Aral Sea is a closed basin, receiving the waters of two rivers only, the Syr Darya and the Amu Darya. In this respect, it resembles the Caspian Sea, which receives the Volga, Ural, and Emba rivers. It is universally supposed that these two seas were united at a comparatively recent period. The evidence for this belief is the statement that banks of shells of species now living in the Caspian Sea are found on the land lying between them. As the level of the Caspian Sea is now 84 feet below that of the Black Sea, and the level of the Aral Sea is 128 feet above that of the Black Sea, if it be supposed that the respective levels of the beds of these two seas were formerly the same as they are now, it follows that the Caspian Sea must, at the time of its connection with the Aral Sea, have been more than 200 feet deeper than it now is. On the other hand, the change in the levels of the two seas may have been due to subsidence of the bottom of the one, elevation of the other, or both. It is further supposed by many that the conjoined Aralo-Caspian Sea had a northward extension, probably on the east of the Ural range, thus connecting with the Arctic Ocean. One reason for this belief, amongst others, is the presence of a Seal in the Caspian Sea whose affinities are rather with *Phoca vitulina* of the Arctic Ocean than with *P. fœtida* of the Mediterranean. It has also been supposed that this Aralo-Caspian Sea had an eastward extension as far as Lake Balkhash. The reason for this view is not easy to suggest, as none of the typical Aral fauna occur in Balkhash, nor are any deposits of Aralian shells found between the two waters. It may be added that Balkhash is bounded, both north and west, by very considerable hills, the Koi Djarlegan, &c.

Moreover, apart from the question as to the extent of the hypothetical Aralo-Caspian Sea, it has been suggested that the Aral Sea, at all events, has retired in recent times from some considerable area, and is continuing to recede thus. This statement, which occurs in several text-books, would appear to be only partially supported by the facts which came within my own observations. In the summer and autumn of 1886, I visited the whole north shore of the Aral Sea lying between Gulf Peroffsky and the mouth of the Syr Darya. From Togusken to Sary Cheganak the shore is formed by high cliffs composed of horizontal beds of Eocene formation, containing fossils. Of these I collected some 130 species, which have been examined by Mr. T. Roberts, of St.

John's College, and Mr. KEEPING, who state them to be of about the age of the London Clay and of the Bracklesham beds of England. In some places these cliffs rise from the water's edge, and in others recede from it, opening up considerable valleys which slope gradually down to the shore. In places where the cliffs do not abut on to the water there is generally a sandy beach, but occasionally, as at Ḳuḳturnaḳ, there is a steep bank of large shingle and pebbles. The shores of the Sary Cheganaḳ (Yellow Gulf), which forms the northern limit of the Aral Sea, are low lying and sandy. These sands extend northward and eastward for about 150 miles, constituting the Ḳara Ḳum (Black Sand). The southern edge of the Ḳara Ḳum is thus the northern shore of the Aral Sea, and it is generally assumed that it was covered by those waters at a comparatively recent period.

The waters of the Aral Sea oscillate greatly under the pressure of the wind, and this effect is especially seen when the wind is from the south for some days. The water is then driven in some hundreds of feet over the almost horizontal beach of the Sary Cheganaḳ.

The Mollusca which have been recorded as occurring in the Aral Sea are *Cardium edule, Adacna vitrea, Dreissena polymorpha, Neritina fluviatilis, Hydrobia ulvæ.* In addition to these I found *Hydrobia spica* in large quantities (this species is already known from the Caspian Sea) and also *Neritina* (? n. sp.).

The *Cardium* occurs in great numbers on all parts of the shore which I visited, and when the wind falls and the sea retires the shore is left covered with stranded Cockles. The highest limit to which the flood thus induced ever reaches is in this way more or less clearly shown by the fresh shells and other *débris* left behind. Above the level of this fresh deposit the ground is always strewn with old shells, indicating the area covered in past times by the water. The coast of the south-west shore of the peninsula Ḳuḳturnaḳ is covered entirely with Cockle shells, extending in a band nearly a mile wide. With the exception of those points in which the cliff rises from the water's edge, there is always a tract of shore on which shells are found. On the hypothesis that the Aral Sea formerly had a much greater depth than at present, it would be expected that shells would be thus found in position for a considerable height above the present level, but this is not the case. On the contrary, where the shores are more or less steep the shells are found in great quantities up to a certain level, about 15 feet above the water, and above this level they are never found. In places where the land slopes very gradually to the water level the horizontal extent of the shell-covered tract is very great, being as much as 15 miles in some places; but whenever the ground rises suddenly so as to reach a greater height than about 15 feet above the Aral Sea level no more shells are found. The fact that the shells cease abruptly at a definite horizon is true both in sandy parts of the coast and on the clayey tracts, and it is equally true of those deposits of shells which occur in the bottom of valleys opening to the sea which are now altogether dry, but which were formerly filled by the sea. Some of these deposits of shells reach inland four or five miles (*e.g.*, Meregen Sai), but always without any marked rising of the ground ; where any elevation occurs the level at which the shells cease is always definite and striking.

The absence of shells above a definite level seems to suggest that the sea has never in recent times extended over parts above that level. There is nothing to suggest that any Aral Sea deposits, higher than this line of demarcation, have been denuded. For, had denudation been the cause of the absence of Aral shells above this line, it would be expected that the shells would *gradually* disappear on a line travelling up from the sea, and that they would disappear at different levels in different places, which is not the case. If, therefore, the Aral Sea did ever extend over a greater tract of country than that which would be covered by it if it rose about 15 feet above its present level, it can only be supposed that such a condition occurred in the remote past, and not that it has gradually diminished to its present size from a much greater extent, as has been often suggested. Moreover, if the Aral Sea had recently retired from a greatly extended area, it must have covered the Ḳara Ḳum entirely, extending to Lake Tschalkar, which is marked on the Russian maps as a lake about 40 miles long and 25 miles broad, forming the termination of the great valleys of the Irghiz and Turgai streams. In the belief that such a connection might have formerly existed between Lake Tschalkar and the Sea of Aral, I travelled down the Irghiz river as far as the lake. I found it to be a vast sheet of salt mud, which becomes dry in summer in most places. The joint stream of the Irghiz and Turgai never reaches the main part of the lake, becoming lost in reedy morasses of nearly fresh water at the western end. The lake was so dry that my camels crossed the west end of it in the beginning of August. Its northern shore is bounded by a range of hills which rise about 600 to 800 feet from the lake. Their southern front, which faces towards the lake, is nearly vertical, and is cut in places by ravines. These hills are composed of horizontal beds containing Eocene fossils, similar to those which were found in the hills on the north-west of the Aral Sea. Above these beds was a deposit of horizontally stratified sand about 80 feet thick.

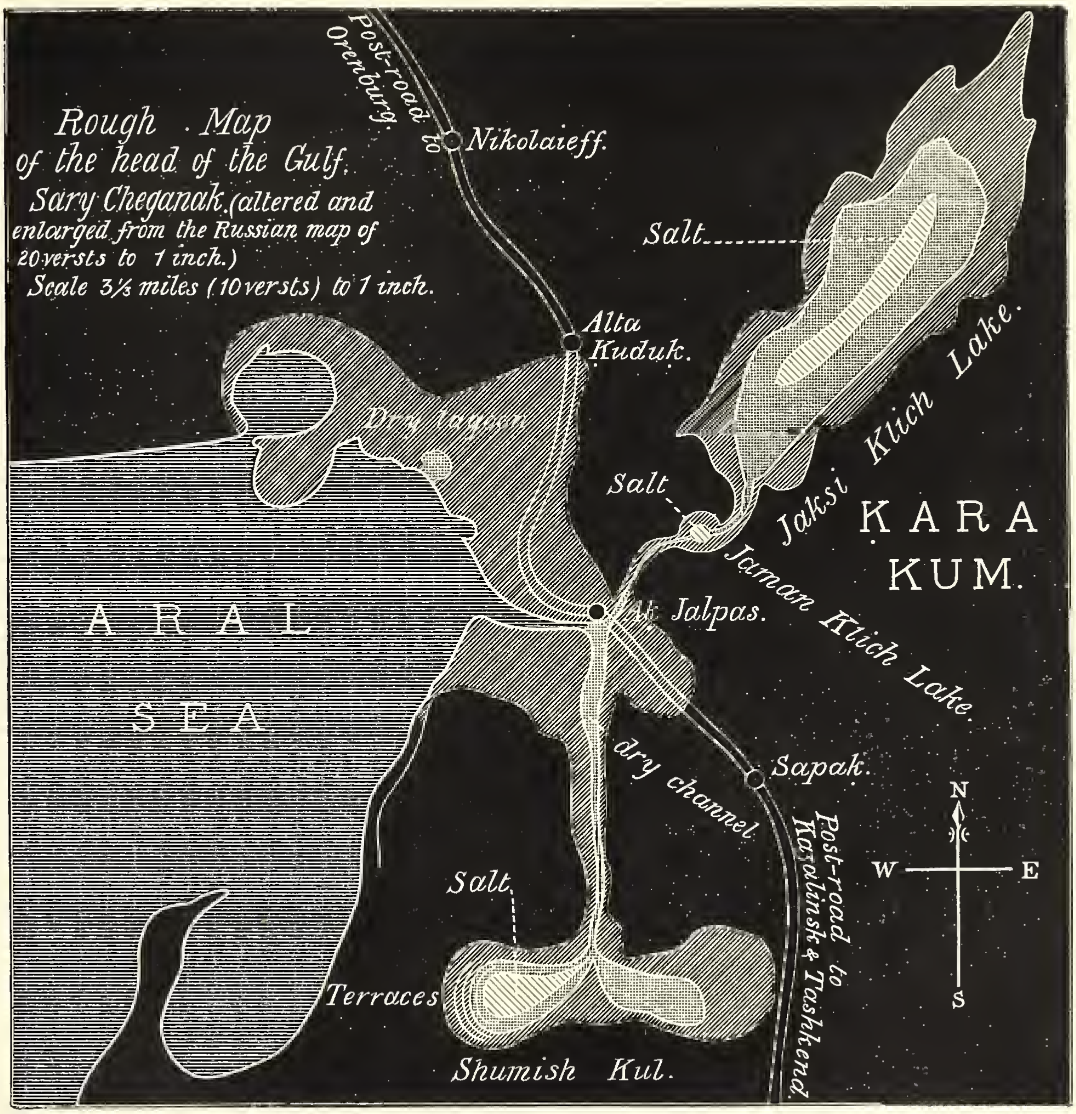
In no case, either in the ravines, or among the hills, or on the shores of the lake, or in the *débris* thrown up at the mouths of the wells, were any shells found other than those of the fossiliferous beds. There was no trace of the previous presence of the Aral Sea. The ground did not differ in any way from considerable low-lying tracts near the Aral Sea, which remain covered with Cockles ; and, had the sea recently been in Lake Tschalkar, these shells could not have failed to be found in quantity. Also in the Ḳara Ḳum, excepting the above-mentioned low-lying tracts, the ground is without Cockles, but on descending to these depressions the deposit of shells is suddenly reached. This is true in the case of the north end of the depression, Jaksi Klich, which, though 15 miles from the Aral Sea, was formerly joined with it by a channel, and equally true of the steepest parts of the bank, as, for example, where the southern slopes of Togusken rise almost vertically from the water's edge. I have also every reason to believe that those parts of the Ḳara Ḳum which I did not visit are also without Aral shells. I made particular and independent inquiry from many of the Kirghiz who live in various parts of the Ḳara Ḳum, showing them Cockle shells (*Aigulaḳ*), and asking if they knew any localities where they were found. They all said that they had seen them at Jaksi Klich, Jaman Klich, and Shumish Kul, which are in the depressed regions, but they had never seen them in any other locality. I made special inquiry with regard to Aris Kul, which is marked on the maps as a considerable depression lying to the east of the Ḳara Ḳum, and I was told by several persons, independently, that no such shells were found there. For these reasons, it seems that, though the Aral Sea has retired within recent times from such an area as would be covered by it if its level were about 15 feet higher than it now is, yet it cannot be shown that it has continuously receded from an area much larger than this. If it ever extended over the Ḳara Ḳum northwards to Tschalkar, this must have been in the remote past, and its disappearance from the definite shell-covered area must have been a comparatively recent event, not continuous with its disappearance from the larger and vaguely defined region which it is supposed to have covered in later Tertiary times.

Special Account of the Basins Jaksi Klich, Jaman Klich, and Shumish Kul.

The region where the greatest exposure has taken place is situated to the north and east of the Sary Cheganak. Here the sandy coast slopes very gradually to the sea, and at the post-station Alta Ḳuduḳ, for example, the shell-covered region is about 3–4 miles wide. But at Aḳ Jalpas there is a dry channel running up from the bay, which divides into two branches, the one running east and north, and the other running south. The latter has a course of about six miles; near Aḳ Jalpas it is about half a mile wide, and is covered with mud, which is impassable after rain. Further south the channel narrows, and in some of the deeper holes in it there is always a little very salt water. This channel runs in a depression between the hills Aḳ Jar and Bultuḳ, and then opens out into a great depression, lying east and west, for a distance of about 8 miles. This place is known to the natives as Shumish Kul. (It is marked on the Russian maps as "Khan Sultan." This name is not known on the spot, though the mountain at the east end of the lake is called Khan Turt.)

The appearance of this lake is very striking. The north and west shores are formed by bare hills, with a few bushes and coarse grass at their base. Thence to the bottom of the lake is a tract of undulating sand, bearing scanty vegetation. Below the sand a stretch of baked mud is exposed, surrounding the pan of salt which fills the lowest part of the lake. The salt lies in large contorted sheets, overlaying each other like frozen waves of muddy ice. On the eastern and southern shores, which shelve away gradually to more distant hills, are great flats of salt mud covered with *Salicornia*, &c.

The biological interest of this place lies in the fact that upon the steep western shore are marked very definite terraces, showing the position of the water at different periods during the progress of the gradual drying up of the lake. On each of these terraces Cockle shells are found in great quantities, having been left there when the water was at the level of the terrace. A series of specimens, therefore, taken from each terrace from above downwards, gives examples of the shells as they were at each stage during the progressive desiccation of the lake. On several of the terraces the shells are paired shells, with the ligaments more or less preserved, placed upon their



oral surfaces, just as they were when alive, being kept in position by a crust of sand cemented with oxide of iron. Unfortunately, there is no reliable means of estimating the time which elapsed during the process of drying up. The intervals of time, however, between the formation of the successive terraces were sufficiently long to enable the shells to acquire definite characters, especially of colour and texture, which made it easy to distinguish shells of any one terrace from those of the one above or below it.

The principal terraces are seven in number, but, before describing in detail the condition of the shells on them, it may be well to give a general account of the changes which were produced in correlation to the diminished size of the lake. The principal changes are as as follows :—

(1.) *Diminution in the Thickness of the Shells,* which is first apparent in the shells of the third terrace. It proceeds to such an extent that the shells of the lowest terrace are almost horny and semi-transparent.

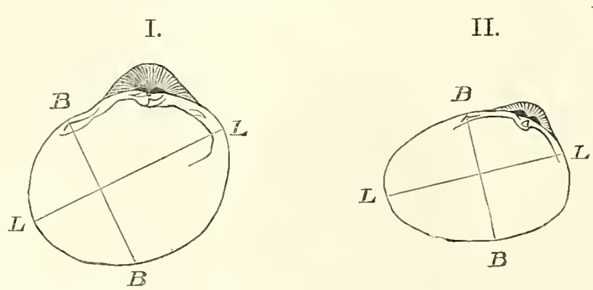
(2.) *The Size of the Beak is Greatly Reduced.*—In the shells of the upper terraces the beak encloses, so to speak, a separate chamber, while in those of the lower terraces it hardly forms a projection on the outside of the shell.

(3.) *The Shells become Highly Coloured.*—This change and (1) occur almost uniformly. The shells of each terrace are very nearly alike in texture, thickness, and degree of coloration.

(4.) *The Grooves between the Ribs appear on the Inside of the Shell as Ridges with Rectangular Faces.*—This change first affects only the ribs behind the 8th or 10th, but on the lowest terraces all the ribs are so affected.

(5.) On the lowest terrace *the shells diminish greatly in absolute size.*

(6.) *The Length of the Shells in proportion to their Breadth Increases.*—I use the term "length" to mean the greatest antero-posterior dimension, and the term "breadth" to mean the dorso-ventral measurement at right angles to the length, passing in right valves across the point of the posterior tooth, and in left valves across the depression into which the posterior tooth of the right valve fits.



Diagrams showing the directions in which the length and breadth of the shells are measured :—
I. A shell from the shore of the Aral Sea.
II. A shell from Jaksi Klich (inner deposit) L, L, length; B, B, breadth.

It must be remembered that, though the tooth is a fixed point in the morphology of the shell, there is no defined point on the ventral margin which can be determined in each shell for comparison with other shells. Hence, I am aware that the points selected for these measurements are arbitrary, and that they are not taken in absolutely homologous places in every shell. Nevertheless, they are very nearly so, and on the whole they are more satisfactory than any others. The object of these measurements is to obtain an arithmetical conception of the difference in the proportion of length to breadth which is apparent to the eye. This difference in appearance is almost all due to a change in the proportion which the greatest length bears to the greatest breadth at right angles to it. The measurements, owing to the irregularities of the shell, were made accurate to half a millimetre, and I believe that any difference due to variation in the selection of the exactly comparable morphological point on the ventral edge of the shell would be found to be within this limit of error. I am also aware that conchologists use the term length for the shorter of these two measurements; but, as this appears confusing to the general reader, it seems better in a paper of more general biological interest to use the terms in their ordinary sense. In comparing these shells of the upper terraces with those of the lower, it will be found that the greatest length is greater in proportion to the greatest width than it is in those shells which have been exposed to salter water.

I have made tables which are intended to show this change in the proportions of the shells in a tabular form. The Tables bring out three points :—

(*a.*) That the change in proportions does not occur in all the shells, nor to an equal degree in those in which it is found. Thereby it differs from the changes which occur in the texture and colouring. A few shells may be found in any terrace at Shumish Kul which do not differ materially in shape from normal shells. In the case of Jaksi Klich, however, almost all the shells are affected.

(*b.*) The second point noticeable in the occurrence of this variation is that it is far more marked in shells of greater absolute size (that is, presumably, of greater age) than in smaller and younger ones. This fact is brought out in the second column of the Tables.

(*c.*) The third fact which appears on comparing the averages is that the lengthening of the shells occurred slightly in the shells of the second terrace; increasingly in those of the third and fourth; reaching a point in the fifth terrace which is practically not afterwards exceeded in shells found as much as 30 feet lower, though the changes in texture, &c., had greatly progressed in these latter. Evidence will be given, moreover, which tends to show that this lengthening of the shells is more probably due to some other consequence of the diminished size of the lake than to the increase in saltness; for example, to its increasing shallowness and consequent high average temperature in summer. Examples will be given of Cockles from lagoons both of the Aral Sea and in Egypt, which, while differing entirely from those of these salt lakes in general appearance, are yet like them in the proportion of length to breadth. The whole question will be fully considered after the specimens have been described. The shells on the several terraces may now be described in detail.

First Terrace.—The shells on the first terrace were, no doubt, living at the time when the Aral Sea was connected with this series of lakes and, perhaps, also for a short period after its separation from them. They lie at the foot of the hills coming down to the lake, and, though mostly covered with earth, it was possible to get plenty of them, especially among the *débris* thrown out by burrowing animals. They are, for the most part, smallish shells, being chiefly 19 mm. to 24 mm. in length. They are thick shells, pale in colour, having from 18 to 22 ribs, the region behind the 11th to 14th ribs being purplish in colour.

No paired shells were found at this level. In 30 shells, all between 21 mm. and 16 mm. in length, the average ratio of length to breadth is 1 : 0·799 ; that is to say, that the average breadth of a shell 20 mm. long would be 15·98 mm.

The Second Terrace.—This is a flat about 50 paces across. Upon it are two well marked ridges of shells, the lowest of which is about 10 feet below the level of the first terrace. These ridges were obviously formed by the casting-up of shells on the beach during gales, as may be seen on the shore of the Aral Sea in many places (Meregen Sai, &c.). They contain no paired shells with ligaments, such as are found lower down in places where the bottom of the lake has been exposed and not afterwards disturbed.

Shells on this terrace were found of the maximum length of 26 mm. They have from 18 to 21 ribs, the region behind the 11th to 16th being purplish in colour. In 20 shells taken from the lower of the two ridges of shells on this terrace the average ratio of length to breadth is 1 : 0·770 in shells between 26 mm. and 20 mm. in length, and 1 : 0·782 in shells between 21 mm. and 16 mm. in length ; that is to say, that among shells similar in size to those of the first terrace the average breadth of a shell 20 mm. long would be 15·64 mm. The shells do not differ materially in consistency from those of the first terrace (*vide* Table of Comparative Weights) ; they are, however, slightly more highly coloured.

The Third Terrace consists of a strip of small sand-hills about 180 yards wide. The division between it and the region which I have called the fourth terrace is not sharply defined, but is indicated by a ring of old tamarisks. Such rings of tamarisks occur round many of the salt lakes of this steppe, and always show that the water stood at a definite level below them for a sufficiently long period to influence the vegetation. Some of the lakes in the Turgai district were surrounded by several concentric rings of tamarisks, showing several distinct periods in the progressive drying up of the water. This ring of tamarisks stands at a level about 20 feet below that of the ridge of shells which marked the lower limit of the second terrace. Amongst the bases of these sand-hills are many Cockles *in situ*, with their ligaments preserved, indicating that this part of the shore remains as it was when it formed part of the bottom of the lake. The shells on this terrace differ from those of the second terrace, being thinner, and showing that appearance of grooving on the inside of the shell which was referred to above (4). In shells of this terrace the grooving is not much marked in the case of ribs anterior to about the 11th. The number of ribs and distribution of colour are as they were in the last terrace.

In 30 shells between 22 mm. and 18 mm. long the average ratio of length to breadth was 1 : 0·751 ; that is to say, that the average breadth of a shell 20 mm. long would be 15·02 mm.

The Fourth Terrace is like the last, in that it is a stretch of shelving sand about

100 yards across, falling about 10 feet in level. On it also are many paired shells *in situ.* These shells differ considerably from those of the third terrace, being much thinner and more highly coloured (*vide* Table of Weights). The grooving on the inside of the shells is generally well marked in all behind the 7th rib.

There are generally only about 17 to 19 well marked ribs, the remainder being slightly indicated on the purple posterior surface of the shell. Most of the shell is purple behind about the 11th rib, and the whole shell is suffused with pinkish-purple (see Plate 26, fig. 4).

In 30 shells whose lengths vary between 26 mm. and 18 mm., the average ratio of length to breadth is 1 : 0·730, and, taking 30 shells from 16 mm. to 21 mm. long, this average ratio is 1 : 0·735; that is to say, that the average breadth of a shell 20 mm· long would be 14·7 mm. The beaks are reduced in size.

The Fifth Terrace is a similar stretch of sand; it is 200 yards wide, falling nearly 20 feet, and upon it are very many paired shells placed on their oral faces, like the others. These shells are much thinner than those of the fourth terrace. They have 15 to 17 well marked ribs, and almost the whole shell is purple in colour in some specimens, but in others the first 3 ribs remain yellowish. The ribbing on the inside of the shell is generally apparent behind the 4th or 5th rib. The beaks are still further reduced in size.

In 30 shells between 27·5 mm. and 21 mm. in length the average ratio of length to breadth is 1 : 0·731; and in 30 shells between 21 mm. and 16 mm. long this average ratio is 1 : 0·743; that is to say, that the average breadth of a shell 20 mm. long is 14·8 mm., not materially differing from those of the last terrace. This terrace ends with the shelving sand. Below it are mud flats, the upper part of which is covered with heaps of muddy sand, cemented together with salt, forming the *Sixth Terrace.* The shells upon it, however, do not differ materially from those of the last, except, perhaps, in being rather thinner.

Below it is the lowest level at which shells are found (*Seventh Terrace*). This level is 8 to 10 feet below that of the fifth terrace, and distant from it about 200 yards. Upon this lowest level are several (five) concentric ridges, composed of shells washed up and partially cemented together with oxide of iron. The shells of which these ridges are made are like those of the fifth and sixth terraces. On the flat mud between these ridges, especially between the fourth and fifth, are great numbers of small paired shells placed on their oral faces. These shells are those of the last Cockles which lived in the lake before it was dried up. At this time the water must have been very salt indeed, as the salt bed itself is about 5 to 6 feet lower and 300 yards distant.

The shells are very small. The largest paired shell found in this place was 21 mm. long. They have 14 to 15 distinct ribs, are very thin, and of an almost uniform purple colour. The grooves between the ribs are all marked on the inside of the shell as ridges with flat sides. The beaks project very little from the general curve of the shell.

The average ratio of length to breadth in 30 shells the lengths of which were between 16 and 21 mm. is 1 : 0·725 ; that is to say, the average breadth of a shell 20 mm. long is 14·50 mm., as compared with 15·98 mm. in the case of the shells of the highest level.

Dreissena polymorpha.—At one side of the lake on the level of the third terrace were found many shells of this form, which did not differ from those of the Aral Sea. The same is true of *Hydrobia ulvæ*, which was found in fair quantities on most of the terraces.

Jaksi Klich.

This is the largest, superficially, of the three dry lakes containing Cockles. Its length is about 10 miles, and its breadth 3 miles. It differs from Shumish Kul in being comparatively shallow. While the former must have been nearly 60 feet deep at the time of the separation from the Aral Sea, the basin of Jaksi Klich cannot have been more than 15 to 20 feet deep. There is not in it a distinct series of terraces, as at Shumish Kul, but the shells occur in two chief deposits, the one marking the original high level of the water, and the other forming a band round the salt which now fills the bottom of the lake. Moreover, owing to the fact that the shells of the outermost deposit are almost all single valves, and not paired shells *in situ*, as at Shumish Kul, a good deal of mixing has become possible amongst them, which was, no doubt, facilitated by the shallowness of the lake ; as the banks are so flat that at the time when the lake was low it may have happened that under a strong wind the water was driven upon the shore even as high as its original level. Hence it results that the upper deposit of shells at Jaksi Klich is more mixed in character than the deposits hitherto described. I will first describe the condition of the shells found at the bottom of the lake. They occur there in enormous numbers, being for the most part washed up into banks. A certain number of paired shells occurs between the ridges. Their texture is uniformly thin and papery, and they are very highly coloured, thus resembling the shells of the lower terraces of Shumish Kul, especially those of the sixth terrace. Their length is very great, and this feature is found in almost every individual shell. While they thus resemble in many respects the shells from the salter levels of Shumish Kul, they yet have several features peculiar to themselves, especially the enormously greater degree to which they are elongated ; also, though their colour resembles the Shumish Kul shells in being much brighter than that of ordinary Aral Sea Cockles, it has a character of its own, which would make it impossible to mistake a shell from either locality.

As will be seen in the Tables, the average ratio of length to breadth in 30 shells varying in length between 30 mm. and 25·5 mm. is 1 : 0·660 ; and in 30 shells varying in length from 25·5 mm. to 19 mm. is 1 : 0·682. It will be seen, therefore, that the increased proportional length is greater in these shells than in any others that were obtained.

The size of the beaks is reduced, just as in the case of the Shumish Kul shells. The shells of the outermost deposit at Jaksi Klich are, as stated above, rather mixed in character. I found, however, one locality towards the southern end of the lake where the bank was comparatively steep, and from this place I obtained a fairly uniform sample. These shells are thin as compared with Cockles of the Aral Sea, but thicker than those of the lower deposit of Jaksi Klich. From the latter they differ also in not being highly coloured and in having the beaks fairly developed, though diminished relatively to those of normal Aral shells. As will be shown hereafter, they very closely resemble those shells which were found on the shore of the lagoon Abu Kir, in Egypt; the length of these shells is as great in proportion to their breadth as it is in those of the fourth or fifth terrace at Shumish Kul. The average ratio of length to breadth in 30 shells varying in length between 22 mm. and 17 mm. is 1 : 0·740; that is to say, that the average breadth of a shell 20 mm. long is 14·8 mm.

Many examples of *Hydrobia ulvæ* were found amongst these modified shells, but they do not differ from those of the Aral Sea.

In attempting to realise the conditions under which the Cockles lived in Jaksi Klich before the separation of this series of lakes from the Aral Sea, the fact of its situation must be borne in mind. It was a large lagoon, ten miles long and three miles broad, very shallow, and connected with the main body of the Aral Sea only by a narrow and shallow channel at Ak Jalpas. Hence the conditions of life in it, in a climate which undergoes the greatest extremes of heat and cold, must have been always very different from those prevailing in Shumish Kul, which had a considerable depth, and so must have maintained a much more constant temperature.

Before, therefore, the communication between the lakes and the Aral Sea was interrupted it is clear that the water of Jaksi Klich must have been sometimes very hot, and, from the consequent evaporation, it was probably in summer much salter than the nearest parts of the Aral Sea. In view then of the obvious correlation between the effects of the diminution in size of Shumish Kul and the increase in the proportional length and thinness, &c., of the shells found there, it appears reasonable to ascribe these appearances in the shells of the outer deposit at Jaksi Klich to similar causes, and these must of necessity have existed, consequent upon the peculiar situation and shallowness of the basin.

All these appearances, as has been shown, became greatly intensified in those shells which lived in it during the period after the separation from the Aral Sea.

Besides the shells in these two deposits, there were found at Jaksi Klich a few shells of an entirely different character. These were very large and very thick shells, generally occurring in pairs, more or less buried in the sand, though without ligaments. The length of these shells was about 30 to 35 mm.; they almost all show the feature of great proportional length and large beaks, and were always found in groups of ten or twelve, lying between the outer and inner deposits. These shells are all much worn. I shall allude to these shells as "great shells." Similar shells will be shown to have occurred at Jaman Klich, on the sand flats between Jaman Klich and Shumish Kul, in a small dry lagoon lately separated from the Aral Sea, and in the old deposits at Abu Kir (Mandara, Plate 26, fig. 12). Taken in connection with these cases of the occurrence of such shells, I think that there can be little doubt that shells of this type are connected with life in shallow lagoons opening out from a sea. All the five localities in which they were found were of this kind, and none were ever found by me anywhere else. On the shores of the Aral Sea and at Shumish Kul none occurred.

Jaman Klich.

This is the smallest of the three dry lakes. It was little more than a large pool formed by a widening and deepening of the channel which connected Jaksi Klich with the Aral Sea. At the time when it was full of water its diameter was about half a mile, and its depth 15 to 20 feet. The bottom of the lake is covered by a sheet of salt about 300 yards across. The shells upon the upper part of its shore do not differ materially from those of the Aral Sea, being thick shells with large beaks and little colour. Their proportional length is rather greater than that of the Aral Sea shells. There is little or no ribbing on their inner surfaces.

The shells at the bottom of Jaman Klich are thin, highly-coloured shells, with much ribbing on the inside and beaks greatly reduced in size. They are greatly elongated, though less so than the shells of Jaksi Klich. The average ratio of length to breadth in 30 shells varying in length between 24 mm. and 16 mm. is 1 : 0·726, being practically the same average ratio as that in the shells at the bottom of Shumish Kul.

Amonst these shells were great quantities of *Dreissena polymorpha*, which, though, as always, very variable in shape, did not differ in any uniform manner from those of the Aral Sea.

At the bottom of Jaman Klich is a considerable number of "great shells." They are like those of Jaksi Klich and are much worn.

On the flat between these two lakes and Shumish Kul are many shells strewn. They are, in all respects, like those of the upper deposits at Jaman Klich, and in no way remarkable. There are amongst them a few "great shells," but no thin or highly coloured ones, which occur only in the three lake beds.

Cockles of the Aral Sea.

In the Aral Sea itself, the Cockles are of very uniform character. They are fairly thick shells (see Table of Weights). The anterior 10 to 11 ribs are generally white, and the remaining 6 or 8 bluish or brown.

There are no "great shells" among them, nor any thin and highly coloured ones.

The average ratio of length to breadth in 30 shells of the Aral Sea varying in length between 22 and 18·5 mm. is 1 : 0·761.

The beaks in every case are large and well developed.

On the west shore of the Sary Cheganaḳ, near Alta Ḳuduḳ, is a small dry lagoon, which had once communicated with the Aral Sea. It was about half a mile wide and had been about 2 to 3 feet deep. In it were many Cockle shells; nearly all of these were "great shells," the remainder being shells of the ordinary Aral type.

This completes the description of the Cockles of the district of the Aral Sea. It has been shown that in each locality a particular type prevails, which varies hardly at all as regards texture and colour, and that, though the individuals of each type vary considerably in shape, yet that there is a distinct preponderance of long shells among those which have been exposed to the conditions incidental to the drying up of the lakes in which they were living; and that, in the case of each of three lakes, the changes undergone by the shells have been similar, though different in degree.

I will now describe the shells found in the lagoons near Alexandria, and then compare them with those of the Aral Sea district.

The Cockles of Lake Mareotis and Lake Abu Kir.

At the present time (1888)* Lake Abu Kir is a shallow salt lake, having an area of about 20 square miles and a depth of about 1 to 2 feet at most. In April, 1888, its specific gravity was 1·05. No living shells were found in it but its shores were covered with great quantities of uniformly small, thin, highly coloured shells (see Plate 26, fig. 10). These shells are elongated in the same way as those of Jaman Klich, which they closely resemble.

The average ratio of length to breadth in 30 of these shells varying in length from 24 mm. to 19·5 mm. is 1 : 0·738. (For average weight see Table of Weights).

These shells are plainly those of the Cockles which last lived in the lagoon of Abu Kir, and it may be supposed that they lived in it under conditions not very different from those now prevailing. It is difficult to assign with certainty any cause for their extinction, but this may perhaps have been due to an unusually dry season following on a low Nile.

The lagoons of Abu Kir and of Mareotis are separated from the sea by a narrow bank, partly of limestone and partly of sand; and from the presence of marine shells in the lagoons it is clear that they formerly communicated with the sea. The Cockles, therefore, of these lagoons are the descendants of those of the Mediterranean.

There is some reason for supposing that they passed through another condition between that of the Mediterranean type and that found on the shores at Abu Kir; for at Mandara and at other points on the shore of the Lake Abu Kir, where cuttings have been made, deposits of great quantities of shells almost invariably occur at a varying depth below the surface.

* Abu Kir was pumped out in May, 1888.

These shells are nearly all of the very large and thick type spoken of above (*vide* p. 309) as "great shells." From the great abundance of shells of this type in the deposits below the present bed of Abu Kir, it seems clear that they were numerous in the locality for a long period. As they are entirely absent among the shells now lying on the shores of the lake (namely, those which were the last inhabitants), I would suggest that these "great shells" perhaps lived there in the period when the sea communicated with the lake. This becomes still more probable in connection with the fact of the occurrence of similar shells at Jaksi Klich, Jaman Klich, and on the flats between them and Shumish Kul, for at the time when these localities were under water and connected with the Aral Sea the conditions in them could not have been very different from those prevailing in the lagoon of Abu Kir when it was open to the Mediterranean. The shells, then, of Abu Kir are of two kinds :—

(1.) Shells of animals lately extinct, which lived in a lagoon of water having a specific gravity of about 1·05 ; these shells show the same variations from the "normal" type as those of the Aral district living under similar circumstances.

(2.) "Great shells" occurring in a more or less definite bed below the level of the present lagoon, the origin of which is uncertain, but which were probably living when the lagoon was open to the sea.

Mareotis and the Fresh-water Lakes at Ramleh.

The Lake Mareotis is now divided by an embankment into an eastern and western part, which differ from each other entirely.

The western lake is full of red brine-water, and beneath the water is a permanent crust of salt.

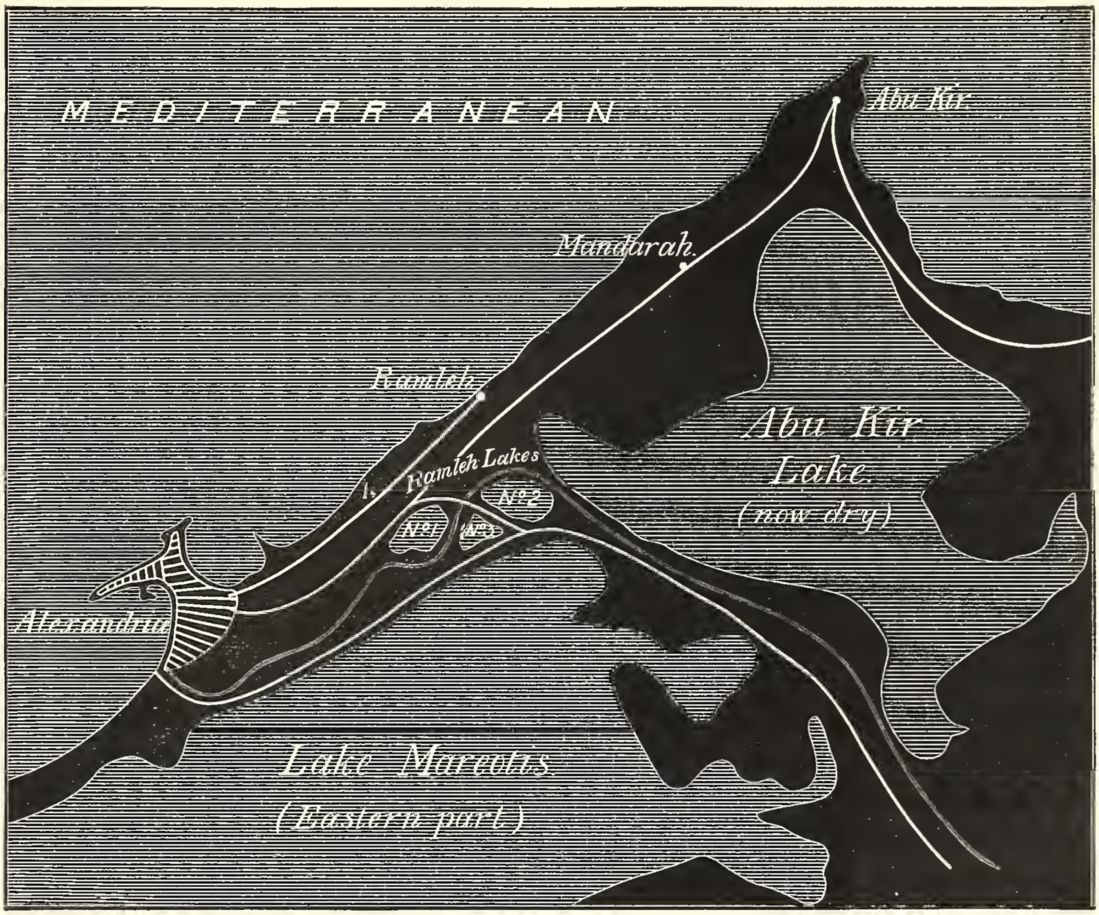
I did not succeed in finding any shells on the shore of this portion, though, no doubt, Cockles lived in it before the changes were made which have led to its present condition.

The eastern lake contained about 1 to 3 feet of water in most places in April, 1888. A very small stream of sea water runs into it near Meks. At the time of my visit its density was about that of the Mediterranean, but it, no doubt, varies greatly with the time of year and the state of the Nile. The lake, which lies 8 feet below the surface of the Mediterranean, is stated to have been nearly dry at the end of the last century, but in the course of military operations in 1801 it was again opened to the sea by the English. Possibly, then, the shells now found on its shore are the descendants of those then admitted from the Mediterranean. Another opening was lately made from the sea, but has been nearly closed, the small stream of water from the sea mentioned above being due to this opening.

At high Nile the level of the lake rises, owing to the infiltration of fresh water, and probably it is brackish at this season. From these considerations and from the many vicissitudes that the lake has undergone, it is clear that nothing can be stated with certainty as to the conditions which have prevailed in it for any length of time, beyond the fact that it has always been a large shallow lagoon, and that a large quantity of fresh water from the Nile has been poured into it every year.

I did not find any live Cockles in it, and am disposed to believe that they are extinct in it, having probably died in consequence of some of the sudden changes which have befallen the lake.

The shells found on the banks of Mareotis are, like those of Abu Kir, of two kinds :—



Rough map of Lake Mareotis, together with Abu Kir and the three Ramleh lakes.

(1.) Old shells, for the most part very large and not greatly differing from the "great shells" of Abu Kir. These occur especially on the cliffs near the road running beside the lake on its south-west shore. The shells here occur at a level several feet above that of the lake. These shells are much worn, and may be regarded as being in a semi-fossil state. They are possibly shells which lived in the lagoon when it was open to the sea.

(2.) Shells found in great numbers on the shores of the lagoon. These are nearly all paired valves with their ligaments. They are all of a most definite type (*vide*

Plate 26, fig. 11) being moderately thin shells, having generally the anterior 6 to 10 ribs yellowish-white in colour, and the posterior 7 to 12 bluish or chocolate coloured; the inside of the shells is much ribbed; the posterior part is generally chocolate colour, and sometimes the whole interior of the shell is so coloured. Many of the shells have bands of dark colour running transversely to the ribs. The shells are nearly all elongated. The average ratio of length to breadth in 30 shells varying in length between 27 mm. and 20 mm. is 1 : 0·680; as in other samples, the elongation is more marked in large shells than in small ones.

The beaks are rather variable, but in most shells they are low.

The peculiarities in colour of these shells are so definite that they could not possibly be mistaken for the shells of Cockles from any other locality.

Amongst these modern shells are a few of the old semi-fossil shells mentioned above, which, however, are so different from them, being bleached and worn, that they may be at once distinguished.

Ramleh Lake No. 1.

By the formation of the Mahmudiyeh Canal, which was begun in 1819, a small piece of water was separated from the great Lake Mareotis, in the neighbourhood of Sidi Gaber station.

This lake is about a mile in greatest diameter, and its water is at the present time fresh, receiving much waste water from the irrigations, and is, perhaps 10 to 12 feet deep in the middle, though shallow at the sides. The bottom at the sides is sand, and in the middle is mud. Great quantities of Cockle shells lie on the bottom of the lake, but I found no live animals, and believe that they are extinct. These shells have a definite character, being thick and coarse in texture, with 14 to 16 anterior ribs, white, and from 3 to 5 posterior ribs, chocolate colour. The region of the anterior ribs (6 to 10) is generally not ribbed on the inside of the shell. The insides of the shells have a peculiar white colour. The shells are very long in proportion to their breadth, and most of them have one or more deeply marked lines of growth. The beaks are high and large. Amongst the smaller shells found here are some few which are extraordinarily thick.

Ramleh Lakes Nos. 2 *and* 3.

By the construction of the railway from Alexandria to Cairo, 1855, a second part of Mareotis has been cut off by an embankment, and the lake thus formed was again divided into two by the embankment recently made to connect the Cairo railway with the Ramleh line. In this way two lakes have been formed, an eastern (No. 2) and a western (No. 3). Both of these are fresh, receiving the water from irrigations. In the western lake I found no Cockles at all, either dead or living, though the water is crowded with Prawns (*Palæmon*, sp. ?).

In the eastern lake (No. 2) were great numbers of living Cockles. In texture these shells resemble those of Ramleh Lake No. 1, though the tendency to ribbing on the inside was not so much marked, being generally slightly present behind the 10th to 12th rib. The colour of the outside of the shells is yellowish-white almost all over, but on the inside the region of the posterior 3 to 6 ribs is chocolate colour, but the rest of the inside of the shells has the same bright white colour as in Ramleh Lake No. 1. The proportion of length to breadth is very great in these shells. In 30 shells varying in length between 29 mm. and 16·5 mm. the average ratio of length to breadth is 1 : 0·657, and in 30 shells varying in length between 21 mm. and 17 mm. this average ratio is 1 : 0·665. It is a remarkable fact that in the case of these shells the increased proportional length is almost as much marked among the small shells as it is amongst the large ones, and, as may be seen in Table VI. (*a*), this feature is present fairly uniformly in nearly all the individuals.

Another character of these fresh-water shells is the frequent occurrence of specimens with the free ventral margins of the shell bent inwards, as shown in Plate 26, fig. 13.

Recapitulation (*Mareotis and Ramleh Lakes*).

The shells found in the Mareotis and Ramleh district were of four kinds: (1) ancient shells, like the ancient shells of Abu Kir; (2) shells lately extinct (?) in Mareotis itself. Though the conditions under which these animals lived cannot be positively stated, it is nevertheless clear that they lived in shallow water, and that this water received in winter a great volume of fresh water from the Nile, being then probably brackish, while it is likely that in summer it was rather salter than the sea.

The shells having lived under these conjectural conditions have definite characters, being long, thin, highly coloured shells.

From them are descended independently (3) the Cockles of Ramleh Lake No. 1, and also (4) the Cockles of Ramleh Lake No. 2. Both (3) and (4) have been living in more or less completely fresh water for some time, and, on comparing them with (2), they will be found to differ from them similarly, and to resemble each other in most respects. They are both fairly thick and coarse, and the high colour of the Mareotis shells is much reduced in (3), and still more so in (4).

The feature of great proportional length remains in both.

As was found in the case of the various samples of Aral shells, the samples of each locality are distinct and easily recognisable, but, excepting a slight difference in colour, (3) and (4) are very nearly alike. A few specimens among (2) resemble in colour those of (3), but they are quite different in texture.

Conclusion.

The importance of these observations lies in the fact that, by examining and comparing the shells, an opportunity is given of observing the origin of a set of structural variations in correlation to, and perhaps in consequence of, environmental changes which are to some extent ascertained. The first point which is to be noticed is that the shells of each sample, whether it be from a separate lake or only from a particular terrace, are more like to each other than to the shells of one of the other lakes, or to those of another terrace in the same lake as at Shumish Kul, where the shells of each terrace have a distinct appearance and character of their own, and may easily be known from the shells of higher or lower terraces.

The next feature of importance is the fact that, in the four independent cases—Shumish Kul, Jaksi Klich, Jaman Klich, and the Egyptian lagoon Abu Kir*—the shells which have lived under similar conditions, *i.e.*, in very salt water, have become like each other, having the characters of thinness, high colour, small beaks, ribbing on the inside, and great relative length. In view of these four instances of similar variations occurring under similar conditions, it seems almost certain that these conditions are in some way the cause of the variations. Similarly, in the case of the two groups of Cockles from Mareotis which have been isolated and exposed to fresh water in separate lakes, the result has been to produce a form of shell in both cases which is practically the same. Cases of this kind, in which it is possible to observe the appearance and progress of a variation through successive generations in the same place, are so rare that it has seemed worth while to describe these shells in detail. The mode of occurrence of the shells in terraces at Shumish Kul provides an almost unique opportunity for beholding the gradual succession of these changes. If, then, it is admitted that the structural changes in the shells are to be regarded as the consequence of the environmental changes in the water of the lake, the question arises to what extent these structural changes follow directly on the changed circumstances, and how far they may not be due to the natural selection of a different type as the fittest to live in the altered state.

Now, while the cases given above do not give a definite answer to this question, they nevertheless contribute something towards it. The chief qualities which appear in the shells which have been exposed to the increased saltness are comparative thinness, high colour, and increased length, together with diminished beaks. If, then, it is supposed that shells having these qualities were being gradually chosen by natural selection as the fittest for the new conditions, it would be expected that in each

* Amongst the shells in the Cambridge University Museum collected by MacAndrew are a few Cockles from a lagoon at Tunis, which show the same features. Though, in the absence of further information as to the locality from which they were brought, nothing can be positively stated, yet it is likely that they afford another instance of a similar variation occurring under similar conditions.

terrace these several attributes would be found in varying degree among the individual shells—that some would be thick and some thin, some long and some short, &c.; on the other hand, if the new qualities were the result of the new conditions, then it would be anticipated that the shells of each terrace would be all nearly similar in texture and shape. The more uniformly any of the new variations are found among the individuals, the more probable is it that they are due to the direct action of environmental change rather than to natural selection; but a new quality, which is found in the several individuals to a greatly varying degree, cannot be held to be shown to be the direct result of the conditions, even though it be found to be increasingly more marked on the average in successive generations as the conditions to which it is supposed to be due become more intense. Now the variations formed among these shells are of two kinds. The variation in proportional length, though becoming more and more marked in the shells which have been exposed to salter water, is not found in all the individuals (*vide* Tables); on the other hand, the variations in the quality, texture, and colour of the shell are found developed to nearly the same degree among all the individuals of successive terraces. Hence it may be fairly supposed, in the case of these latter variations, that they are really due to direct environmental change. The same also is true of the shells from the fresh-water localities, the texture and colour of which are practically uniform, while a good deal of variation is found in the shape, though the general prevalence of the long type is clear. In view of the manifest connection between variation in the texture, &c., of the shells and the conditions in the lake, it would be interesting to know more clearly the mode of action of these conditions in producing those effects, but as to this it is difficult to make a conjecture. No doubt they are the result of changes in the nutrition of the animals, but more than this does not seem clear. It can scarcely be supposed that the thinness of the shells was due to a deficiency of lime in solution in the water, since this would rather increase in relative amount with the evaporation. Moreover, the shells from the two fresh-water lakes at Ramleh are fairly thick. Neither can the deficiency in the amount of the shells be due to general starvation, since there is no diminution in absolute size at Shumish Kul, except in the case of the shells of the lowest level, which do appear generally ill-nourished; while, at Jaksi Klich, those shells which have become thin and papery from the desiccation of the lake are, on the whole, absolutely larger than an average sample of shells from the Aral Sea.

It may here be remarked that the striking similarity between the shells which had been exposed to very salt water at Abu Kir and those of the salt lakes of the Aral region has features of special interest, since not only have the similar conditions prevailed in producing two forms closely resembling each other, but this has been achieved, though the animals subjected to the influence were at first unlike and had had a very different history. For even supposing that the Cockles in the Aral Sea were originally derived from those of the Mediterranean, which is uncertain, yet the

Aral shells have been living for ages in water containing less than a third of the salt contained in Mediterranean water, and the Aral Cockle is quite sufficiently different from that of the Mediterranean to constitute a well marked variety. So that, while the Cockles originally isolated in Abu Kir came directly from the Mediterranean, the ancestors of those which were subjected to increased saltness at Jaksi Klich, &c., had been living in brackish water in the Aral Sea for an indefinite number of generations, *yet the resulting forms in both cases are closely alike.*

It is not well to press conclusions of this kind too far, and it may be that unfavourable conditions of some kind quite other than increased saltness may result in producing similar variations. All that can be stated with certainty is that shells exposed to increasingly salt water do change in a particular way, and that they do so with great regularity and uniformity. In the same way it has been shown that the influence of fresh water does not lead to the production of a peculiar type of shell. In the case of the variation consisting in increased proportion as to length, it is especially probable that the cause lies in the general unfavourableness of the conditions. It was shown to be present both in those shells which had been exposed to salt water and in a still greater degree among those which had been living in fresh water. It is not rare to find occasionally shells of Cockles from the English coast of similar shape. Nevertheless, the regularity of the presence of this feature among the shells from these abnormal situations is so great as to make it certain that this phenomenon is in some manner due to the conditions. Instances in which it is possible to actually trace the occurrence of variations are so rare that no apology is required for having given so much attention to details which would be otherwise unimportant. In the cases here given it has not only been possible to observe the variations, but also to obtain the actual ancestors of the varying offspring for comparison, and in the case of the shells of Shumish Kul an opportunity is given of doing this at several successive stages.

I wish to express my thanks to many persons who have assisted me in the course of my investigations and especially to Sir ROBERT MORIER, G.C.B., British Ambassador at St. Petersburgh, who obtained permission for me to travel in Central Asia; to M. SEMENOW, Vice-President of the Imperial Geographical Society, and to M. MAXIMOVITCH of the Botanic Garden, for much valuable information and advice; also to C. A. COOKSON, Esq., C.B., British Consul at Alexandria. Moreover, though this page may never reach them, I cannot let this opportunity pass without expressing my gratitude for the courtesy and hospitality which I everywhere met with at the hands of the Kirghiz people.

Tables Showing Variations in the Average Ratio of Length to Breadth in Shells from Different Localities.

Explanation.—In the first column of these Tables the actual measurements of the lengths and breadths of each shell are given in millimetres. The second column is constructed from the first. It shows the number of shells of each length which compose the sample of 30, and also shows the average breadth of shells having the same length. The third column is constructed from the second by dividing the average breadths in each case by the length. The final average ratio is obtained from the third column by multiplying these quantities by the number of shells from which they were derived, adding together these products and dividing the sum by 30.

[In the column in which the breadths are given the figures in brackets show the number of shells having the same breadth.]

Table I.—The Aral Sea.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
22	17·5, 17(4)	22	17·1	5	1 : 0·77
21·5	16	21·5	16	1	·74
21	16(4)	21	16	4	·75
20·5	16	20·5	16	1	·76
20	16·5, 16(4), 15·5(3), 15(4), 14	20	15·4	13	·77
19·5	15·5, 15, 14	19·5	15	1	·76
18·5	14, 13	19	14·8	3	·76
		18·5	13·5	2	·72

Average ratio of length to breadth in 30 shells varying in length between 22 mm. and 18·5 mm. is 1 : 0·761.

Table II.—Shumish Kul.

The First (Highest) Terrace.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
21	17, 16	21	16·5	2	1 : 0·77
20·5	16(2)	20·5	16	2	·78
20	16(5), 15·5, 15(2)	20	15·6	8	·78
19	15·5(2), 15(4)	19	15·1	6	·79
18·5	15·5, 15, 14·5, 14	18·5	14·7	4	·79
18	15(3), 14·5(2), 14(2)	18	14 5	7	·80
17·5	14·5, 14	17·5	14·2	2	·81
17	14(5), 13·5, 13	17	13·7	7	·81
16·5	14	16·5	14	1	·84
16	13	16	13	1	·81

Average ratio of length to breadth in 30 shells varying in length between 21 mm. and 16 mm. is 1 : 0·799.

The Second Terrace.

(*a*.) Shells Varying in Length between 21 mm. and 16 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
21	17(2), 16(2)	21	16·5	4	1 : 0·78
20·5	16	20·5	16	1	·78
20	16(2), 15·5, 15(3)	20	15·4	6	·77
19·5	16, 15·5, 15	19·5	15·5	3	·79
19	16, 15·5(3), 15, 14	19	15·2	6	·80
18·5	14	18·5	14	1	·76
18	15(2), 14 5, 14(3)	18	14·4	6	·80
17·5	14	17·5	14	1	·80
17	14	17	14	1	·82
		16	13	1	·81

Average ratio of length to breadth in 30 shells varying in length between 21 mm. and 16 mm. is 1 : 0·782.

(*b.*) Shells Varying in Length between 26 mm. and 20 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
26	20	26	20	1	1 : 0·76
25	19(2), 18·5	25	18·8	3	·75
24·5	19	24·5	19	1	·77
24	19, 18(2)	24	18·3	3	·77
23	17(2)	23	17	2	·73
22	18(4), 17·5(2), 17(2)	22	17·5	8	·79
21	17, 16	21	16·5	2	·78
20·5	16	20·5	16	1	·78
20	16, 15·5, 15(2)	20	15·3	4	·76
19·5	15·5, 15	19·5	15·2	2	·75
19	15·5(2), 15	19	15·3	3	·80

Average ratio of length to breadth in 30 shells varying in length between 26 mm. and 20 mm. is 1 : 0·770.

The Third Terrace.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
22	17, 16(4)	22	16·2	5	1 : 0·73
21·5	16·5, 16(4), 15	21·5	15·9	6	·74
21	16·5, 16(3), 15	21	15·9	5	·75
20	16(2), 15(3)	20	15·4	5	·77
19·5	15	19·5	15	1	·76
19	15(2), 14·5, 14(2)	19	14·5	5	·76
18	14·5, 14, 13	18	13·8	3	·76

Average ratio of length to breadth in 30 shells varying in length between 22 mm. and 16 mm. is 1 : 0·751.

The Fourth Terrace.

(*a.*) Shells Varying in Length between 21 mm. and 16 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
21	16·5, 16, 15·5, 15(2)	21	15·4	5	1 : 0·73
20·5	16·5, 16. 15(2)	20·5	15·6	4	·76
20	16, 15, 14·5, 14(3)	20	14·5	6	·72
19·5	15, 14(2)	19·5	14·3	3	·73
19	14, 13·5, 13	19	13·5	3	·71
18·5	13	18·5	13	1	·70
18	14, 13	18	13·5	2	·74
17·5	14, 13	17·5	13·5	2	·77
17	13	17	13	1	·76
16	12·5, 12, 11·5	16	12	3	·75

Average ratio of length to breadth in 30 shells varying in length between 16 mm. and 21 mm. is 1 : 0·735.

(*b.*) Shells Varying in Length between 26 mm. and 18 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
26	19	26	19	1	1 : 0·73
25	18	25	18	1	·71
23	18, 17·5, 16	23	17·1	3	·74
22·5	17	22·5	17	1	·75
21	16·5, 16, 15·5, 15(2)	21	15·4	5	·73
20·5	16·5, 16, 15(2)	20·5	15 6	4	·76
20	16, 15, 14·5, 14(3)	20	14·5	6	·72
19·5	15, 14(2)	19·5	14·3	3	·73
19	14, 13·5, 13	19	13·5	3	·71
18·5	13	18·5	13	1	·70
18	14, 13	18	13·5	2	·74

Average ratio of length to breadth in 30 shells varying in length between 26 mm. and 18 mm. is 1 : 0·730.

The Fifth Terrace.

(*a.*) SHELLS Varying in Length between 21 mm. and 16 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of Shells.	
21	17, 16·5, 16(2), 15·5	21	16·2	5	1 : 0·77
20·5	15	20·5	15	1	·73
20	15·5, 14	20	14·7	2	·73
19·5	14·5, 14	19·5	14·2	2	·73
19	15, 14(4), 13·5, 13	19	13·9	7	·73
18	14(2), 13·5	18	13·8	3	·73
17·5	13·5, 13	17·5	13·2	2	·75
17	13·5, 13(2), 12·5	17	13	4	·76
16	13, 12·5, 12(2)	16	12·3	4	·77

Average ratio of length to breadth in 30 shells varying in length between 21 mm. and 16 mm. is 1 : 0·743.

(*b.*) SHELLS Varying in Length between 27·5 mm. and 21 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
27·5	19	27·5	19·5	1	1 : 0·70
26	20, 19, 18	26	19	3	·73
25·5	17	25·5	17	1	·67
25	19·5, 19, 18·5, 18, 17·5	25	18·5	5	·74
24	19(2), 18·5, 18, 17·5	24	18·4	5	·75
23	17, 16·5(2), 16(2)	23	16·4	5	·71
22·5	17(2), 15·5	22·5	16·5	3	·73
22	17·5, 17, 16·5, 15	22	16·5	4	·75
21·5	16	21·5	16	1	·74
21	17, 16	21	16·5	2	·73

Average ratio of length to breadth in 30 shells varying in length between 27·5 mm. and 21 mm. is 1 : 0·731.

The Seventh Terrace.

SHELLS Varying in Length between 21 mm. and 16 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
21	15	21	15	1	1 : 0·71
20·5	15	20·5	15	1	·73
20	15(3), 14·5, 14(3)	20	14·5	1	·72
19·5	13·5	19·5	13·5	1	·69
19	14·5, 14(2), 13	19	13·8	4	·72
18·5	13	18·5	13	1	·70
18	14(2), 13·5(2), 12	18	13·4	5	·74
17·5	13, 12	17·5	12·5	2	·71
17	13(2), 12(4)	17	12·3	6	·72
16	13, 12	16	12·5	2	·78

Average ratio of length to breadth in 30 shells varying in length between 21 mm. and 16 mm. is 1 : 0·725.

TABLE III.—Jaksi Klich.

Shells of the Outer Deposits Varying in Length between 22 mm. and 17 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
22	16(3)	22	16	3	1 : 0·72
21	16(3), 15·5, 15(3)	21	15·7	7	·74
20	15(7), 14·5, 14	20	14·8	9	·74
19·5	14(2)	19·5	14	2	·71
19	15, 14	19	14·5	2	·76
18·5	14(2)	18·5	14	2	·75
18	14, 13(2)	18	13·3	3	·73
17·5	13·5	17·5	13·5	1	·77
17	14	17	14	1	·82

Average ratio of length to breadth in 30 shells varying in length between 22 mm. and 17 mm. is 1 : 0·740.

Shells of the Inner Deposit.

(*a.*) Shells Varying in Length between 25·5 mm. and 19 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
25·5	17·5, 17	25·5	17·2	2	1 : 0·73
25	17·5, 17, 16·5	25	17	3	·68
24·5	16(2)	24·5	16	2	·65
24	17, 16(2)	24	16·3	3	·67
23·5	16(2)	23·5	16	2	·68
23	15·5(2), 14·5	23	15·1	3	·65
22·5	16, 15·5, 15	22·5	15·5	3	·68
22	16(2), 15(4)	22	15·3	6	·69
21·5	15·5, 15(2)	21·5	15·1	3	·70
21	14·5	21	14·5	1	·69
20	13	20	13	1	·65
19	14	19	14	1	·73

Average ratio of length to breadth in 30 shells varying in length between 25·5 mm. and 19 mm. is 1 : 0·682.

(*b.*) Shells Varying in Length between 30 mm. and 25·5 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
30	20	30	20	1	1 : 0·66
29	20	29	20	1	·69
28	19, 17·5	28	18·2	2	·65
27·5	19·5, 19, 18	27·5	17·5	3	·63
27	19(3), 18(2), 17·5	27	18·4	6	·68
26·5	18(2), 17·5, 17	26·5	17·6	4	·67
26	18(3), 17 5, 17(3)	26	17·5	7	·67
25·5	18, 17·5(2), 17(3)	25·5	17·3	6	·67

Average ratio of length to breadth in 30 shells varying in length between 30 mm. and 25·5 mm. is 1 : 0·660.

TABLE IV.—Shells from the Bottom of Jaman Klich.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
24	17, 16·5	24	16·7	2	1 : 0·69
23	17, 16	23	16·5	2	·71
22	16(3)	22	16	3	·72
21·5	15·5	21·5	15·5	1	·72
21	15(2)	21	15	2	·71
20·5	15	20·5	15	1	·73
20	14(2)	20	14	2	·70
19	14·5, 14(2), 13·5(2)	19	13·9	5	·73
18·5	14(4), 13	18·5	13·8	5	·75
18	13·5, 13	18	13·2	2	·73
17·5	14, 13	17·5	13·5	2	·77
17	13, 12	17	12·5	2	·73
16	11·5	16	11·5	1	·71

Average ratio of length to breadth in 30 shells varying in length between 24 mm. and 16 mm. is 1 : 0·726.

TABLE V.—Shells from the Shore of Mareotis.

Shells Varying in Length between 27 mm. and 20 mm.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
27	19, 18	27	18·5	2	1 : 0·68
26	18	26	18	1	·69
25	18(2), 17·5, 17, 16(2)	25	17	6	·68
24·5	17	24·5	17	1	·69
24	16·5	24	16·5	1	·68
23	16(2), 15·5(2), 15(2)	23	15·5	6	·67
22·5	16(2), 15·5	22·5	15·8	3	·70
22	15·5, 15	22	15·2	2	·69
21·5	15	21·5	15	1	·67
21	15, 14(4)	21	14·2	5	·70
20	14(2)	20	14	2	

Average ratio of length to breadth in 30 shells varying in length between 27 mm. and 20 mm. is 1 : 0·680.

Table VI.—Ramleh Fresh-water Lake No. 2.

(*a.*) *Shells Varying in Length between* 21 *mm. and* 17 *mm.*

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
21	15, 14, 13·5	21	14·1	3	1 : 0·67
20	14, $13(^3)$, 12·5	20	13·1	5	·65
19·5	$13(^2)$	19·5	13	2	·66
19	13·5, $13(^4)$, $12·5(^3)$, $12(^2)$	19	12·7	10	·66
18	$13(^3)$, 12·5, 12, $11·5(^2)$	18	12·3	7	·68
17·5	$12(^2)$	17·5	12	2	·68
17	11·5	17	11·5	1	·67

Average ratio of length to breadth in 30 shells varying in length between 21 mm. and 17 mm. is 1 : 0·665.

(*b.*) *Shells Varying in Length between* 29 *mm. and* 16·5 *mm.*

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
29	17	29	17	1	1 : 0·58
28	19, 18	28	18·5	2	·66
27·5	18·5, $18(^2)$, 17	27·5	17·7	4	·64
26	19, 18·5, 17, 16·5	26	17·7	4	·68
25	17·5, 16	25	16·7	2	·67
24	$17(^2)$, 16·5, 15·5, 15	24	16·2	5	·67
23·5	15·5	23·5	15·5	1	·65
23	15, 14	23	14·5	2	·63
22	15·5	22	15·5	1	·70
21·5	15, 14	21·5	14·5	2	·67
20·5	13	20·5	13	1	·65
20	14, 12	20	13	2	·63
19·5	13	19·5	13	1	·66
19	12	19	12	1	·62
16·5	11·5	16·5	11·5	1	·69

Average ratio of length to breadth in 30 shells varying in length between 29 mm. and 16·5 mm. is 1 : 0·657.

TABLE VII.—Shells from the Shore of Abu Kir.

Measurements of shells in millimetres.		Average breadth of shells having the same length.			Ratio of length to breadth.
Length.	Breadth.	Length.	Average breadth.	Number of shells.	
24	17·5, 17	24	17·2	2	1 : 0·71
23	17, 16, 15·5	23	16	3	·69
22·5	17·5, 16·5, 15·5	22·5	16·5	3	·73
22	16, 15·5, 15	22	15·3	3	·69
21·5	15(5), 14	21·5	14·8	6	·68
21	16, 15·5, 15(2)	21	15·3	4	·72
20·5	15	20·5	15	1	·74
20	14·5, 14(5)	20	14	6	·70
19·5	14·5, 14	19	14·2	2	·75

Average ratio of length to breadth in 30 shells varying in length between 24 mm. and 19·5 mm. is 1 : 0·738.

TABLE VIII.

This Table gives the results of the previous Tables. The extremes of length of the shells measured for these averages are given in millimetres, and the average breadths are given in terms of the length, which is taken as 1.

Locality.	Level.	Smaller samples.		Larger samples.	
		Extremes of length.	Average breadth.	Extremes of length.	Average breadth.
Shore of Aral Sea	. .	22 –18·5	0·761		
Shumish Kul	First terrace . .	21 –17	0·799		
" "	Second terrace .	21 –17	0·782	26–19	0·770
" "	Third terrace. .	22 –18	0·751		
" "	Fourth terrace .	21 –16	0·735	26–18	0·730
" "	Fifth terrace . .	21 –16	0·743	27–21	0·731
" "	Seventh terrace .	21 –16	0·725		
Jaksi Klich	Upper deposit .	22 –17	0·740		
" "	Lower deposit .	25·5–19	0·682	30–25·5	0·660
Jaman Klich	Lower deposit .	24 –16	0·726		
Shore of Mareotis	. .	. .	. .	27–20	0·680
Ramleh Lake No. 2 (fresh-water).	. .	21 –17	0·665	29–16·5	0·657
Shore of Abu Kir	. .	24 –19·5	0·738		

Table Showing the Comparative Weights of Shells of Similar Size.

For the purpose of this Table twenty shells were chosen from each sample to be compared, as nearly alike in length as was possible.

The first column gives the name of the locality, the second the level, the third shows the extremes of length in millimetres of the shells selected, the fourth column gives the sum of the lengths of the twenty shells, and the fifth column gives the total weight.

Locality.	Level.	Extremes of length in millimetres.	Average length of 20 specimens.	Total weight in grammes of 20 specimens.
Shore of Aral Sea	..	21–17	19·2	13·3
Shumish Kul	First terrace	21–17	19·1	14·1
,, ,,	Second terrace	21–17	19·4	14·5
,, ,,	Fourth terrace	21–17	19·2	6·5
,, ,,	Fifth terrace	21–17	18·9	6·1
,, ,,	Seventh terrace	21–17	19·7	4·6
Shore of Abu Kir	..	21–17	19·0	6·4
Jaksi Klich	Upper deposit	23–19	20·4	7·8
,, ,,	Lower deposit	23–19	20·4	5·5
Jaman Klich	Lower deposit	21–17	19·2	5·1
Ancient shells exposed at Mandara	..	26–21	23·4	24·2
Shore of Mareotis	..	25–22	23·8	12·0
Ramleh Lake No. 1 (fresh-water)	..	25–20	21·4	18·3
Ramleh Lake No. 2 (fresh-water)	..	26–23	24·1	23·6

Explanation of Plate 26.

Figs. 1–7 represent shells of *Cardium edule* from the successive terraces at Shumish Kul.

Fig. 1. Right valve from the first (*i.e.*, the highest) terrace.
Fig. 2. Right valve from the second terrace.
Fig. 3. Left valve from the third terrace.
Fig. 4. Right valve from the fourth terrace.
Fig. 5. Right valve from the fifth terrace.
Fig. 6. Right valve from the sixth terrace.
Fig. 7*a*. Left valve from the seventh terrace, seen from the outside.
Fig. 7*b*. The same shell as fig. 7*a*, seen from the inside, showing the grooves.
Fig. 7*c*. Dorsal view of an individual from the seventh terrace, showing the reduced size of the beaks.

Fig. 8. Right valve from the lower deposit at Jaksi Klich.
Fig. 9. Right valve from the lower deposit at Jaman Klich.
Fig. 10. Left valve from the edge of the great lagoon at Abu Kir.
Fig. 11. Left valve from the western shore of Lake Mareotis.
Fig. 12. Left valve from the deposit of sub-fossil shells at Mandara.
Fig. 13. *Cardium edule* from the fresh-water lake at Ramleh (referred to in the text as Ramleh Lake No. 2), seen from its oral end.

All the figures were drawn by the Cambridge Scientific Instrument Company. They show the natural size of the shells and their colours as they appear when *wet*.

VIII. *On the Descending Degenerations which follow Lesions of the Gyrus Marginalis and Gyrus Fornicatus in Monkeys.*

By E. P. France. *With an Introduction by Professor* Schäfer, *F.R.S.*
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[Plates 27–29.]

INTRODUCTION.

The following paper contains the record of an investigation into the degenerations which follow lesions of the gyrus marginalis and gyrus fornicatus in Monkeys. The work has been carried on under my direction by Mr. France, with the aid of a grant from the Government Grant Fund, and represents part of a long investigation into the degenerations which follow artificially produced cerebral lesions, the material for which has been furnished by cases operated upon in conjunction respectively with Professor V. Horsley and Dr. Sanger Brown. These cases and the physiological results of the operations have already been published in the 'Philosophical Transactions.'* The experiments here dealt with, twelve in number, comprise only the lesions of the gyrus marginalis and gyrus fornicatus, and, with one exception (case 12), are taken from the series of experiments performed in conjunction with Mr. Horsley.†

Of the twelve cases, six were of removal, or attempted removal, of the gyrus marginalis, and six of removal, or attempted removal, of the gyrus fornicatus. But in only one or two instances was the lesion, as determined by *post-mortem* examination, exactly limited to the convolution which it was attempted to remove, for in most cases the adjacent gyrus was to a certain extent involved in the injury. This was especially the case when removal of the gyrus fornicatus had been attempted, on account of its deep situation, and the difficulty of getting at it without some manipulation of the superjacent gyrus. Nevertheless, the removal of one or the other

* V. Horsley and E. A. Schäfer, "A Record of Experiments on the Functions of the Cerebral Cortex," 'Phil. Trans,' B, 1888, pp. 1–45; and Sanger Brown and E. A. Schäfer, 'Phil. Trans.,' B, 1888, pp. 303–327.

† In an Appendix, which has been added subsequently, lesions of the external motor cortex are dealt with by Mr. France.

gyrus was sufficiently complete in all the cases here selected to produce characteristic symptoms and characteristic descending degenerations.

It may be remembered that the symptoms which were found (by HORSLEY and myself) to follow removal of one marginal gyrus indicated paralysis of the trunk muscles and of most of the leg muscles (especially the extensors) of the opposite side of the body; double marginal lesion causing a corresponding bilateral paralysis. This result was in conformity with our earlier experiments, which demonstrated that on electrical excitation of this gyrus movements of the trunk and opposite leg were chiefly produced. It showed conclusively that the gyrus marginalis is to be regarded as part of the so-called motor region of the brain as mapped out by FERRIER. It was, therefore, only to be expected that we should find descending degenerations along the course of the pyramidal tract, as is indeed seen to be the case if the following account of the degenerations and the photographic representations of sections through the spinal cord and medulla are referred to. The chief feature of interest in this part of the investigation is the localisation of the degeneration mainly to the postero-lateral part of the crossed pyramidal tract area of the cord.

With regard to the gyrus fornicatus, the results of electrical excitation were found by us to be negative, so far at least as any movements of muscles were concerned. And we also found that, except such slight paresis as might well have been accounted for by the unavoidable injury to the adjacent marginal convolution, even very extensive removal of the gyrus fornicatus was productive of no muscular paralysis. On the other hand, we obtained well marked deficiency in the general and tactile sensibility of the opposite side of the body, and concluded therefrom that this part of the limbic lobe was probably concerned with the reception of sensory impressions.

If the view which is usually held, viz., that in the central nervous system the direction of conduction and degeneration is the same, be correct, this conclusion of ours would have to be modified, in consideration of the extensive descending degenerations along the whole area of the pyramidal tract which are recorded by Mr. FRANCE as resulting from the lesions of the gyrus fornicatus, for it would be difficult, with these facts before us, to arrive at any conclusion other than that centrifugal nervous impulses emanate from the cells of this gyrus, and pass down along the course of the motor tract.

The question which would then arise is, What is the nature of these centrifugal impulses? The observations already referred to would seem to show that they do not pass to the skeletal muscles, and the idea suggests itself that they are of vaso-motor (? inhibitory) character. Such a supposition is not at variance with the hemi-anæsthetic results obtained on removal of the gyrus fornicatus, for these results might be explained by supposing contraction of cutaneous vessels (and consequent numbness) to result from the removal.

But are we bound to accept the above view regarding conduction and degeneration in the central nervous system? It by no means follows that we are. The only law of universal applicability (to both central and peripheral nervous system) regarding these phenomena, is the Wallerian, that degeneration supervenes in all nerve fibres which are cut off from their nutrient centres, which in all probability are the cells from which the fibres have originally grown out (HIS). The nutrient centres for the higher motor tracts lie in the grey matter of the cerebral cortex, and there is nothing intrinsically improbable in the supposition that those for the higher sensory tracts are also to be found there. To decide between these two explanations must be the object of future investigations. [E. A. SCHÄFER.]

ON THE DESCENDING DEGENERATIONS WHICH FOLLOW LESIONS OF THE GYRUS MARGINALIS AND GYRUS FORNICATUS.*

SUMMARY OF THE DEGENERATIONS WHICH FOLLOW MARGINAL LESIONS.

I have examined six cases where a lesion in the marginal convolution was produced. In two cases this injury was strictly confined to the marginal (cases 1 and 4), the gyrus fornicatus being quite uninjured.

In the remaining cases there was some injury to the adjacent external surface of the hemisphere, or to the gyrus fornicatus.

In the *internal capsule* the degeneration has been difficult to detect, and all that I can say, as far as this series is concerned, is that I have not seen degeneration in front of the knee, but have occasionally been able to make out scattered degenerated fibres in the posterior half.

In the *pons* the degeneration is easily seen, scattered, apparently indiscriminately, in the pyramidal bundles. If the cortical lesion had been extensive and the animal had lived for some time (ten weeks or more), a distinct difference in the size of the bundles on the two sides could be seen with the naked eye after hardening.

The *medulla oblongata* also shows degeneration in the pyramids. Here it is more concentrated than in the pons. A shrinking of the pyramid corresponding to the side on which the cortical lesion has been made similar to that in the pons can be seen with the naked eye if the animal has lived long enough for much sclerosis to have been set up (see fig. 2*b*, Plate 28).

In the *spinal cord* the degeneration is continued in the crossed pyramidal tract on the side opposite the lesion, and can be traced in this tract as far as the lower lumbar region.

I have not found degeneration in the direct pyramidal tract in any case. In all cases where the degeneration in the crossed pyramidal tract on the side opposite the lesion is well marked, degeneration in the crossed pyramidal tract on the *same side* as the lesion can also be seen (see fig. 2*d*), occupying a similar position to that on the

* The remainder of the paper is by Mr. E. P. FRANCE.

other side, but very much less in amount. This degeneration has been followed down the cord as far as the lumbar region in some instances.

The shape of the degeneration in the lateral column is sufficiently constant in all cases where there has been considerable injury to the marginal convolution for it to be considered characteristic of this lesion.

In the *cervical region* it is narrowly triangular or claw-shaped, the base being towards the posterior cornu, and the (expanded) apex at the surface of the cord, reaching this at about the middle of the lateral border (see fig. 1c.). One side of the triangle is formed by the line of separation between the crossed pyramidal tract and the direct cerebellar tract. Along this line the degeneration always appears to be most complete, that is to say, the degeneration is chiefly confined to the posterior and outer part of the crossed pyramidal tract; this is noticeable down the whole of the cord. In the cervical region the degeneration spreads out immediately within the circumference of the cord, the direct cerebellar tract appearing as if pushed backwards towards the apex of the posterior cornu, where it occupies an area which is triangular in section, in place of the oblong tract seen in sections lower down.

In the *upper dorsal region* the appearance is much the same as in the cervical region, except that the connection of the degeneration with the circumference of the cord is becoming less extensive, although the whole tract of degeneration has approached a little nearer the circumference, from which it is separated by the now oblong cerebellar tract.

In the *lower dorsal region* the degenerated tract has become less in amount and has approached quite close to the circumference, leaving only a narrow band of healthy fibres in the position of the direct cerebellar tract, and does not extend as far forward, although the apex of the triangle still touches the surface. There are a greater number of healthy fibres to be seen between the grey matter and the degeneration than in the cervical or upper dorsal regions.

In the *lumbar region* the degeneration is very much less marked than higher in the cord, and occupies the angle formed by the posterior root exit and the circumference. In cases where the lesion in the brain had been extensive the degeneration could be traced as far as the fifth lumbar nerve. I have not carried sections below this point.

Summary of Degenerations following Lesions of the Gyrus Fornicatus.

The brains and spinal cords of six Monkeys, in which a part or the whole of the gyrus fornicatus had been removed, were examined.

In most cases the *post-mortem* examination showed that the marginal gyrus had also been injured to a variable extent, owing, no doubt, to the difficulty experienced in exposing and excising a convolution so deeply situated as the gyrus fornicatus without interfering with the adjacent marginal gyrus.

In two instances the removal of the grey matter of the gyrus fornicatus was nearly complete (Nos. 10 and 11), and with scarcely any injury to the marginal. In these, as well as in all the other cases recorded, there is well marked and extensive degeneration in the crossed pyramidal tract.

In the brains of Nos. 9 and 12 there were lesions other than that of the gyrus fornicatus. In No. 9 a considerable lesion in the hippocampal and under surface of the occipital regions had been effected, and in No. 12 lesions of both temporal lobes.

But the degeneration which passes down into the spinal cord has in all the instances here investigated been confined to one tract—the lateral pyramidal tract—mainly on the side opposite the lesion, and appears to have been in every case produced by the lesion of the gyrus marginalis or of the gyrus fornicatus.* The region of the direct cerebellar tract is encroached upon in some instances (Nos. 2, 6, 7, and 8), but the extent, and even the occurrence, of this encroachment has not appeared to me to bear any constant relation to the amount of lesion of the gyrus fornicatus, occurring, for example, in a case where there was little injury to the gyrus fornicatus, and not occurring in other cases after much more complete removal, although the animals lived three months or more. I conclude, therefore, that the apparent invasion of the direct cerebellar tract is due to some individual variation in the course of the fibres of the crossed pyramidal tract, and not to any degeneration of fibres belonging to the direct cerebellar tract itself. I have not been able to make out with any certainty in my specimens the course of the degeneration in the internal capsule.

In the *mid-brain, pons*, and *medulla* (fig. 4*b*) the degeneration has the same appearances as that following marginal lesions, and is found only in the pyramidal bundles on the same side as the lesion.

In the *spinal cord* the degeneration occupies *the whole sectional area of the lateral pyramidal tract*, and in the cervical region is no longer confined to the part bordering on the direct cerebellar tract, as was observed with degenerations following purely marginal lesions (compare fig. 5*c* with fig. 2*d*).

In the *upper dorsal region* the extent and shape of the lesion is similar to that in the cervical region, but about the middle of the dorsal region it begins to diminish in extent, the outer and anterior part of the pyramidal tract assuming its normal appearance first.

At the *lumbar enlargement* it is very much lessened in proportion, but is still distinct, being confined chiefly to the angle formed by the posterior root exit and circumference, as with the marginal degeneration. The degeneration can usually be traced as far as the fifth lumbar nerve, beyond which point I have not carried sections.

Although following the course of the pyramidal tract by no means all the fibres of that tract are degenerated, many remaining normal.

* I have ascertained that lesions of the hippocampal region alone are not followed by any perceptible degenerative changes in the spinal cord.

Record of Cases Investigated, showing in each Case the Situation and Extent of the Cerebral Lesion, the Symptoms Observed during Life, and the Degenerations which were found to have Resulted from the Lesion.

Case 1.—*No.* 11 *of First Series.** (Figs. 1*a* to 1*e*, Plates 27 and 28.)

Lesion.—Removal of a longitudinal strip of grey matter from the left side of the brain, along the margin of the longitudinal fissure from the level of the anterior end of the præcentral nearly to the parieto-occipital fissure (fig. 1*a* in surface view, fig. 1*b* in section).

Result.—Some paralysis of both right limbs, which gradually became less evident in the arm; the leg paresis was permanently obvious. This Monkey was killed one year after the operation.

Degenerations Observed.

Pons.—There is a difference in the appearance of the two sides, the pyramidal bundles on the left side being stained with aniline blue-black more darkly than on the right, and appearing smaller.

Medulla.—Here the naked eye appearance of the pyramid of the left side is different from that of the right, being smaller and more deeply stained.

Microscopically, the left pyramid is considerably degenerated and sclerosed, although there are a great many healthy fibres scattered about in it.

Spinal Cord. Cervical Enlargement.—Sections here show degeneration, with sclerosis, in the crossed pyramidal tract on the right side (see fig. 1*c*), extending from the posterior root outwards and forwards till it reaches the circumference at about the middle of the lateral surface. The degeneration is claw-shaped, with the root of the claw at the posterior cornu, the convex side towards the direct cerebellar tract, and the tip at the surface of the cord.

The crossed pyramidal tract is not entirely degenerated, that part only which is adjacent to the direct cerebellar tract being affected, the anterior part having remained healthy. There is some degeneration on the same side as the lesion in a similar position to that on the opposite side.

No degeneration can be seen in the anterior median column on either side either in this case or in any other which has been examined by me.

Dorsal Region.—Here the degeneration is narrower than in the cervical region,

* The series of experiments recorded by Horsley and Schäfer (*loc. cit.*) will be referred to as the First Series; those recorded by Sanger Brown and Schäfer (*loc. cit.*) as the Second Series. Further illustrations of the extent and depth of the several lesions are to be found in the plates accompanying those papers.

and is nearer the circumference, leaving, however, a band of healthy fibres in the position of the direct cerebellar tract.

Degenerated fibres and slight sclerosis can be seen with the microscope on the same side (left) as the lesion in a similar position.

Lumbar Enlargement.—The degeneration here is less in amount. It occupies the angle between the posterior root and the circumference. A few degenerated fibres can be seen in a similar position on the same side as the lesion.

Case 2.—*No.* 12 *of First Series.* (Figs. 2*a* to 2*d*, Plates 27 and 28.)

Lesion.—Ablation of the posterior three-fourths of the left marginal convolution and an adjoining strip of the external surface as far as sulcus x (fig. 2*a*). The gyrus fornicatus was also somewhat injured.

Result.—Paralysis of the right side of the trunk and right leg, and partial paralysis of the right arm, which had imperfect power of extension from the shoulder.

This animal was killed six months after the operation.

Degenerations Observed.

The pieces from which sections were cut showed, even with the naked eye, well marked degeneration in the pyramidal bundles in the pons and medulla on the same side as the lesion, and in the spinal cord in the crossed pyramidal tracts of both sides, although much more obviously on the side opposite the lesion.

Medulla.—There is well marked, scattered degeneration in the left pyramid (fig. 2*b*), with considerable shrinking and sclerosis.

Spinal Cord.—*Cervical Region.*—There is a marked triangular patch of degeneration in the crossed pyramidal tract on the right side, denser where it borders the direct cerebellar tract, and reaching the circumference near the middle of the lateral border, where it spreads both backwards and forwards, but especially the latter, involving the outer part of the direct cerebellar tract.

In a section at the level of the second cervical nerves (fig. 2*c*) a band of degeneration is seen close to the circumference, extending from the posterior root exit to where the pyramidal tract degeneration reaches the circumference. On the left side there is a small amount of degeneration, scattered over a similar area, and on both sides there is sclerosis in the degenerated areas.

Dorsal Region.—At the level of the fourth nerves (fig. 2*d*) the degeneration is very well defined. It extends from the apex of the posterior cornu (right side) outwards and forwards as a diminishing strip, till it reaches the surface about the anterior end of the direct cerebellar tract. There is also a little degeneration along the external border of the direct cerebellar tract, which spreads along the circumference less than in the cervical region. On the left side there is some degeneration in a similar position to that on the right (see fig. 2*d*).

Eighth Dorsal.—The degeneration here (right side) has much the same position as at the level of the fourth dorsal, although rather less in amount: that is to say, there are more normal fibres in the degenerated patch. The patch is nearer the circumference than higher up. There is a little degeneration to be seen on the left side, having a similar position to that on the right.

Lumbar Region.—The degeneration in the crossed pyramidal tract on the right side comes quite to the circumference, and extends from the posterior root as a small patch outwards and forwards.

A few degenerated fibres can be seen in a similar position on the left side.

Case 3.—*No.* 14 *of First Series.*

Lesion.—Removal of the left gyrus marginalis for rather more than the posterior two-thirds, with a small amount of injury to the gyrus fornicatus (see 'Phil. Trans.,' B, 1888, Plate 2, fig. 14).

Result.—Paralysis of the leg and trunk muscles on the right side. The arm was slightly paralysed at first, but soon recovered, and for the first few days there was some loss of reaction to tactile impressions.

This animal died three and a-half weeks after the operation.

Degenerations Observed.

Internal Capsule.—No degeneration can be made out with sufficient distinctness.

Midbrain, Pons, and Medulla.—Scattered degeneration is to be seen in the pyramidal bundles of the left side.

Spinal Cord. Cervical Region.—Scattered degeneration can be seen in the crossed pyramidal tract on the right side, bordering the direct cerebellar tract and gradually approaching the circumference. The greatest amount of degeneration is towards the posterior cornu; it becomes less as it passes forwards and outwards.

The degeneration has a similar position to that seen in No. 2, although it is less distinct and smaller in amount.

A few degenerated fibres can be made out with a higher power (Zeiss, E) in a similar position on the left side.

Dorsal Region.—Degenerated fibres can be seen in a similar position to those in the cervical region, except that they are, on the whole, nearer the circumference.

A few degenerated fibres can be seen on the left side as far as the lower dorsal region.

Lumbar Enlargement.—Degeneration is still to be seen here, on the right side, in the angle between the posterior root and the circumference.

Case 4.—No. 19 of First Series.

Lesion.—Excision of both marginal convolutions: on the left side for the posterior three-fourths of its length, on the right side to a rather less extent.

Frontal sections through the brain show that that part of the gyrus bordering the calloso-marginal fissure, and dipping down into it, was but little injured; the upper border also was but little injured (see 'Phil. Trans.,' B, 1888, Plate 2, figs. 19R and 19L).

Result.—Almost complete paralysis of the leg and trunk muscles, but there is some ability to move, especially to flex, the legs, particularly the left leg.

This animal died on the ninth day after the operation.

Degenerations Observed.

No degeneration could be seen with the naked eye.

Microscopic. Pons.—A few degenerated fibres are to be seen in the pyramidal bundles of both sides.

Medulla.—Scattered degeneration can be seen in the pyramids on both sides, more distinctly than in the pons.

Spinal Cord. Cervical Enlargement.—Degenerated fibres can be seen on both sides scattered about the crossed pyramidal tracts, chiefly occupying the part which borders on the direct cerebellar tract, and extending outwards and forwards from the apex of the posterior cornu.

Dorsal Region.—The patch of degeneration is more concentrated, although smaller in extent. In the lower dorsal region especially it lies closer to the circumference of the cord. It is more marked on the right than on the left side.

Lumbar Region.—In the lumbar region the degeneration can still be seen on both sides, occupying the angles between the posterior root and the circumference.

The degeneration consists of swollen fibres with axis cylinders in various stages of breaking down.

Case 5.—No. 21 of First Series.

Lesion.—Removal of the posterior three-fourths of both marginal gyri at one operation. Frontal sections through the brain show that the removal was complete except along the calloso-marginal fissure (see 'Phil. Trans.,' B, 1888, Plate 3, figs. 21A to 21D).

Result.—Paralysis of the trunk and legs.

This animal died on the twenty-seventh day after the operation.

Degenerations Observed.

There was a slightly marked appearance of degeneration to be seen with the naked eye in both crossed pyramidal tracts all down the cord.

Microscopic. Pons.—Scattered degeneration can be seen in the pyramidal bundles of both sides.

Medulla.—Scattered degeneration can be seen in both anterior pyramids.

Spinal Cord. Cervical Region.—The degeneration occupies that part of the crossed pyramidal tracts which borders on the direct cerebellar tract, and extends from the posterior cornua towards the circumference on both sides. It is not very well marked, but occupies a similar position to that of all the other marginal cases.

Dorsal Region.—In the dorsal region it is relatively less in amount, closer to the circumference, and does not extend as far forwards.

Lumbar Enlargement.—It is much less in amount here, and lies in the angles formed by the posterior cornua and the circumference of the cord.

Case 6.—*No.* 22 *of First Series.*

Lesion.—Removal of both marginal convolutions at two operations.

Frontal sections through the brain show that the convolutions were completely removed, except a small strip of grey matter at the deepest part of the calloso-marginal fissure on the left side. The adjoining external surface was also injured.

Result.—The first operation on the left side produced the usual paralysis of the opposite hind limb and of the trunk. The second operation (right side) produced paralysis of the trunk and legs of the opposite side, except that the knee and hip can be feebly flexed.

The animal died three months after the first operation.

Degenerations Observed.

Very well marked degeneration is visible to the naked eye on both sides in the crossed pyramidal tracts.

Pons and Medulla.—Under the microscope sections show scattered degeneration in the pyramidal bundles and pyramids.

Spinal Cord. Cervical Enlargement.—There is degeneration on both sides, extending over a large portion of the crossed pyramidal tracts, and involving part of the region of the direct cerebellar tracts.

The degeneration in the crossed pyramidal tracts is like that observed in the spinal cords of the other animals after similar lesions, although more extensive than in most cases. The region of the direct cerebellar tract* is also greatly involved on both sides. A small triangular patch of healthy fibres represents this tract, close to the posterior root exit.

Dorsal Region.—The degeneration is well marked and rather more defined than in the cervical region. The parts in the direct cerebellar tracts noticed to be free from degeneration in the cervical region are still seen, and appear rather larger.

* Probably the portion of the pyramidal tract which encroaches on the cerebellar tract.

Here the sclerosis has the shape and position characteristic of degeneration following marginal lesions, being claw or wedge shaped, with the base towards the posterior cornu, and the apex extending forwards and outwards until it reaches the circumference about the middle of the lateral column, where it spreads out, and joins posteriorly the degeneration which occupies the superficial part of the direct cerebellar tract region. The degeneration is relatively less in amount than in the cervical region.

Lumbar Enlargement.—The degeneration is small in amount, and lies on each side close to the circumference of the sections, in the angle formed by the posterior root.

Case 7.—No. 36 of First Series.

Lesion.—Removal of a considerable part of the left gyrus fornicatus. The marginal convolution was found to be injured in the greater part of its extent ('Phil. Trans.,' B, 1888, Plate 6, fig. 36).

Result.—The whole of the right side of the body, as far as the iliac crest, was almost completely insensible to touch, prick of a pin, and to a jet of cold water suddenly applied. There was loss of sensibility over the right arm. The right leg, although not anæsthetic, was far less sensitive than the left. The arm, leg, and trunk are paresed, although they are still used. There was incomplete recovery from the paresis.

The animal died seven weeks after the operation.

Degenerations Observed.

Midbrain and Pons.—Extensive scattered degeneration can be seen in the pyramidal bundles of the left side.

Medulla.—The degeneration in the pyramids is very extensive on the left side, more so than in simple marginal lesions.

Spinal Cord. Cervical Enlargement.—On the right side there is extensive scattered degeneration in the crossed pyramidal tract, involving the greater part of the area occupied by this tract. The degeneration forms a broad triangular patch, extending from the posterior cornu outwards and forwards, reaching the circumference a little behind the middle of the lateral surface (corresponding with the area of the crossed pyramidal tract).

From the postero-lateral groove there extends, close to the circumference, a narrow band of degeneration in the outer part of the direct cerebellar tract region, which joins, as it passes forwards, the anterior and external end of the degeneration in the crossed pyramidal tract; so that this latter degeneration and that extending along the circumference enclose between them a band of healthy fibres of the direct cerebellar tract.

There are a few degenerated fibres to be seen in a similar position on the opposite side of the cord (left).

Dorsal Region.—The degeneration here is more triangular than in cases of simple marginal lesion, and more extensive. It does not reach quite so far forward as in the cervical region, but is nearer the circumference of the cord; so that in this part (middle dorsal) there is only a narrow and irregular band of healthy fibres left in the position of the direct cerebellar tract. The degeneration seen in the cervical region, extending along the circumference of the cord, here disappears.

Lumbar Enlargement.—The degeneration is confined to the angle formed by the posterior cornu and the circumference; it is much smaller than in the dorsal region, but more extensive than with simple marginal lesions.

Case 8.—*No.* 37 *of First Series.* (Figs. 3*a*, Plate 27, and 3*b*, Plate 28. See also 'Phil. Trans.,' B, 1888, Plate 6, figs. 37B to 37E.)

Lesion 1.—The anterior part of the left gyrus fornicatus was removed.

Result.—The external ear of the opposite side gave no reaction to tactile impressions producing pain elsewhere; it could not be determined whether any other parts were completely insensible.

Lesion 2.—A week after the first operation, the greater part of the remainder of the convolution was cut away (see fig. 3*a*).

Frontal sections through the marginal convolution and gyrus fornicatus show that that part of the gyrus fornicatus which borders on the corpus callosum has remained almost uninjured, whilst, on the other hand, that part of the marginal convolution which borders on the calloso-marginal fissure is injured in two places (anteriorly and posteriorly).

Result.—Great diminution of sensibility over the right side; tactile impressions produced no reaction; painful impressions were slowly perceived, and not localised. This "allochiria" began to be exhibited about a week after the second operation; it afterwards disappeared. No paresis was observed.

The animal was killed three months after the first operation.

Degenerations Observed.

Degeneration is distinctly seen with the naked eye in the pons and medulla (pyramid) on the left side, and in the spinal cord, as far as the lower lumbar region in the crossed pyramidal tract, on the right side, involving also the direct cerebellar tract on the same side to a considerable extent.

Midbrain and Pons.—In the pyramidal bundles of the left side scattered degeneration is visible with the microscope; it is not so great in amount as in the pons of No. 6, but, on the other hand, the cerebral lesion in this case is not so extensive.

Medulla.—Scattered degeneration is distinctly to be seen in the left pyramid. There is well-marked sclerosis, besides degenerated nerve fibres.

Spinal Cord. Cervical Region.—There is degeneration in the crossed pyramidal tract on the right side, similar in shape and position to that seen after lesions of the motor areas, being triangular in shape, with a broad base towards the posterior cornu, but becoming narrower as it extends forwards and outwards till it reaches the circumference, about the middle of the lateral surface. Here it spreads out a little, joining a tract of degeneration which passes forwards in the direct cerebellar tract close to the circumference, from near the posterior root. There is a band of healthy fibres belonging to the cerebellar tract, between the degeneration at the surface of the direct cerebellar tract and that in the main part of the crossed pyramidal tract.

Dorsal Region (fig. 3*b*).—The degeneration comes nearer to the circumference, leaving a narrower band of healthy fibres in the position of the direct cerebellar tract. It extends over the whole area of the pyramidal tract.

Lumbar Enlargement.—The degeneration is considerably less on the right side, and on the left there is none to be seen. It extends from near the posterior root exit a short distance forwards close to the circumference, and lies chiefly in the angle formed by the posterior cornu and the circumference, although it does not come quite close to the posterior cornu.

Fourth Lumbar Nerve.—A small amount of degeneration can still be seen on the right side, in a similar position to that noted above, but not quite so near the root exit.

Case 9.—*No.* 39 *of First Series.*

Lesion 1.—Incomplete removal of the gyrus fornicatus and considerable injury to the middle of the marginal convolution (see 'Phil. Trans.,' B, 1888, Plate 7, fig. 39).

Lesion 2.—Removal of that part of the limbic lobe which bends round the splenium of the corpus callosum.

Lesion 3.—A fortnight later the posterior part of the hippocampal convolution was scooped away.

Result.—Anæsthesia, and distinct muscular paresis of both right limbs.

This animal was killed about five months after the first operation.

Degenerations Observed.

Degeneration could be seen with the naked eye in the pyramidal bundles in the midbrain, pons, and medulla. After hardening, these parts look paler and smaller than those of the opposite side.

In the spinal cord a patch of degeneration is to be seen occupying the crossed pyramidal tract as far down as the third lumbar nerve.

Microscopically, degeneration can be seen, scattered in the pyramidal bundles of the *midbrain* and *pons* of the left side.

The *medulla oblongata* shows considerable degeneration and sclerosis (apparently more than in the pons) in the left pyramid.

Spinal Cord. Cervical Region.—There is a patch of degeneration in the crossed pyramidal tract on the right side, triangular in shape, extending from the cornu outwards and forwards till it reaches the surface near the middle of the lateral border. The degeneration is most marked in that part which borders the direct cerebellar tract; there are many degenerated fibres and considerable sclerosis. The direct cerebellar tract is encroached upon at its anterior end by the crossed pyramidal tract degeneration, which spreads out, backwards and forwards, a little way along the circumference of the cord.

Dorsal Region (fourth dorsal nerve).—The lateral tract degeneration comes nearer the circumference of the cord, and is altogether situated more posteriorly than in the cervical region.

The degeneration does not invade the direct cerebellar tract in this region.

On the opposite side there are a few degenerated fibres and some sclerosis in a corresponding area.

Eighth Dorsal Nerve.—The degeneration here is less in amount than at the fourth dorsal; it occupies a similar position, except that it comes altogether to the surface.

Lumbar Enlargement.—The degeneration here is small in amount and lies in the angle formed by the circumference of the cord and the tip of the posterior cornu. Degeneration can still be seen on the other side.

Case 10.—*No.* 40 *of First Series.* (Figs. 4*a*, Plate 27, 4*b* and 4*c*, Plate 28. See also 'Phil. Trans.,' B, 1888, Plate 7, fig. 40B.)

Lesion.—Removal of the right gyrus fornicatus, with injury to the marginal gyrus (fig. 4*a*, Plate 27).

Frontal sections through the brain show that the gyrus fornicatus was completely removed, and that the lower border of the marginal, especially at the front, was much undermined, and thus partly cut off from the corona radiata. There were also one or two small patches of softening on the external surface.

Result.—There was, at first, some paresis of the limbs and of the facial muscles on the left side.

For ten days there was entire loss of reaction to tactile and painful impressions on the left side. There was gradual, but only partial, recovery of sensation.

The animal died ten weeks after the operation.

Degenerations Observed.

Degeneration and shrinking could be seen with the naked eye in the pyramidal bundles of the pons and medulla on the right side, and in the crossed pyramidal tract on the left side, down the whole length of the cord.

Pons.—Microscopically there is distinct scattered degeneration in the pyramidal bundles of the pons.

Medulla.—The right pyramid shows extensive degeneration and sclerosis (fig. 4*b*).

Spinal Cord. Cervical Enlargement.—There is a well defined patch of degeneration involving the whole crossed pyramidal tract on the left side, triangular in shape, with the base towards the posterior cornu and the apex at the middle of the lateral surface of the cord. The apex spreads out along the circumference posteriorly, and encroaches for a short distance upon the cerebellar tract. The degeneration extends far forwards, as in most of the other similar lesions; the mesial part of the degeneration (the part towards the grey matter) is less concentrated than the part bordering the direct cerebellar tract. There is a little sclerosis.

On the right side (side of the lesion) there is a small amount of degeneration and sclerosis, scattered over a similar area.

Dorsal Region (fig. 4*c*).—The degeneration occupies the whole area of the crossed pyramidal tract, and appears even to extend in advance of that tract.

It is more concentrated than higher up, as is the case in all dorsal sections. In the lower dorsal region the degeneration has much the same appearance, except that it is altogether nearer the circumference of the cord, and does not extend as far forwards.

There is a very small amount of degeneration in a similar position on the opposite side.

Lumbar Enlargement.—The degeneration here is irregularly oblong in shape, and extends from the postero-lateral groove along the circumference for about one-fourth the distance from that groove to the anterior median fissure.

It does not touch the posterior cornu, except near the root exit.

No degeneration can be seen on the right side (side of the lesion).

Case 11.—*No.* 42 *of First Series.* (Figs. 6*a*, 6*b*, and 6*c*.)

Lesion 1.—Removal of the anterior two-thirds of the left gyrus fornicatus (see fig. 6*a*, Plate 27). Frontal sections through the brain show that only that part of the gyrus fornicatus which is seen on the mesial aspect was completely removed, those portions of the gyrus which lie next the corpus callosum and at the bottom of the calloso-marginal fissure having remained practically intact.*

Result.—Great diminution of sensibility over the right side of the body, which gradually, but only partially, passed off. Some muscular paresis on the right side, especially of the leg.

Lesion 2.—Eleven weeks after the operation, the right gyrus was exposed and injured by scratching with a needle. The permanent injury caused by the needle wa so slight as hardly to be perceptible. No result was observed during life.

The animal was killed thirteen weeks after the first operation.

* It is especially to be noted that in this case there was no perceptible injury of any region, excitation of which has been proved to produce muscular movements.

Degenerations Observed.

Degeneration could be seen *with the naked eye* in the crossed pyramidal tracts on the right side, in the whole length of the spinal cord.

Internal Capsule.—There are many degenerated fibres to be seen with the microscope, in a horizontal section, scattered about posterior to the knee.

(The *midbrain, pons,* and *medulla* of this brain were lost.)

Spinal Cord. Cervical Enlargement (fig. 6*b*, Plate 29).—Scattered degeneration, with considerable sclerosis, can be seen in the crossed pyramidal tract on the right side, having a shape like that seen in the other cases recorded; it is triangular, with the base at the posterior cornu, and extends outwards and forwards, becoming narrower till it reaches the surface, where it tends to spread out.

On the left side there is a little sclerosis, scattered over a similar area. There are also some recently degenerated fibres in the same area, the result, no doubt, of the needle operation on the right gyrus fornicatus.

Dorsal Region.—At the level of the fourth dorsal nerve (fig. 6*c*) the degeneration on the right side does not extend as far forward as in the cervical region, but it covers a relatively greater area.

On the left side there is also some degeneration, but the difference on the two sides is very marked.

At the level of the *eighth dorsal nerve* the degeneration is less in amount on both sides; it lies close to the surface of the cord, and does not extend as far forwards as at the level of the fourth dorsal.

Lumbar Region.—The degeneration in the crossed pyramidal tract is close to the surface, and lies in the angle formed by the posterior cornu and circumference of the cord. It is much less in amount on the right side than in the dorsal region, and on the left can hardly be seen.

Case 12.* (Figs. 5*a*, 5*b*, and 5*c*.)

Lesion.—Removal of the right gyrus fornicatus (fig. 5*a*, Plate 27), with some injury to the marginal convolution.

Result.—Anæsthesia over the whole left side of the body, partial in some places, complete in others. The left forearm retained its sensibility; the rest of the arm, the trunk, and the leg had very little, if any, sensibility. This condition continued for seven weeks, with slight and gradual improvement. The leg was slightly paresed.

At subsequent operations the greater part of the temporal lobe was removed on the right side, and the superior temporal gyrus on the left (see SANGER BROWN and SCHÄFER, 'Phil. Trans.,' B, 1888, p. 312, Monkey, No. VII.).

The animal was killed more than eight months after the first operation.

* This case of lesion of the gyrus fornicatus is mentioned in a paper by Professor SCHÄFER ("On Sensory Localisations in the Cerebral Cortex") in 'Brain,' April, 1888.

Post mortem, it was found that the middle part of the gyrus fornicatus was destroyed, and that the corresponding part of the marginal convolution was somewhat injured and depressed (fig. 5*a*). Besides the lesion of the temporal lobes above mentioned, the surface of the brain just above and in front of the Sylvian fissure was also found to be slightly injured.

Degenerations Observed.

In the midbrain, pons, and medulla the shrinking of the pyramidal bundles on the right side, as compared with those of the left, is very distinctly visible to the naked eye. They are also more deeply stained with aniline blue-black on the right than on the left side.

Microscopically, a number of degenerated fibres can be seen on the right side. In the medulla there is also some sclerosis.

Spinal Cord.—Level of second cervical nerve. Degeneration could be seen, even with the naked eye, in the lateral pyramidal tract on the left side (fig. 5*b*). There is a little degeneration in the pyramidal tract on the same side as the lesion (right), although less than after an extensive marginal or other motor lesion.

Level of sixth cervical nerve.—The degeneration has much the same appearance here as at the second cervical, except that it is rather better defined.

Level of fifth dorsal nerve.—The patch of degeneration is smaller than in the cervical region, but denser; it lies somewhat closer to the surface, especially nearer the posterior root exit (fig. 5*c*).

At the level of the eighth dorsal the patch is perceptibly smaller than at the fifth.

Lumbar Region.—At the level of the first nerve the degeneration is less extensive. It lies along the circumference of the cord near the apex of the posterior cornu.

At the level of the fifth lumbar the degeneration can still be made out without difficulty.

That the degeneration in this case had nothing to do with the lesions in the temporal lobes has been ascertained by carefully investigating other cords in which there were lesions in those lobes only.

Methods Employed in Preparing the Tissues for Microscopic Examination.

The brain and spinal cord of each animal, immediately after removal from the body, were placed in Müller's fluid, or bichromate of potash (2 per cent. solution), and hardened in this for not less than a month, the fluid being repeatedly changed. After hardening, they were first rinsed with water, or placed directly into methylated spirit without washing, and allowed to remain in this until cut up.

For staining by Weigert's or Pal's method, I have obtained better results if the brain has not been allowed to remain in spirit for more than a week, although the length of time it has been kept in spirit does not seem to affect the staining with carmine or aniline blue-black.

For cutting sections the freezing method has chiefly been used. I have employed the celloidin method for particular purposes, but the wrinkling which occurs on passing large sections through xylol has been found a drawback to the use of this method.

The following have been used for staining the sections, viz., aniline blue-black,* lithium-carmine, WEIGERT's and PAL's processes. Aniline blue-black has given, on the whole, the best results in my hands, for I have been able to detect early degeneration better than by either WEIGERT's or PAL's methods, and sclerosis equally well. It is also better for photographic purposes than any of the others, except PAL's.

I have found it better to stain individual sections than to stain in bulk, and, if the sections are passed through acidulated water after staining with aniline blue-black, the colour and differentiation are improved.

Lithium-carmine† has given good results when used to stain in bulk, the piece being kept in the solution for not less than a month. An objection to this stain is that it is trying to the eyesight to examine a long series of sections.

* Aniline blue-black . . .	2 grm.
Methylated alcohol . . .	60 c.c.
Distilled water	40 c.c.

† Lithium carbonate (sat. sol.)	100 c.c.
Carmine	5 grm.

Appendix.

Received April 22,—Read May 16, 1889.

On the Degenerations which follow the Removal of the External Motor Cortex, and of the whole Motor Cortex of One Hemisphere in Monkeys as compared with those which follow Lesions of the Gyrus Marginalis alone.

Since sending in the foregoing paper I have investigated the following cases :—

A.—Three of removal of the external motor surface of the brain in which the animals lived for a considerable time (from two to four months) after the operation.* The lesions were so much alike, and the resulting degenerations are so similar, that it would be superfluous to describe each case separately.

Sections were cut through the brain obliquely downwards and forwards perpendicular to the fibres of the crusta, and transverse sections of the pons, medulla, and of the three regions of the spinal cord were made.

Degenerations Observed.

In the *internal capsule* the degeneration is well-marked and constant, occupying the side next the lenticular nucleus. The inner side next the thalamus is almost entirely normal. The degeneration is greatest in amount about the angle, extending over the middle third.

Crusta.—The degeneration occupies the middle third and is clearly defined, the dorsal part being less completely degenerated than the ventral.

Pons.—The degeneration extends over the whole pyramidal bundles of the same side, almost all the fibres being degenerated.

Medulla.—The whole pyramid on the same side is degenerated, except a narrow portion towards the posterior and mesial border, which is less degenerated than the rest of the tract.

Spinal cord.—In the *upper cervical region* the whole extent of the crossed pyramidal tract is degenerated, that part bordering the direct cerebellar tract less completely than the rest. The degeneration extends along the circumference, backwards towards the posterior root exit, as a gradually narrowing band enclosing the direct cerebellar tract, which has an elongated wedge shape.

In the *dorsal region* the degenerated tract is entirely separated from the circumference by the direct cerebellar tract. It can be followed down the cord as far as the *lower lumbar region* where it is seen as a few degenerated fibres close to the circumference and a little external to the posterior root exit.

* For an account of the symptoms observed during life see "A Record of Experiments upon the Functions of the Cerebral Cortex," Cases 4, 5, and 8, by Professors Schäfer and Horsley, 'Phil. Trans.,' B, Vol. 179 (1888), pp. 1–45.

B.—Four cases of removal of the entire motor surface from one hemisphere in monkeys.

The symptoms observed during life, and the degeneration found after death were almost exactly alike in all these cases; but in the two which only lived one week the degeneration was not so easy to follow out as in the other two.

These animals lived for periods varying from one week to five months after the operation.*

Degenerations Observed.

In the internal capsule the degeneration is similar to that following lesions of the external motor cortex, but in addition to this, the inner side of the capsule is degenerated.

In the *crusta* and *pons* but little difference can be made out, the degeneration in the pyramidal tract being, perhaps, somewhat more complete.

In the *medulla* the pyramid of the same side is entirely degenerated.

In the *spinal cord* the whole crossed pyramidal tract is degenerated, the part bordering on the direct cerebellar tract as completely as the rest. The degeneration gradually diminishes as it descends, but can still be seen at the level of the 3rd and 4th lumbar nerves.

Summary of Results obtained by the Study of Degeneration following Lesions of the Motor Cortical Area.

Removal of the grey matter of the motor area of the brain, exclusive of the marginal gyrus, produces degeneration which becomes collected in the internal capsule, and thence downwards follows a regular and definite course as has already been abundantly shown by previous observers.

Diagram 1.

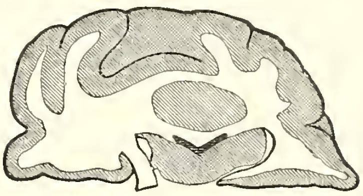
Horizontal section through one hemisphere showing the position of degeneration in the internal capsule following removal of the *external* motor surface (exclusive of gyrus marginalis).

In the *internal capsule*, as seen in a horizontal section, it occupies the middle third extending farther behind the knee than in front (diagr. 1), but quite distinct for a short distance in front of the knee. The degeneration does not involve the whole breadth of the internal capsule, but leaves the inner border almost entirely free.

* For an account of the symptoms observed during life and the exact extent of cerebral surface removed, see "A Record of Experiments upon the Functions of the Cerebral Cortex," *loc. cit.*, Cases 15, 16, 17, and 18.

The part of the capsule along the inner border is occupied by the fibres from the gyrus marginalis (diagr. 2), as is proved by the fact that when the entire motor area (including the gyrus marginalis) is removed, the whole width of the internal capsule is degenerated (diagr. 3), although the inner border never appears so completely degenerated as the outer.

Diagram 2.

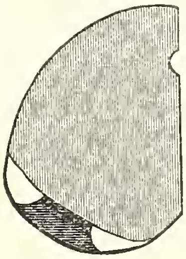


Horizontal section through one hemisphere showing the position of degeneration following removal of the gyrus marginalis.

Diagram 3.

Frontal section through brain, showing diagrammatically the course, through internal capsule, taken by degeneration following lesions in both the outer and mesial motor surface.*

Diagram 4.



Diagrammatic representation of the degeneration in the crusta.

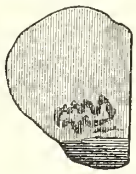
In the crusta, the degeneration occupies the middle third (diagr. 4), and little difference in appearance can be seen between that following an external motor (exclusive of gyrus marginalis), and that following a complete motor lesion; except that after removal of the external motor area only, the degeneration is not so great along the dorsal as along the ventral border of the crusta.

In the medulla the whole area of the pyramid is degenerated in those cases where

* The course of the fibres from the mesial surface is represented by darker shading than that of those from the external motor surface.

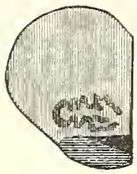
the whole motor area was removed; whilst in those where only the external motor area was involved a part along the posterior mesial border appears less completely degenerated than the rest (diagrams 5 and 6).

Diagram 5.



Degeneration in the pyramid of the medulla following lesions of the external motor area.

Diagram 6.



Degeneration following lesions of the entire motor area.

In the spinal cord the degeneration occupies the whole extent of the crossed pyramidal tract in those cases where the whole motor area was removed; but where only the external motor area was involved, that part of the pyramidal tract bordering on the direct cerebellar tract is less degenerated, though never quite free from degeneration.

This fits in with what I have said of the degeneration following marginal lesions, which chiefly occupies the part bordering on the direct cerebellar tract.

In the cervical region, the degeneration besides occupying a position corresponding to that seen in the dorsal region, also extends as a narrow band along the circumference towards the posterior root exit (diagr. 7), but there is always a well-marked tract of healthy fibres (direct cerebellar tract) close to the posterior root exit, extending into and separating the degeneration in the crossed pyramidal tract proper from the band extending along the circumference.

Diagram 7.

The degeneration as seen in the upper cervical region showing the direct cerebellar tract enclosed by degenerated crossed pyramidal tract.

Diagram 8.

Showing the degeneration at the level of the sixth dorsal nerve.

In the dorsal (diagr. 8) and lumbar regions no constant difference in shape and position can be made out between the degeneration following removal of the external motor surface, and removal of the entire motor area.

It gradually diminishes as it descends, but can be seen in the lumbar region (at the level of the 3rd or 4th nerve) as an oval patch close to the circumference and near the posterior root exit.

I have never in any case in the Monkey observed degeneration in the anterior columns of the spinal cord, and conclude, therefore, that in these animals the pyramidal decussation in the medulla oblongata is complete.

Description of Plates 27–29.

Case 1 (Figs. 1*a* to 1*e*).

Fig. 1*a* (Plate 27).* View of the brain showing the extent of the lesion.
Fig. 1*b* (Plate 28). Transverse vertical section through the brain showing the depth of the lesion.
Fig. 1*c* (Plate 28). Transverse section of the spinal cord in the cervical region.
Fig. 1*d* (Plate 28). Transverse section of the spinal cord in the middle dorsal region.
Fig. 1*e* (Plate 28). Transverse section of the spinal cord at the lumbar enlargement.

Case 2 (Figs. 2*a* to 2*d*).

Fig. 2*a* (Plate 27).* View of the brain showing lesion.
Fig. 2*b* (Plate 28). Transverse section of the medulla.
Fig. 2*c* (Plate 28). Transverse section of the cord at the level of the second cervical nerve.
Fig. 2*d* (Plate 28). Transverse section at the level of the fourth dorsal nerve.

Case 8 (Figs. 3*a* and 3*b*).

Fig. 3*a* (Plate 27).* View of the left side of the brain showing the lesion in the gyrus fornicatus.
Fig. 3*b* (Plate 28). Transverse section of the spinal cord in the cervical region.

Case 10 (Figs. 4*a* to 4*c*).

Fig. 4*a* (Plate 27).* View of the right side of the brain showing the lesion in the gyrus fornicatus.
Fig. 4*b* (Plate 28). Transverse section of the upper part of the medulla.
Fig. 4*c* (Plate 28). Section of the dorsal cord.

Case 12 (Figs. 5*a* to 5*c*).

Fig. 5*a* (Plate 27). View of the left side of the brain showing the lesion in the gyrus fornicatus.
Fig. 5*b* (Plate 29). Transverse section of the spinal cord in the cervical region.
Fig. 5*c* (Plate 29). Transverse section from the middle dorsal region of the cord.

* Figures marked with an asterisk are taken from the paper by Horsley and Schäfer. The others are from photographs.

Case 11 (Figs. 6*a*, 6*b*, 6*c*).

Fig. 6*a* (Plate 27). View of mesial surface of left hemisphere showing lesion in gyrus fornicatus.

Fig. 6*b* (Plate 29). Cervical cord showing degeneration in right pyramidal tract.

Fig. 6*c* (Plate 29). Dorsal cord.

Fig. 7 (Plate 29). Transverse section from the upper dorsal region of the spinal cord of a Monkey, in which nearly the whole of the motor cortex of one side of the brain was removed some months previously.

Figs. 8, 9, and 10 (Plate 29). Photographs of spinal cord degeneration under a high power.

Fig. 8. One month after production of lesion in brain. At the upper and right hand part of the figure are seen normal fibres belonging to the direct cerebellar tract.

Fig. 9. Three months after production of lesion in brain. At the lower and right hand part of the figure are seen normal fibres belonging to an adjacent tract.

Fig. 10. Six months after production of lesion in brain.

INDEX

TO THE

PHILOSOPHICAL TRANSACTIONS (B)

FOR THE YEAR 1889.

G.

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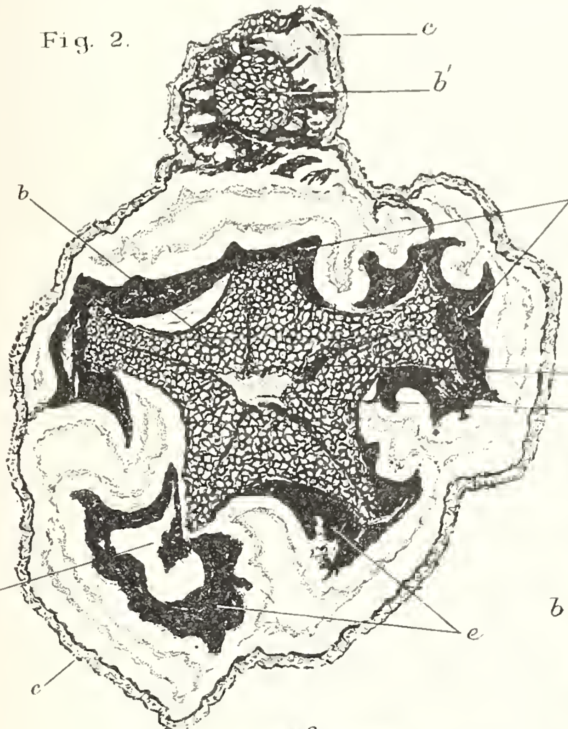

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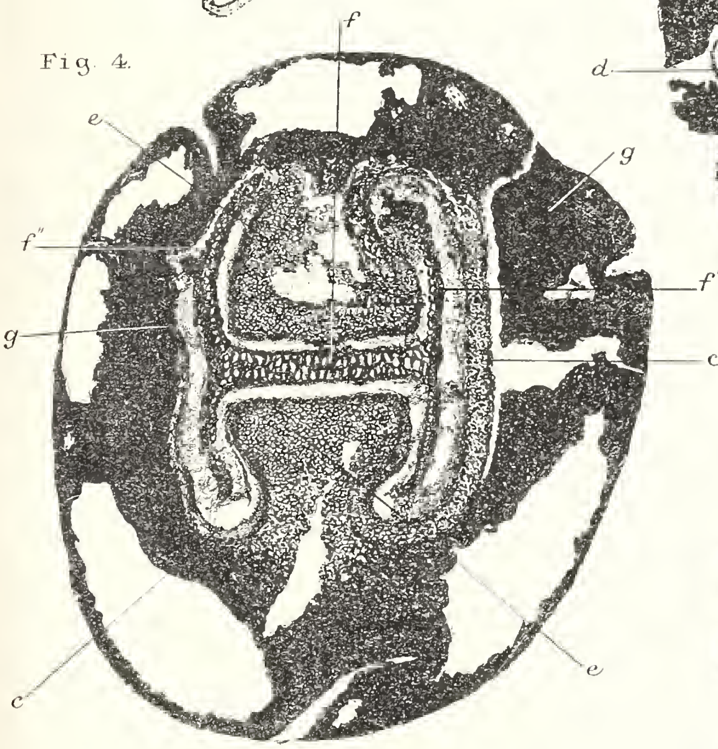

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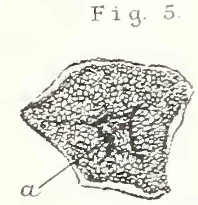

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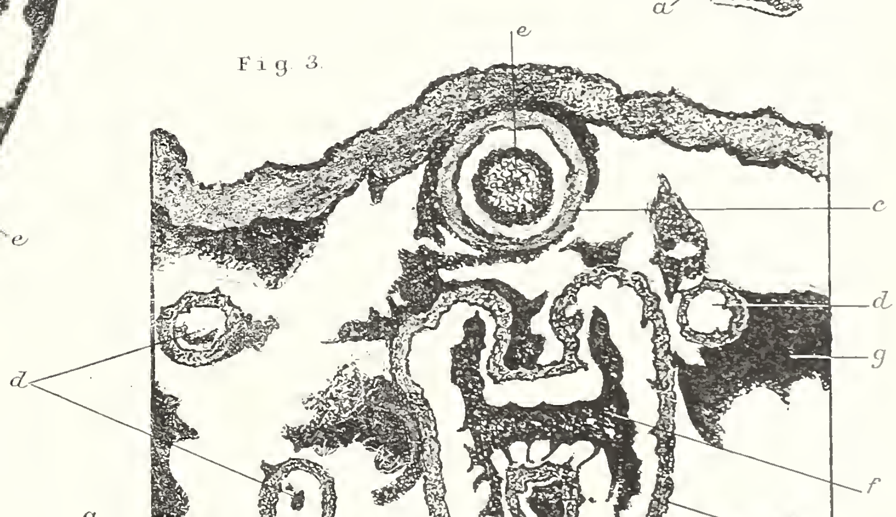

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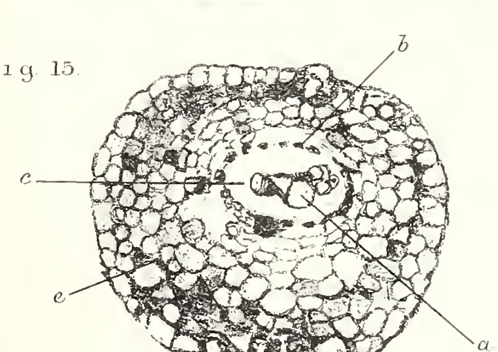

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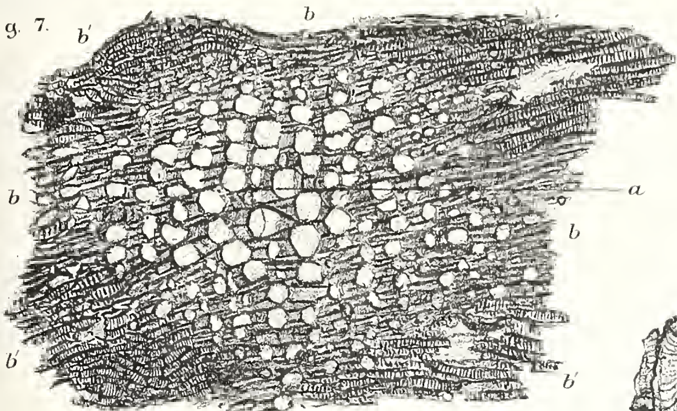

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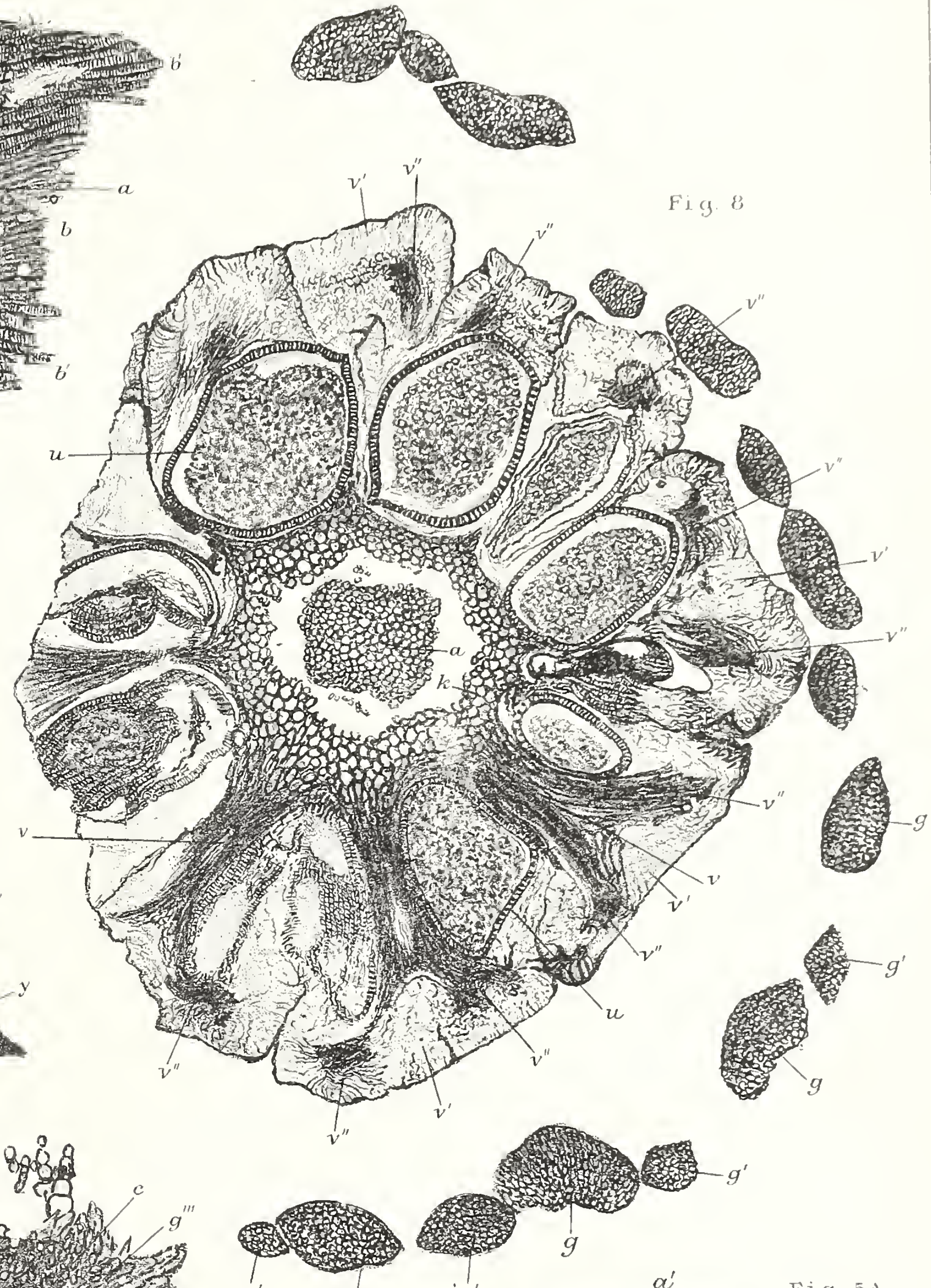

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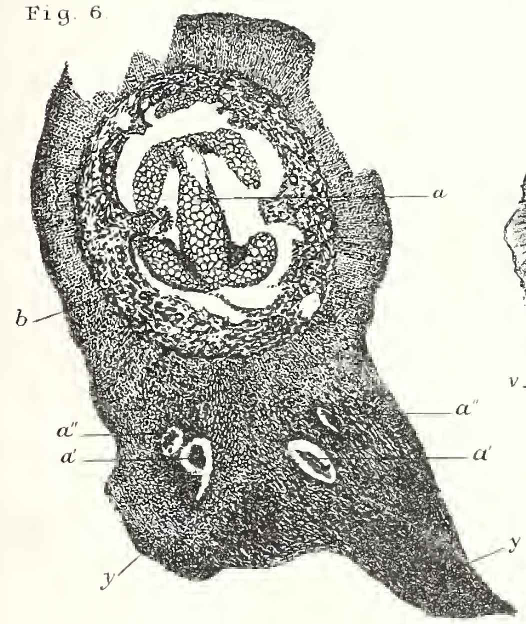

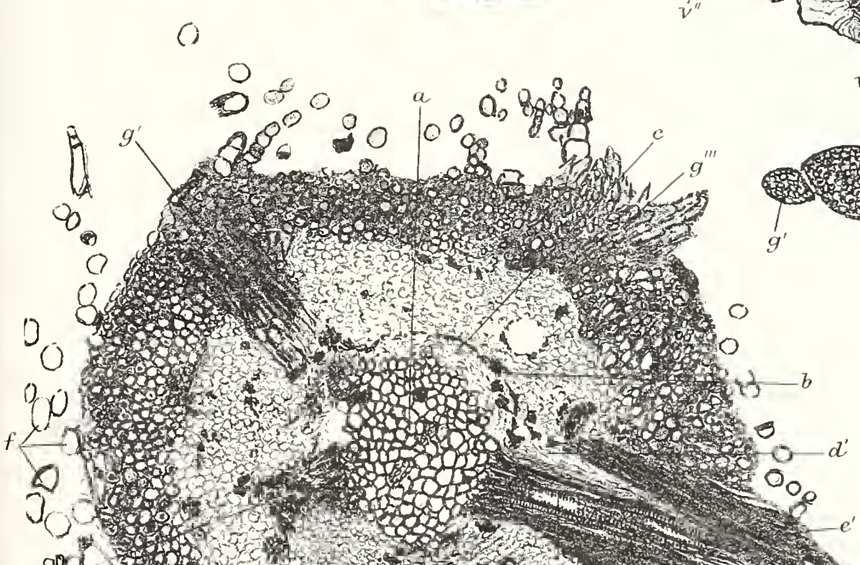

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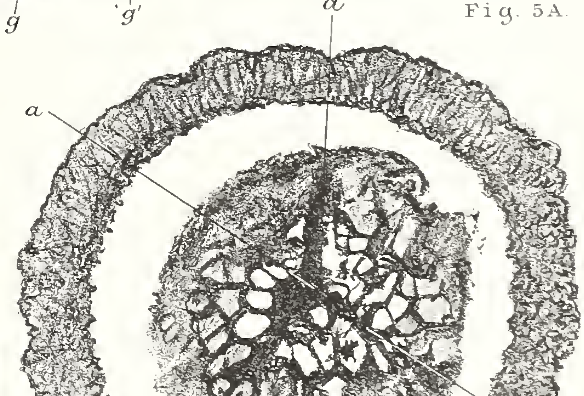

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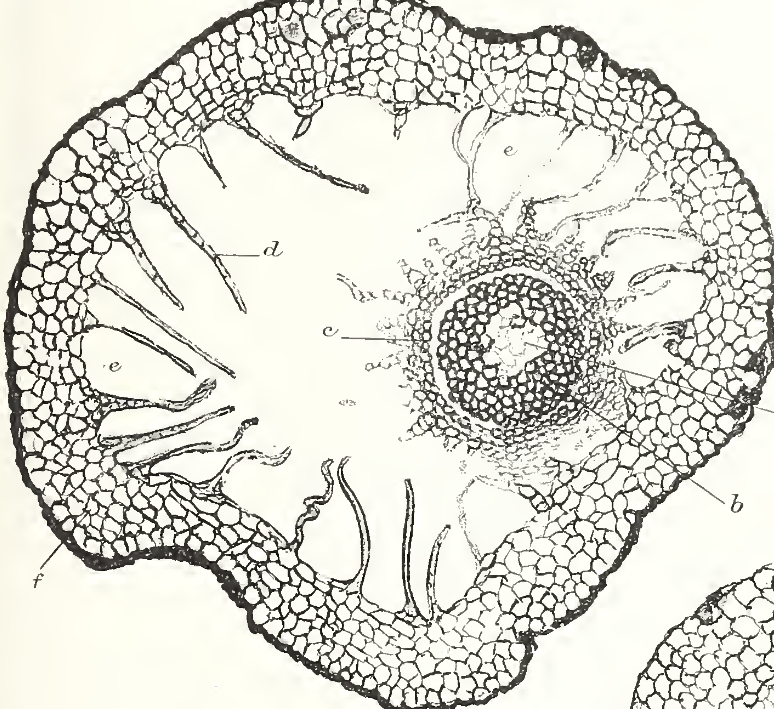

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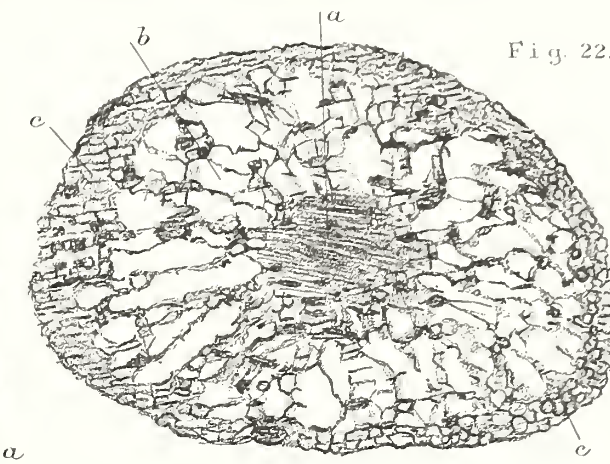

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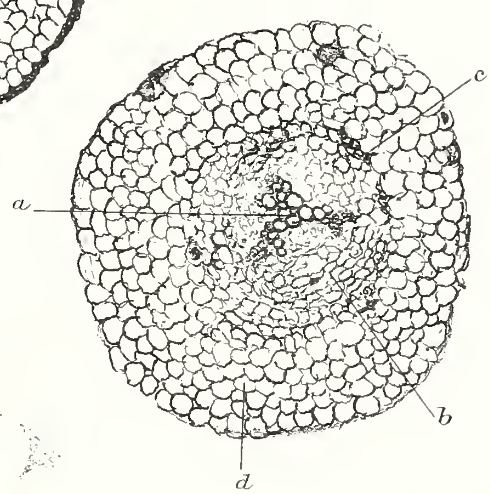

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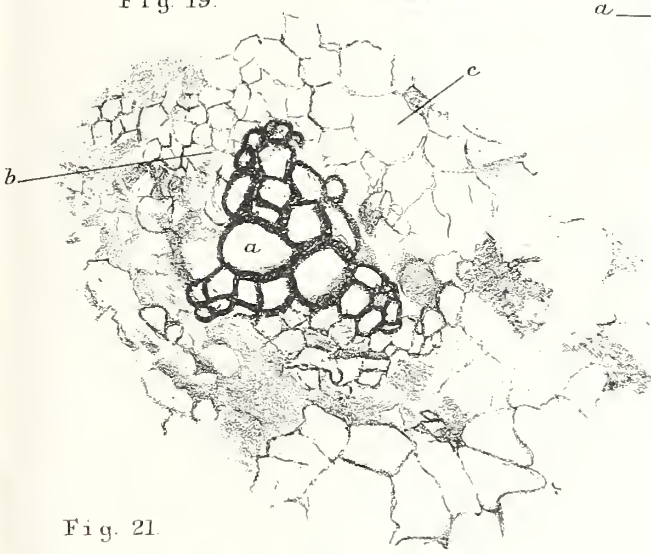

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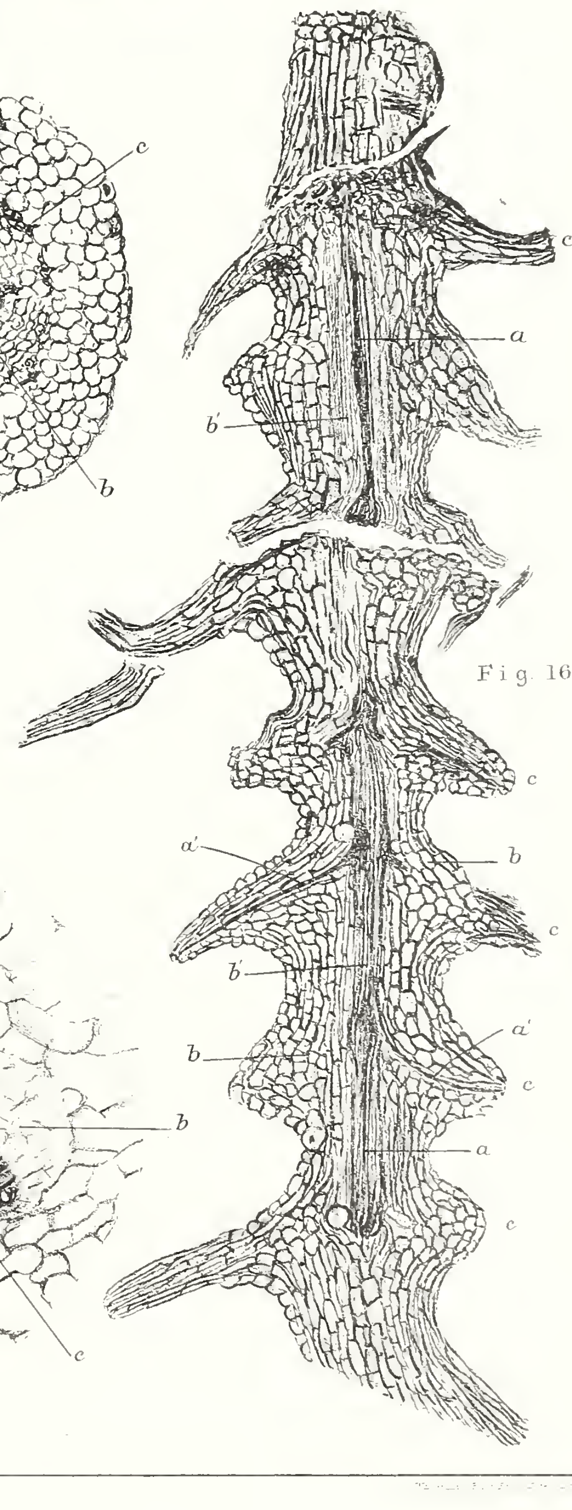

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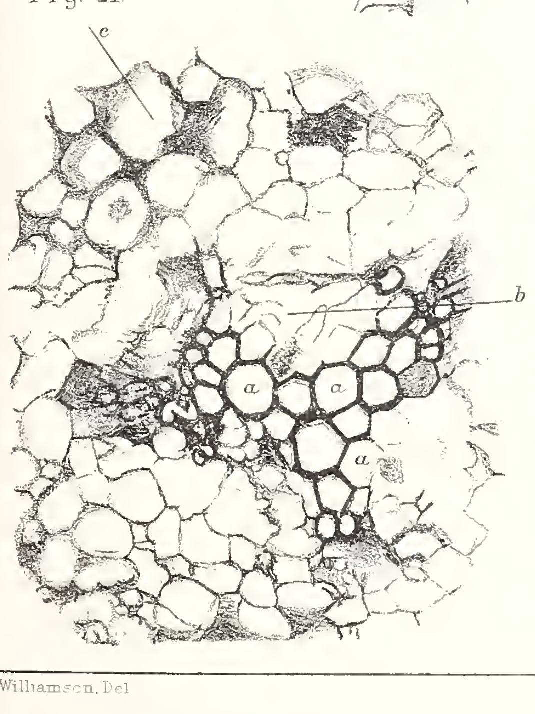

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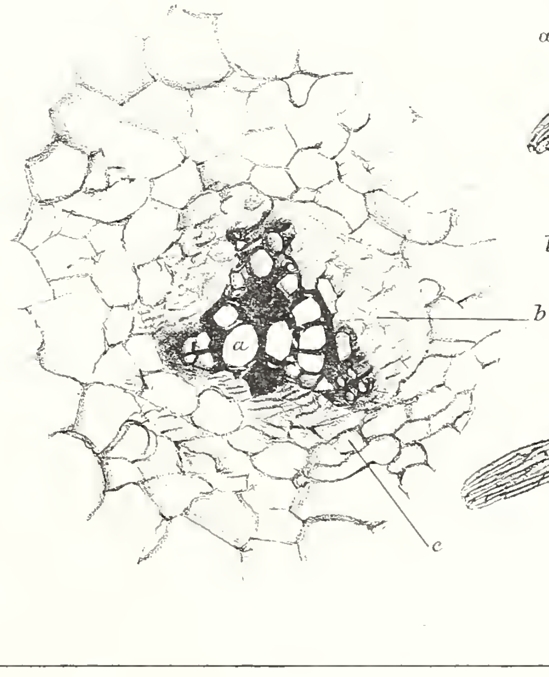

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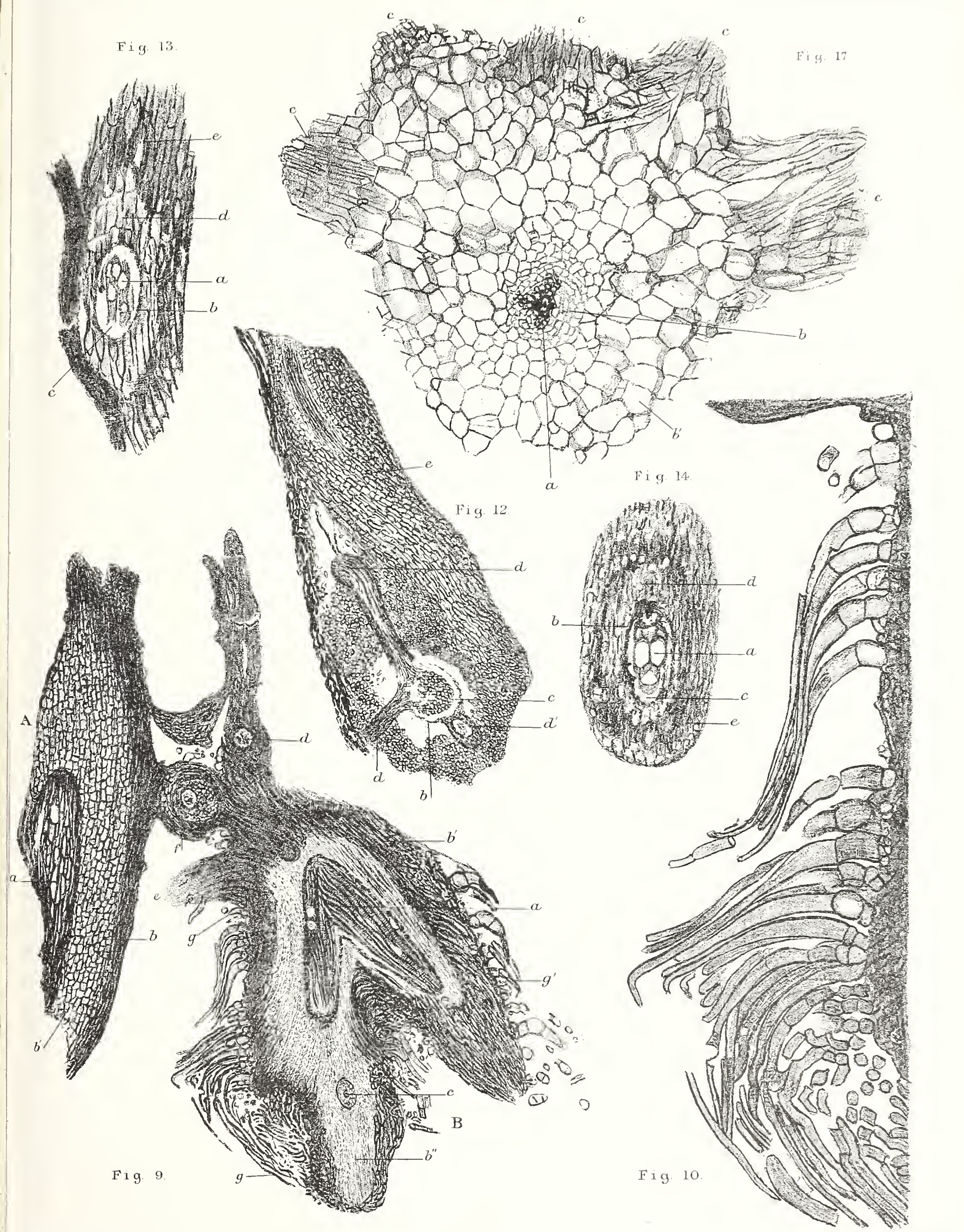

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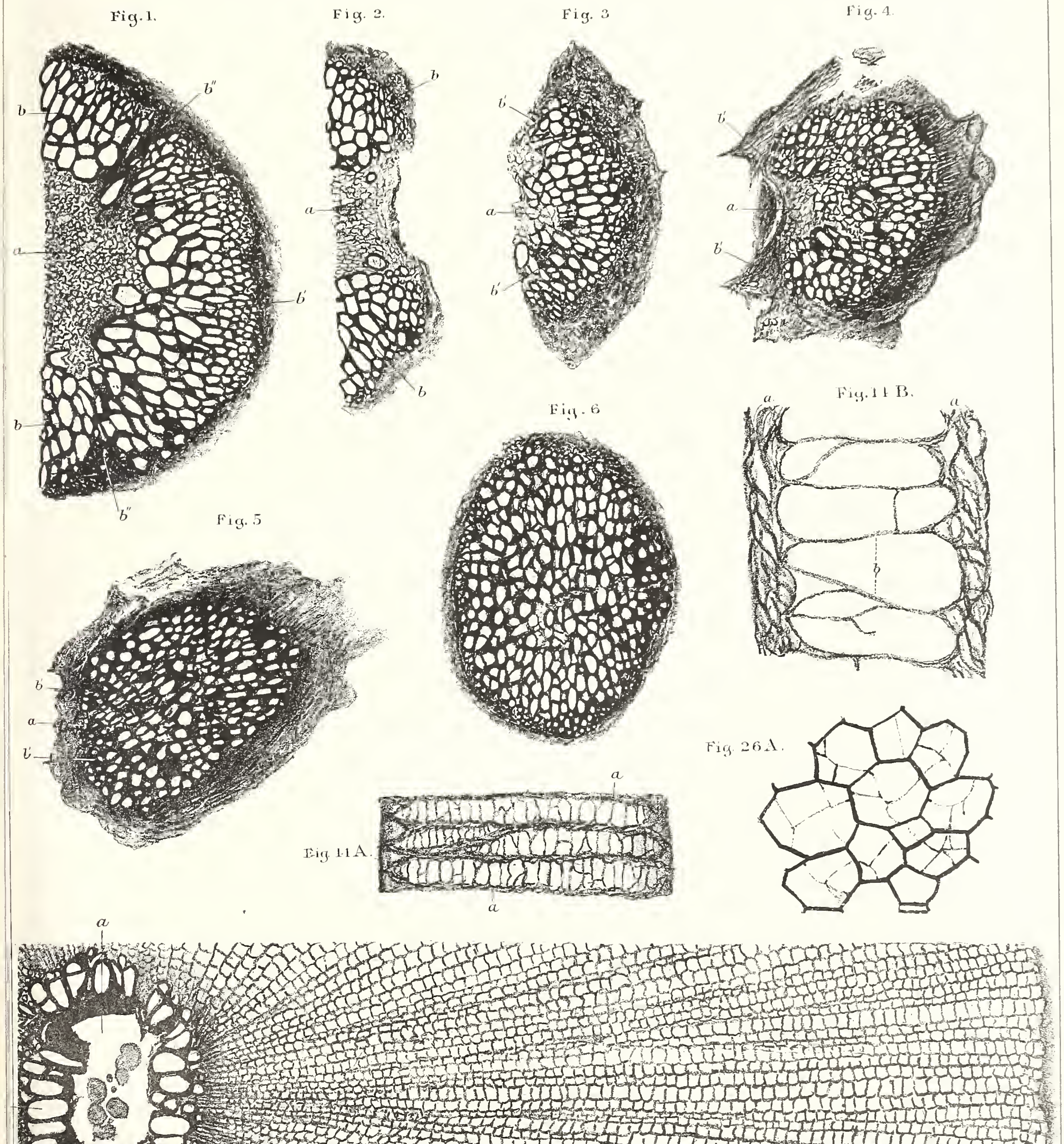
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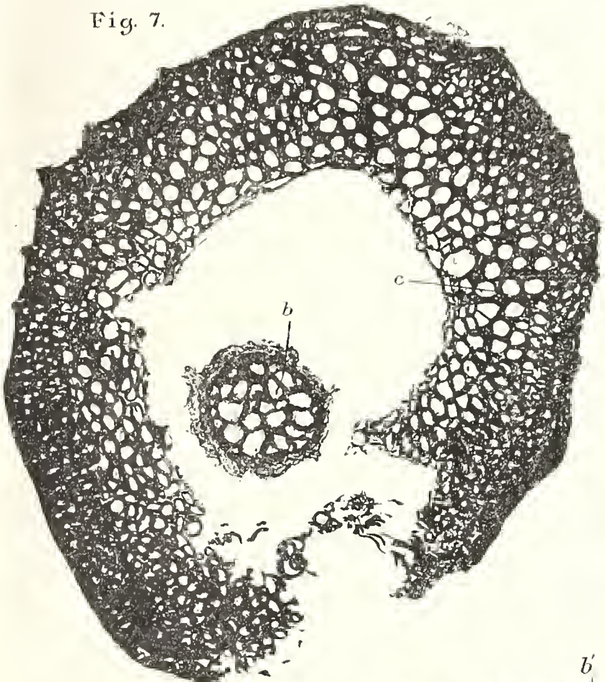

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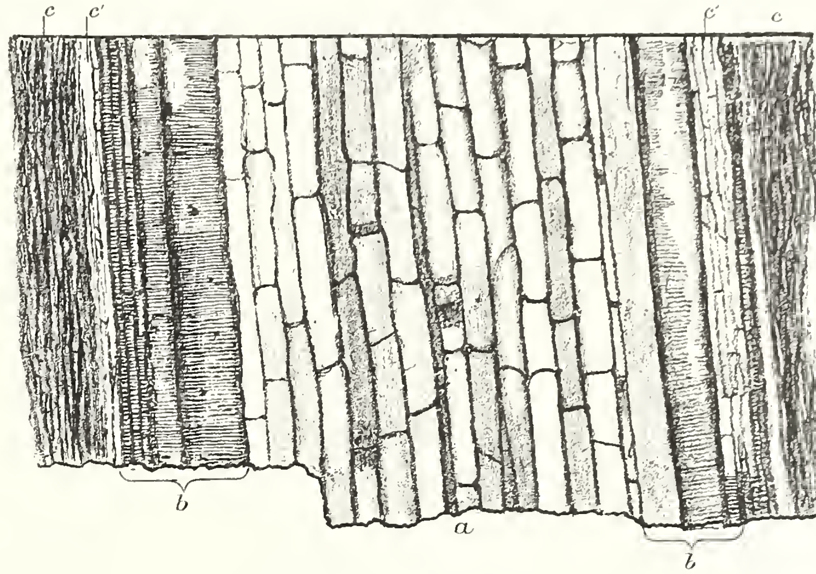

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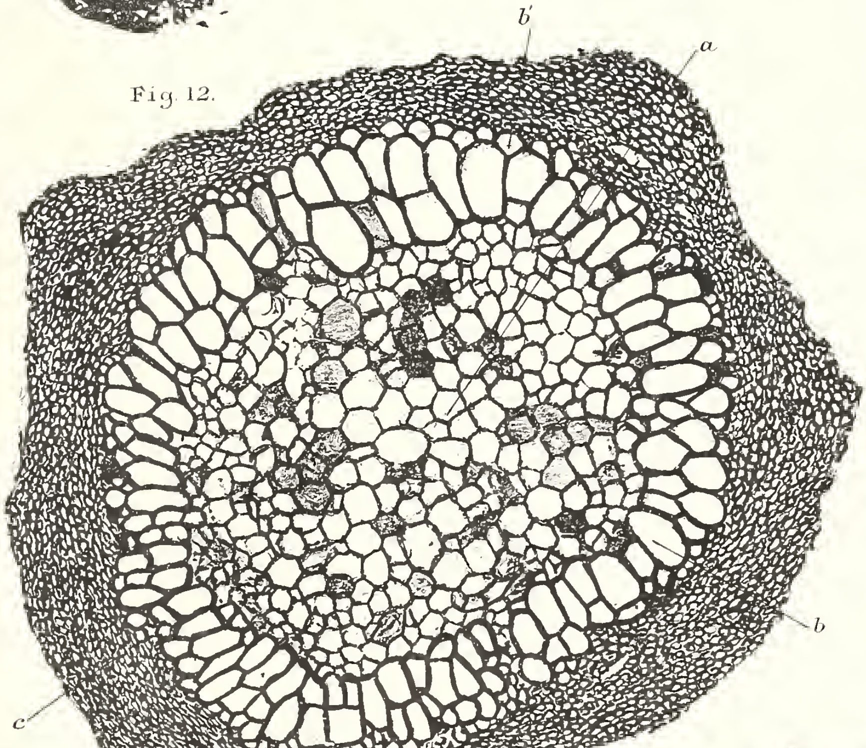
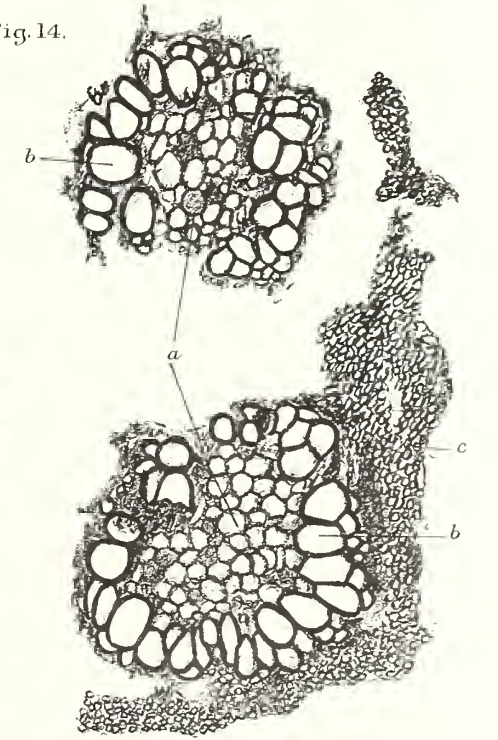


Fig. 14.



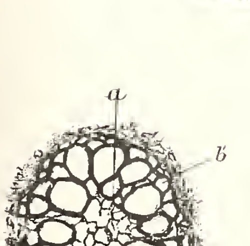
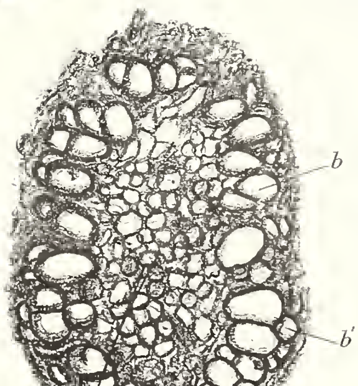


Fig. 9.

Fig. 10.



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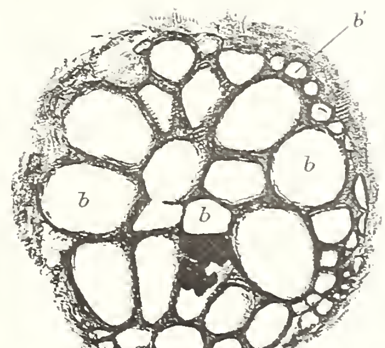
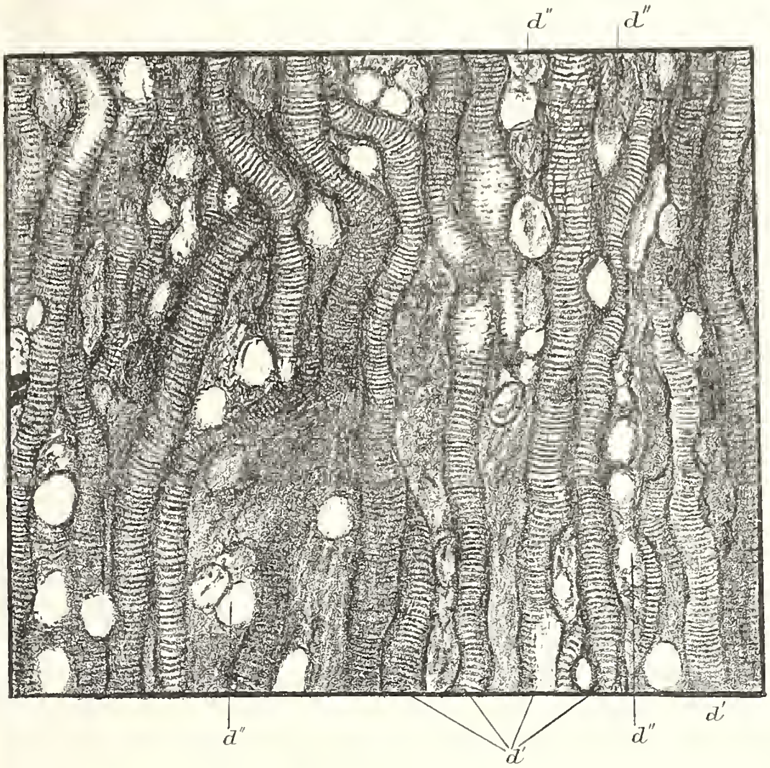


Fig. 8.

Fig. 17.



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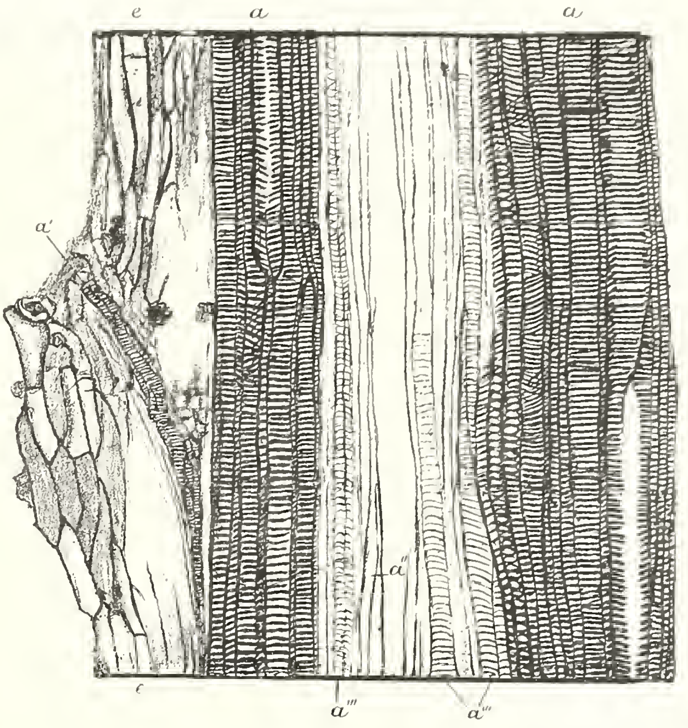


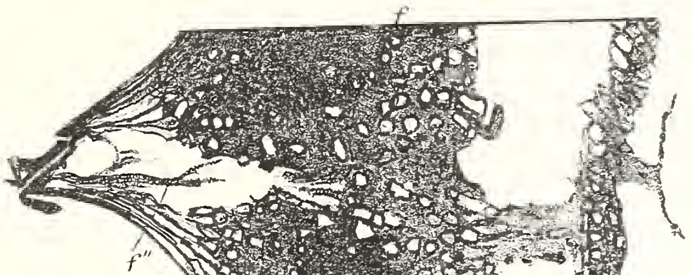
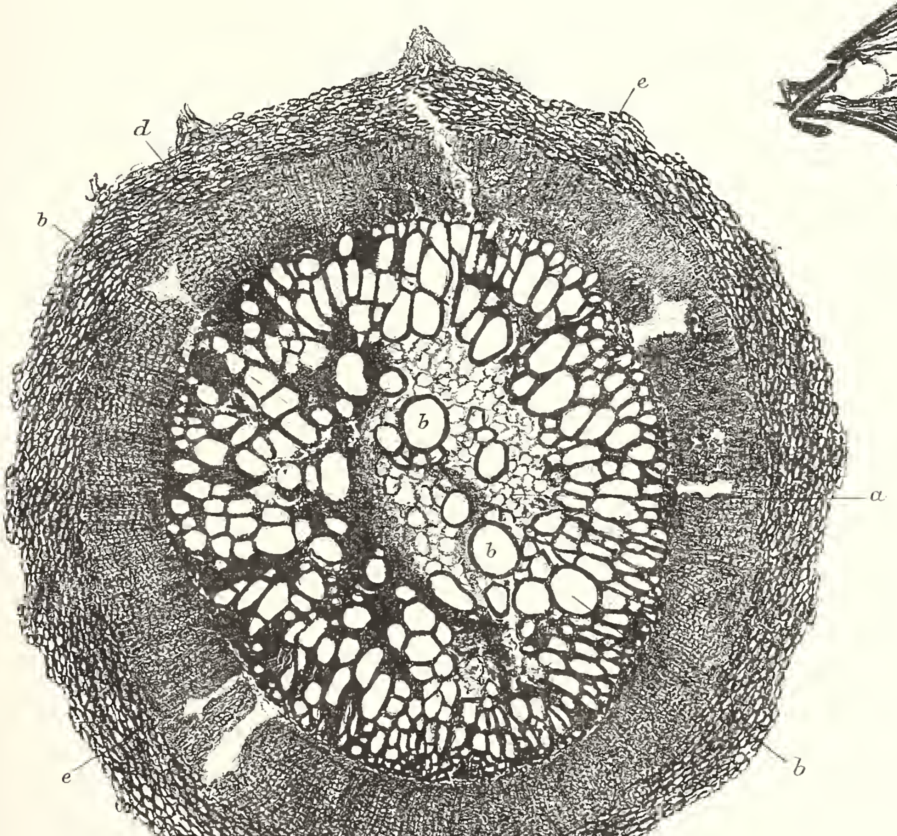


Fig. 20



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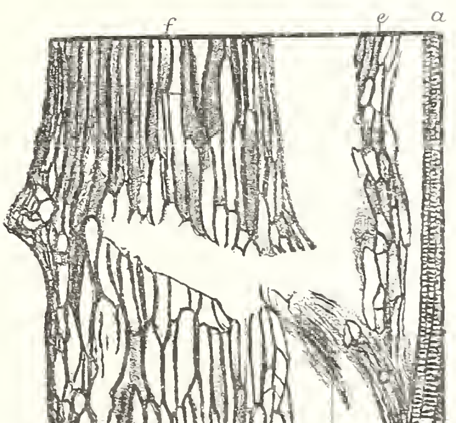


Fig. 21.

Fig. 19.

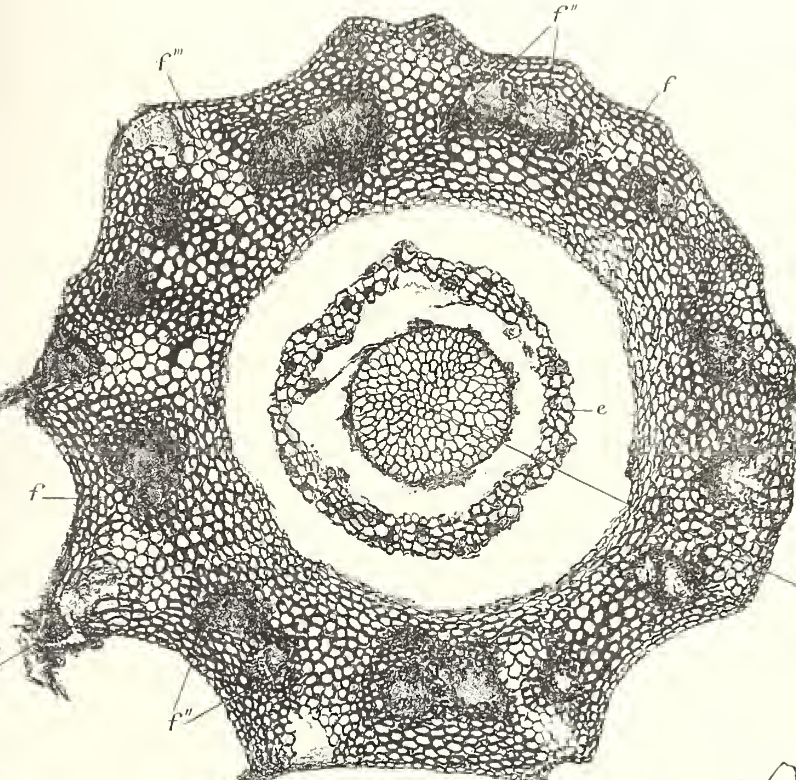
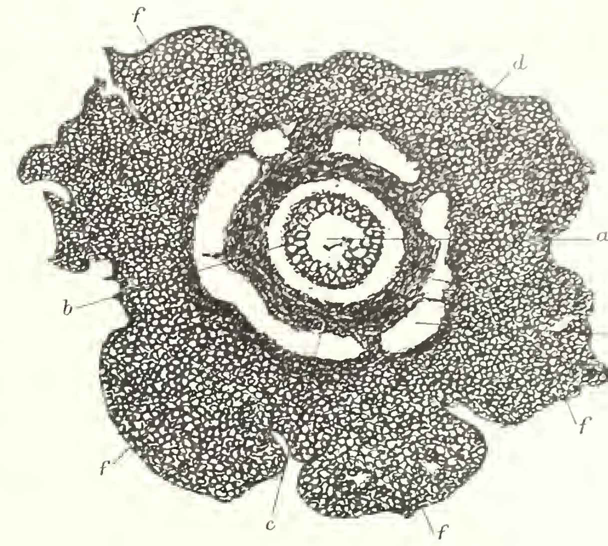


Fig. 23



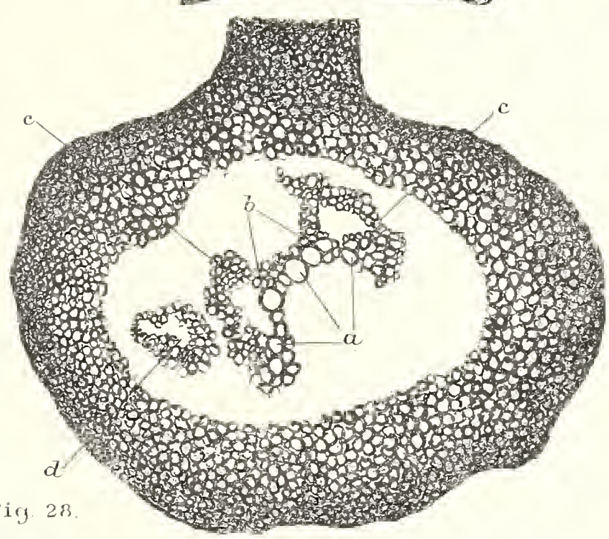
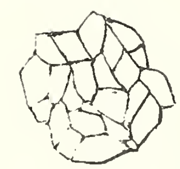


Fig. 28.



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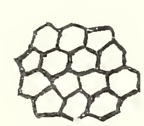
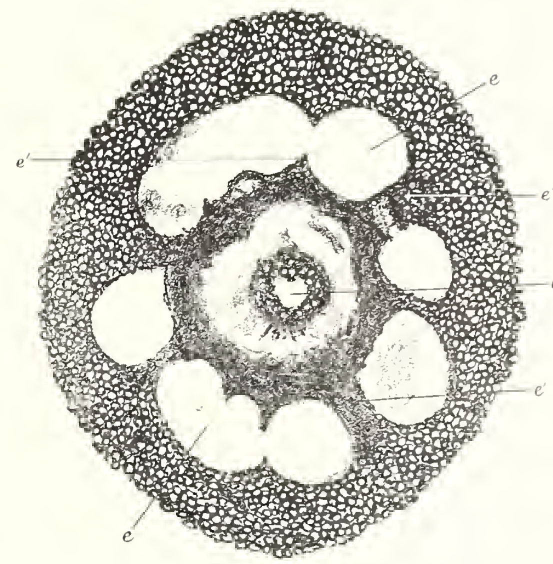


Fig. 26.



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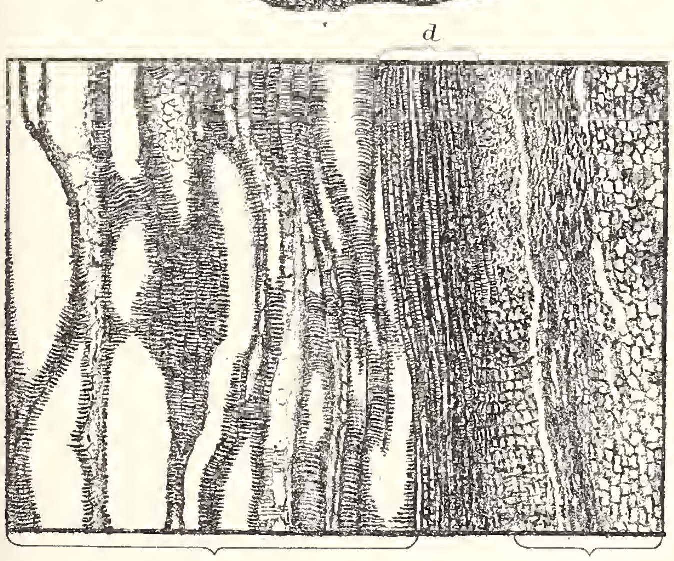


Fig. 18.

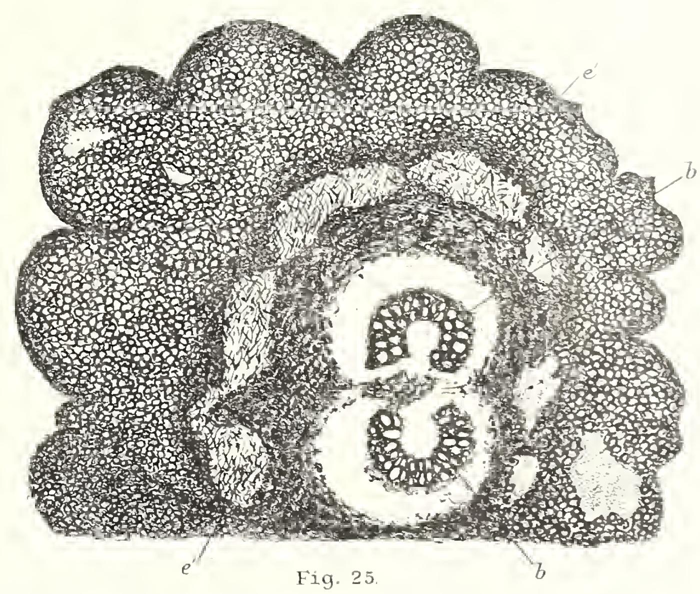
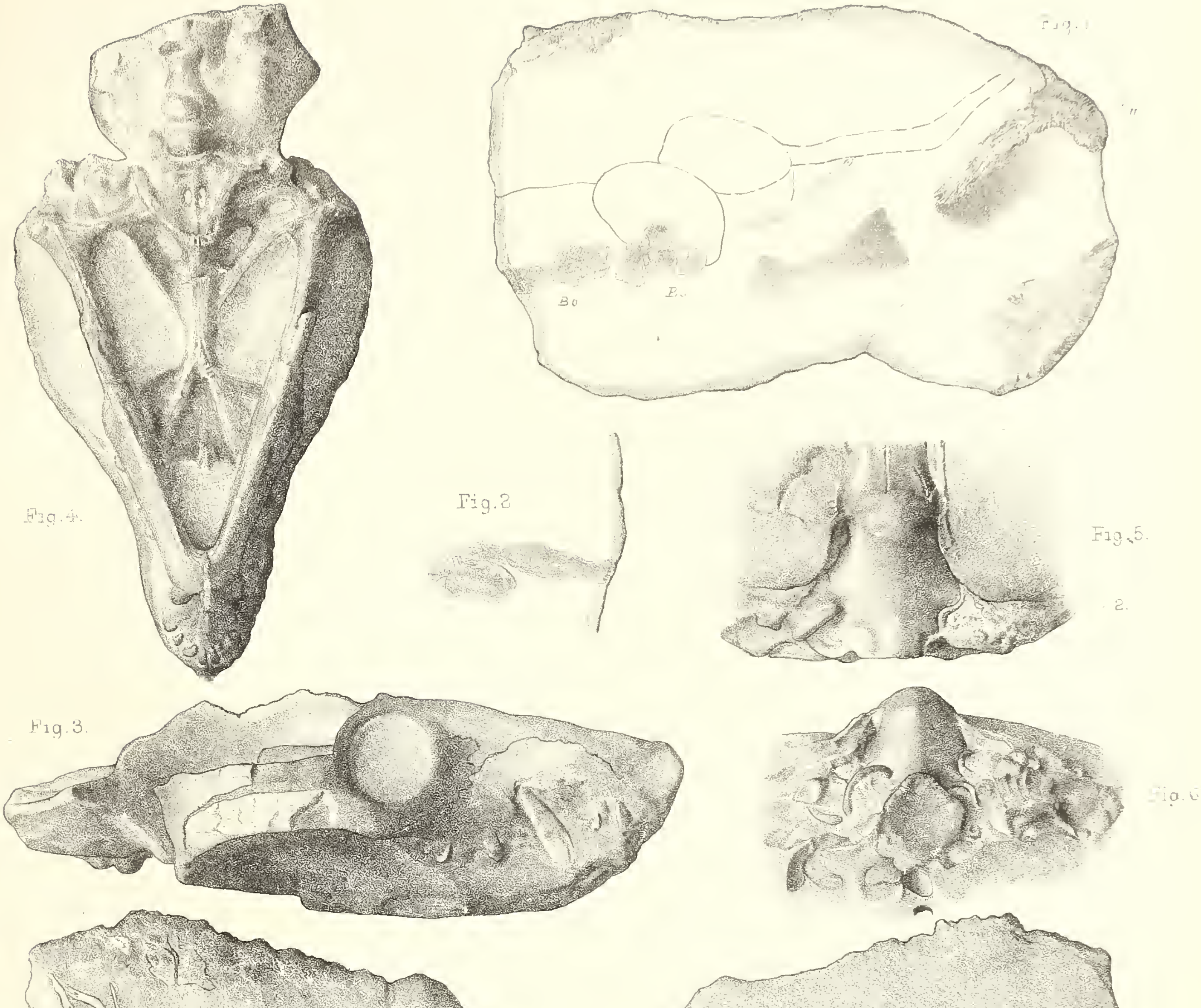


Fig. 25.

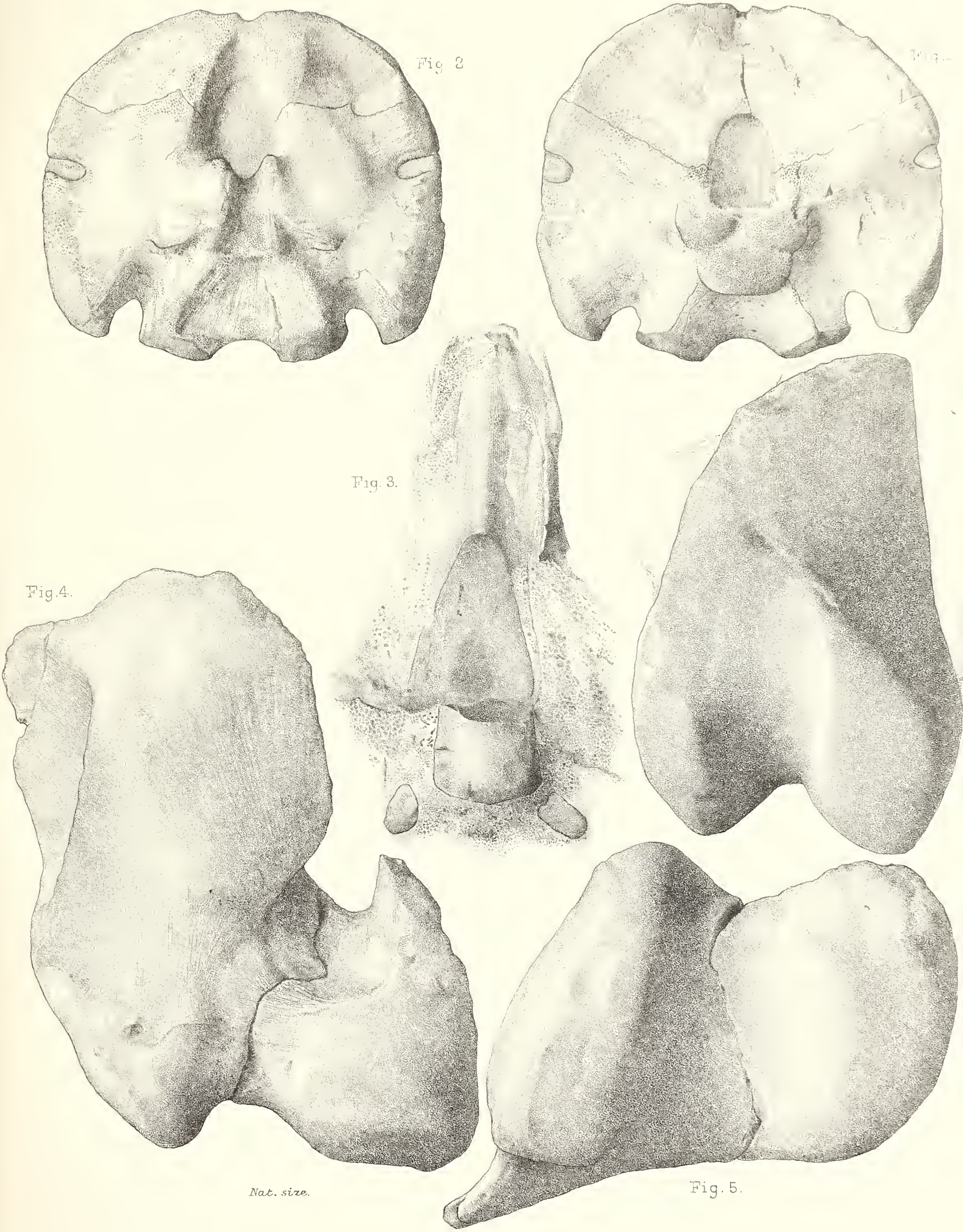
W. C. Williamson, Del.



G. M. Woodward ad nat. lith.

West, Newman imp.

Galesaurus & Procolophon.



G. M. Woodward, ad. nat. lith.

West Newman & Co. imp.

South African Anomodontia.

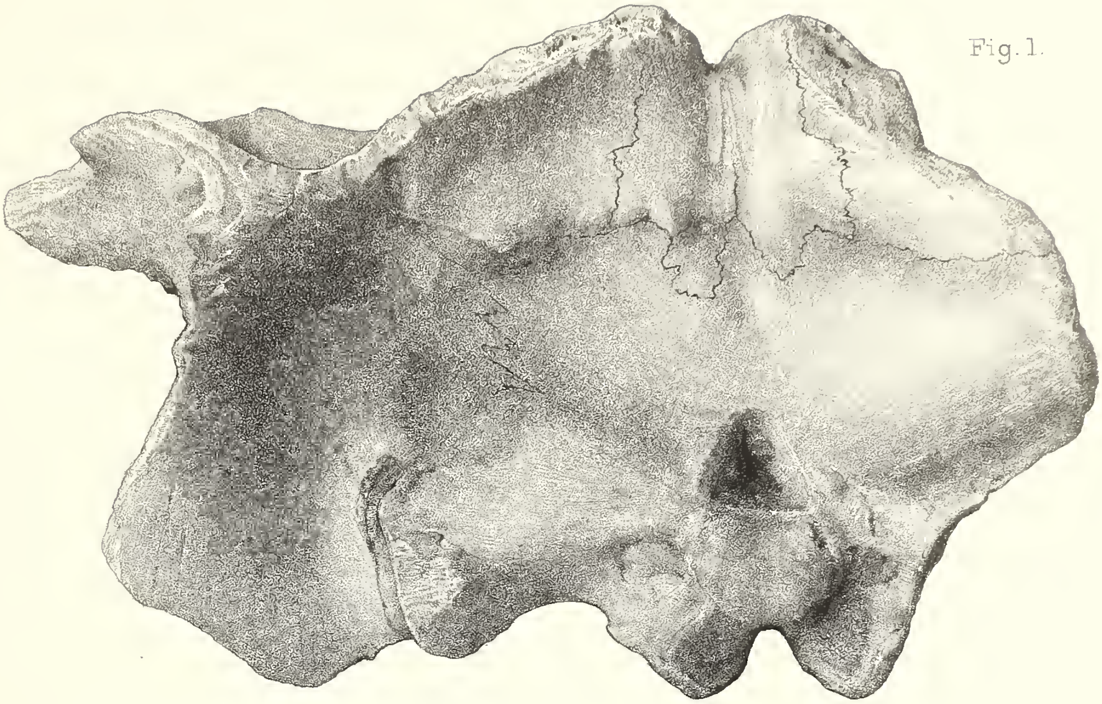


Fig. 1.

Fig. 2.

Nat. size.

G.M.Woodward ad nat. lith. West Newman & Co imp.

Dicynodon microtrema.

Fig. 2.

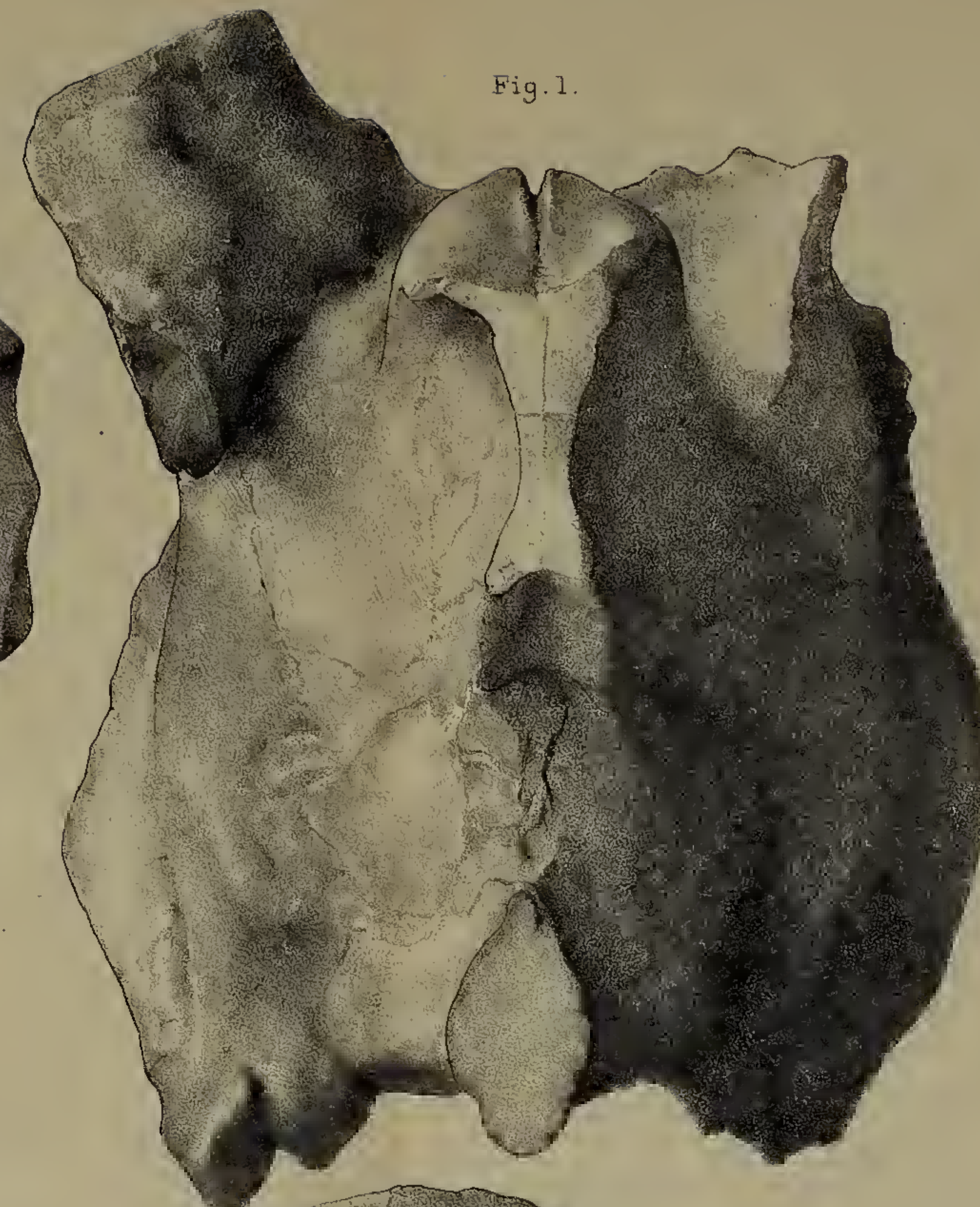


Fig. 1.

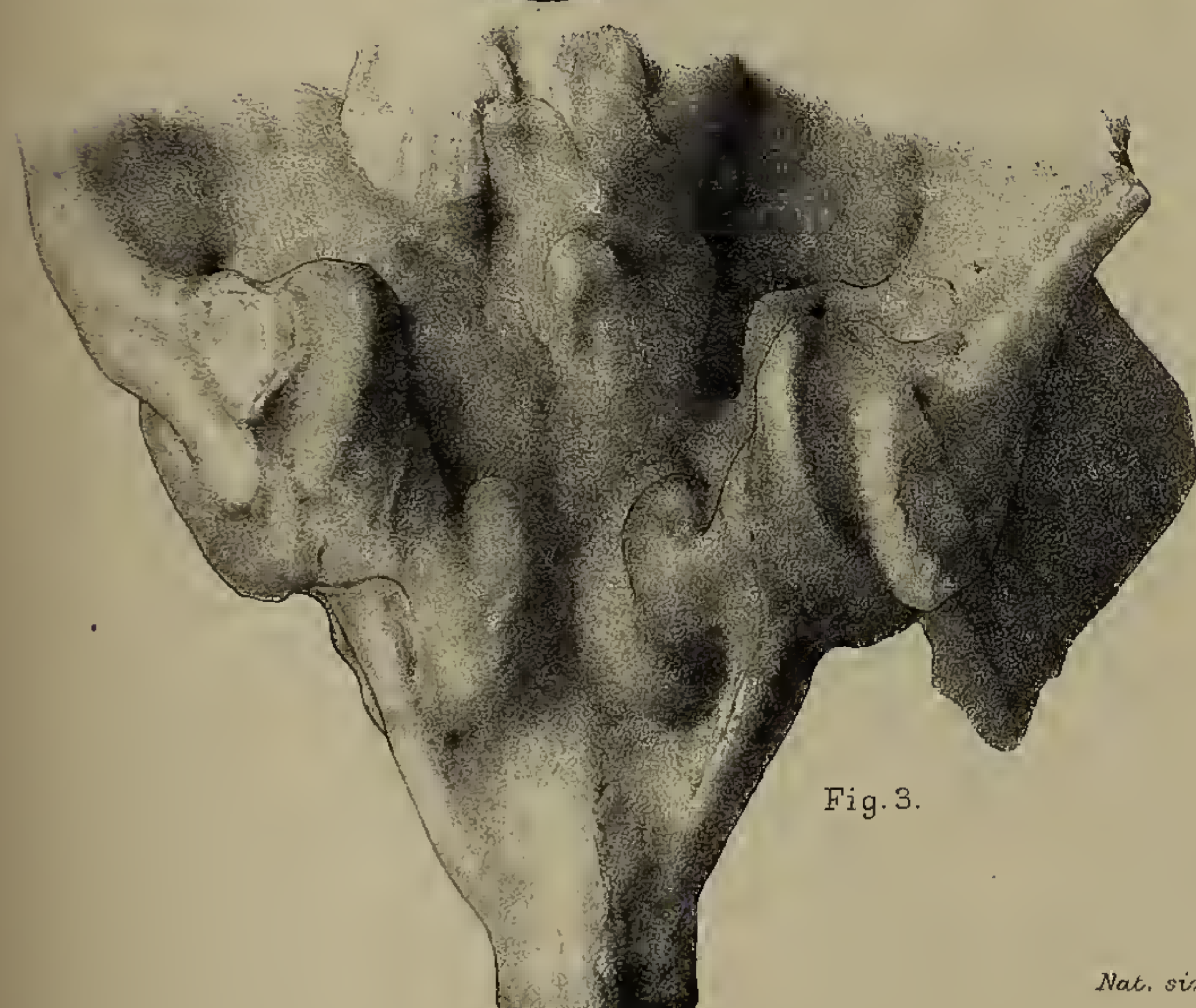


Fig. 3.

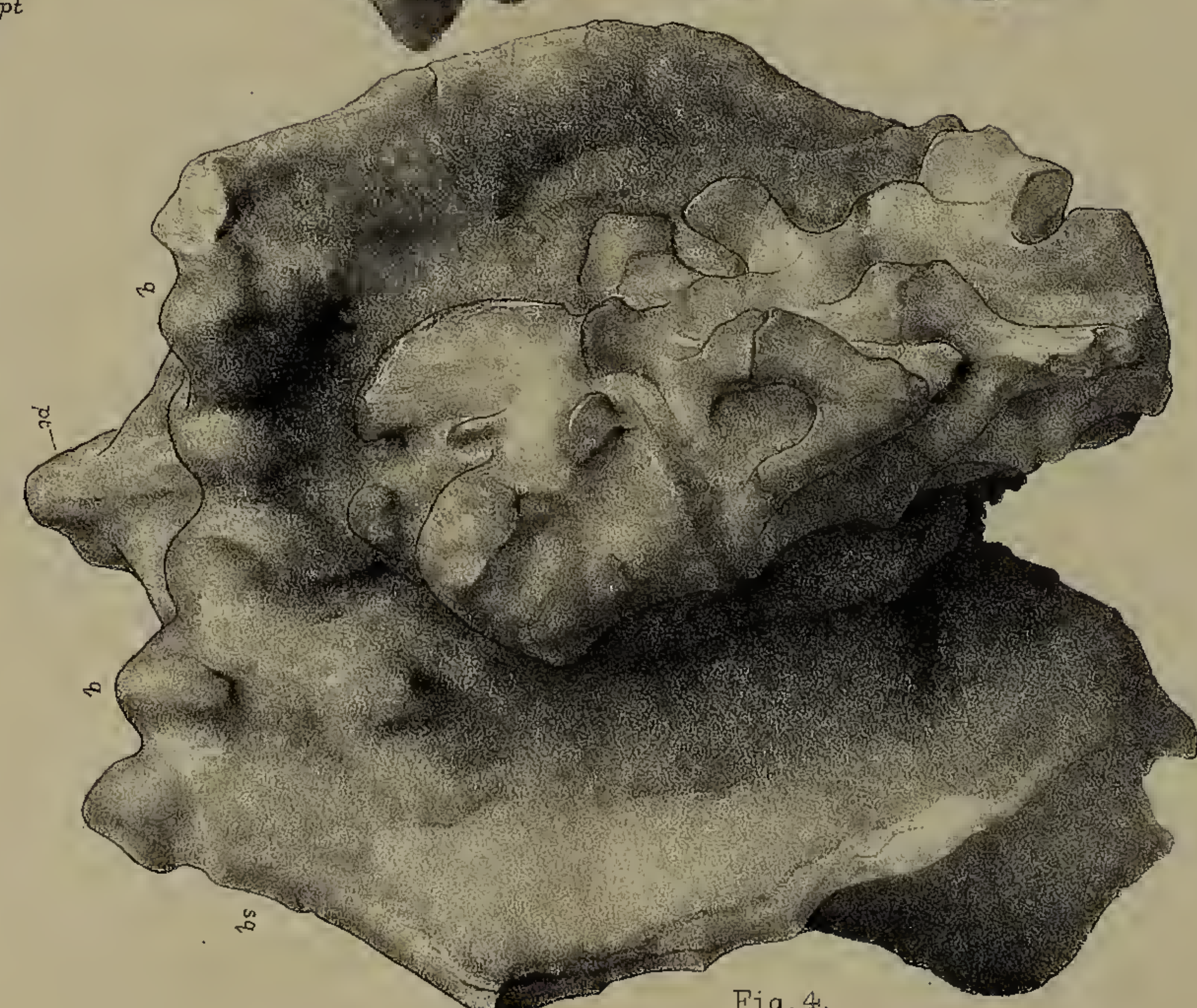


Fig. 4.

Nat. size.

G. M. Woodward ad nat. lith. West, Newman imp.

Tropidostoma Dunnii.

Fig. 1

Fig. 2

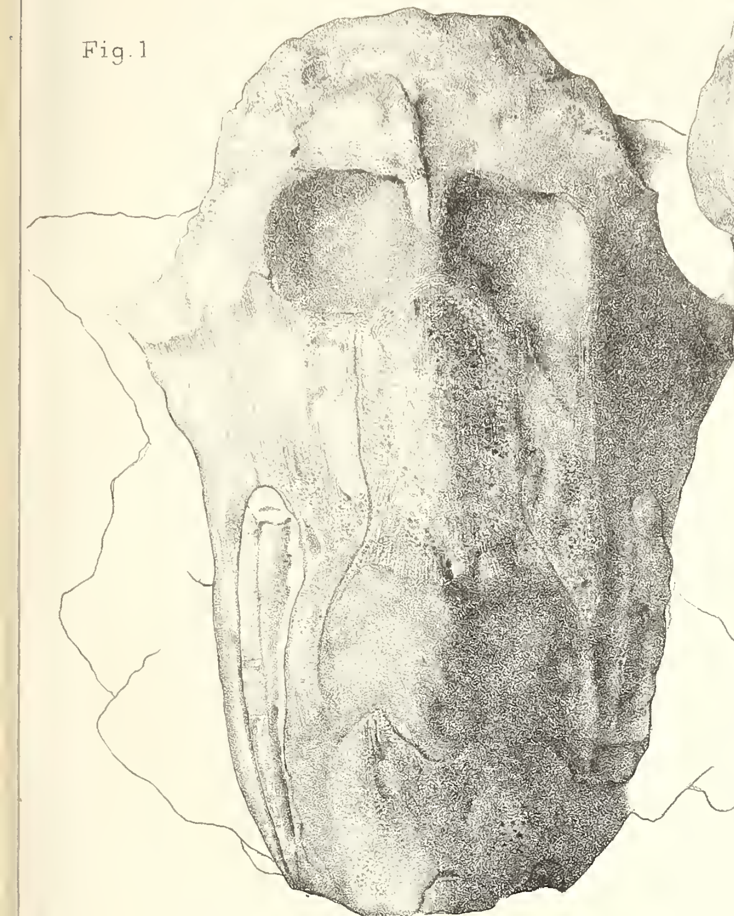
$\frac{2}{3}$ *nat. size*

W. H. Wesley, ad nat. lith.

West, Newman imp.

Dicynodon tigriceps.

Fig. 1



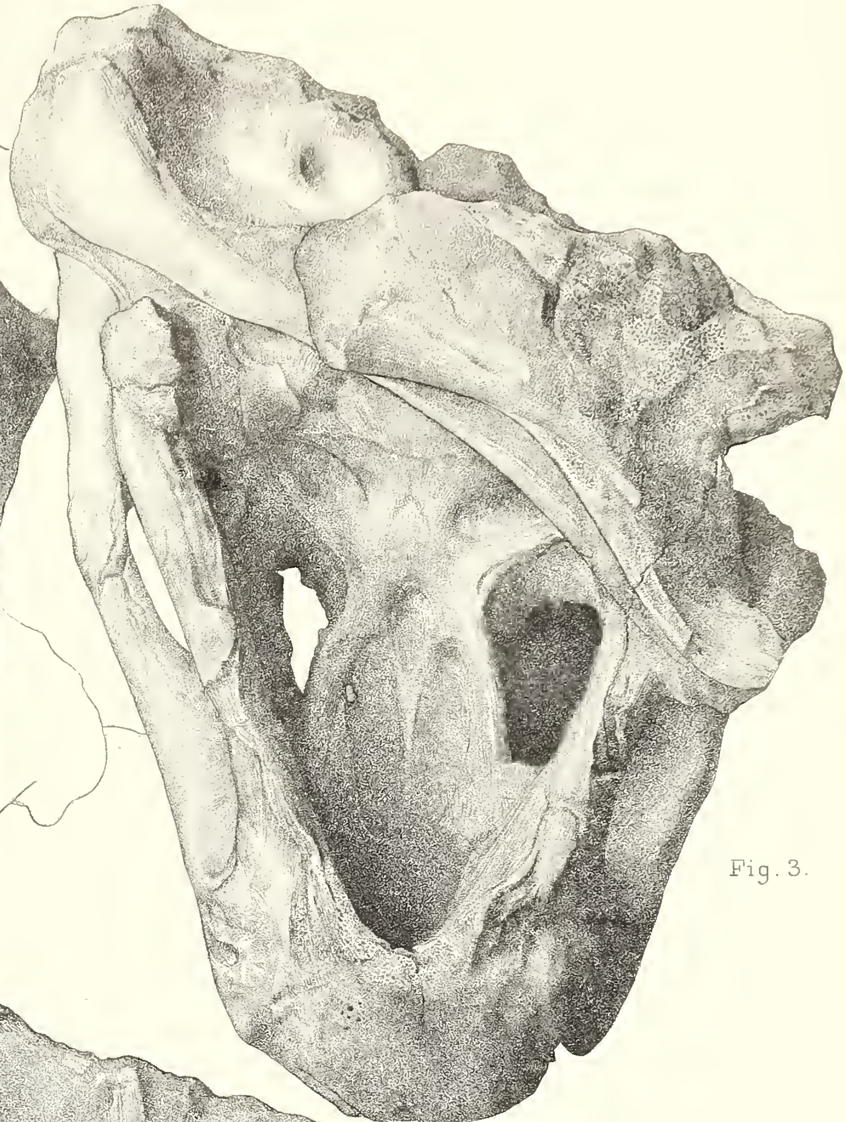
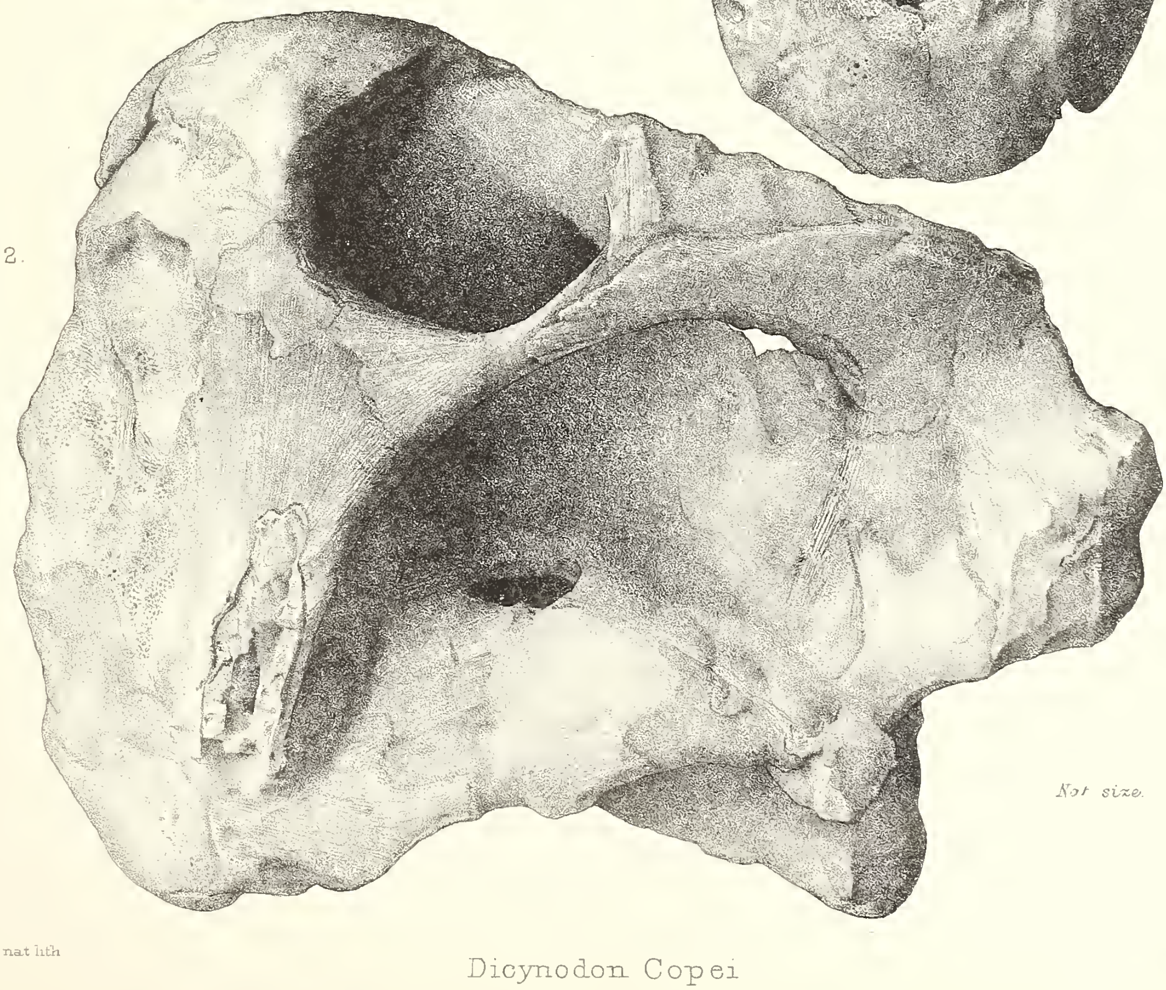


Fig. 3.

Fig. 2.



Nat. size.

G. M. Woodward, ad nat lith

Dicynodon Copei

West, Newman & Co. imp

Phil. Trans. 1889. B. *Plate* 15.

Fig. 1

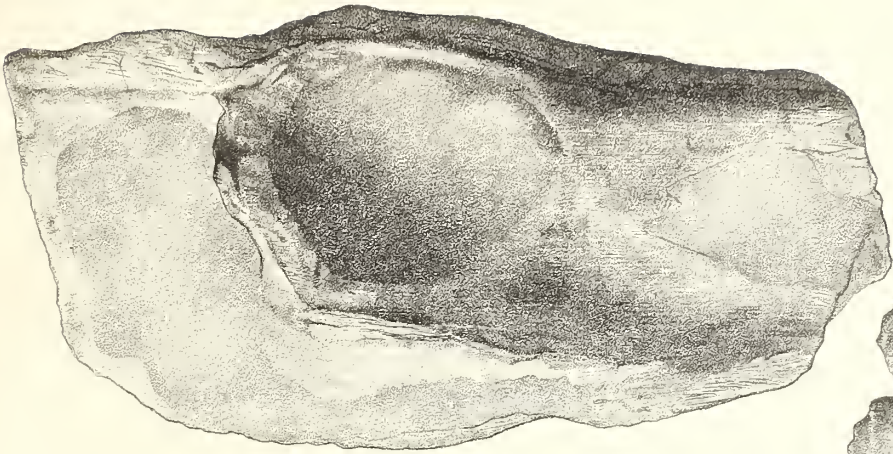
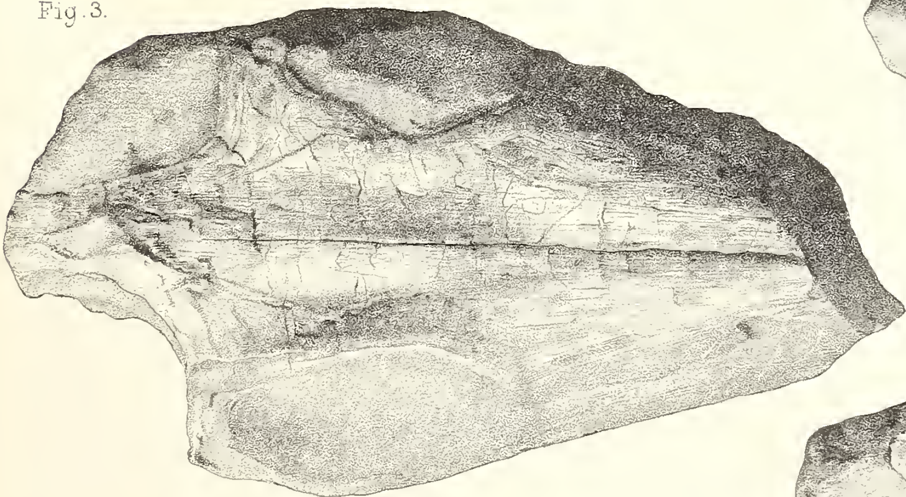


Fig. 2.

Fig. 3.



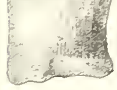


Fig. 7

Nat. size

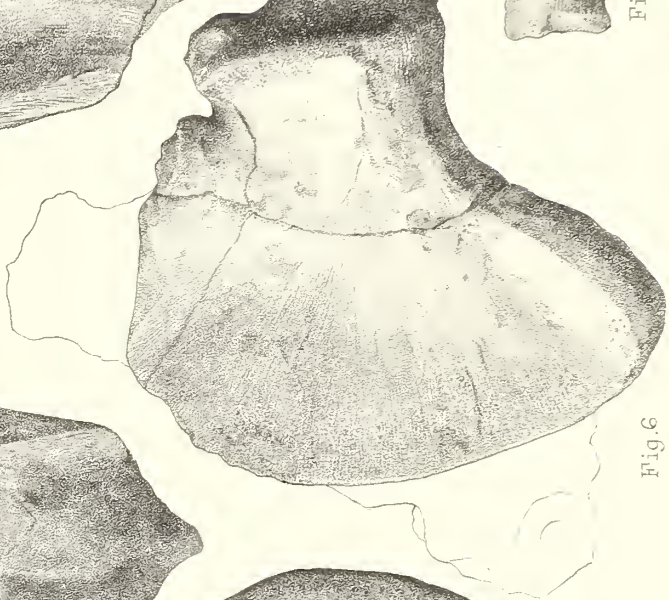


Fig. 6

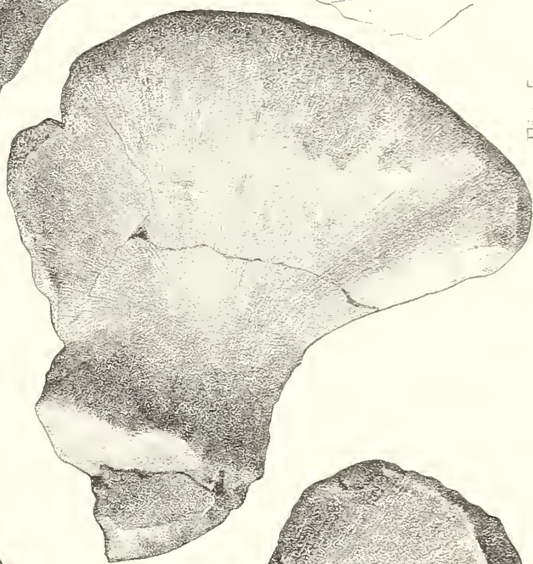


Fig. 5.

Fig. 4.

G.M. Woodward ad nat. lith.

Hyorhynchus platyceps.

West, Newman imp.

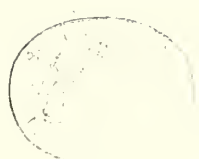
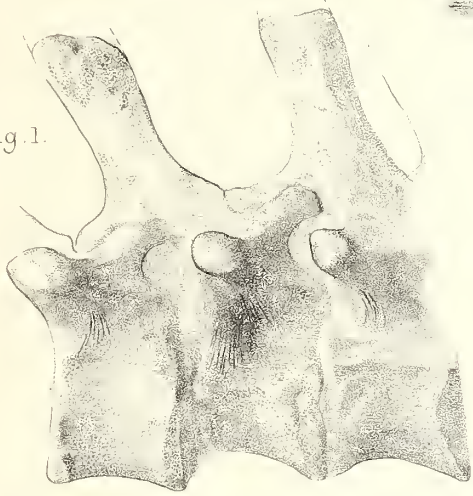


Fig. 3.

Fig. 2.

Fig. 1.



Nat. size.

Fig. 4.

G. M. Woodward, ad nat lith. West, Newman imp.

Titanosuchus ferox &c.

Fig. 2.

Fig. 1.

nat. size

W.H. Wesley, ad nat lith.

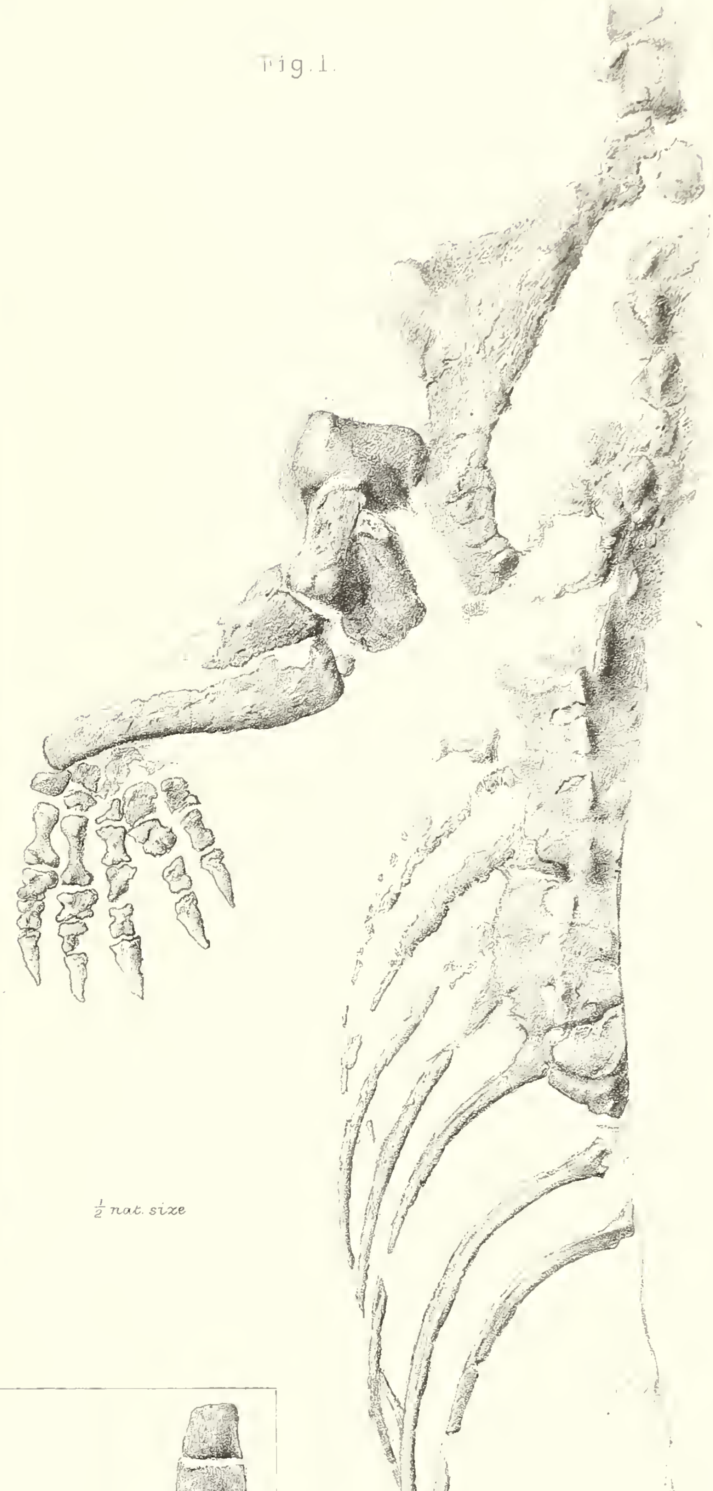
Platypodosaurus.

West, Newman imp.

Fig. 2.

nat. size

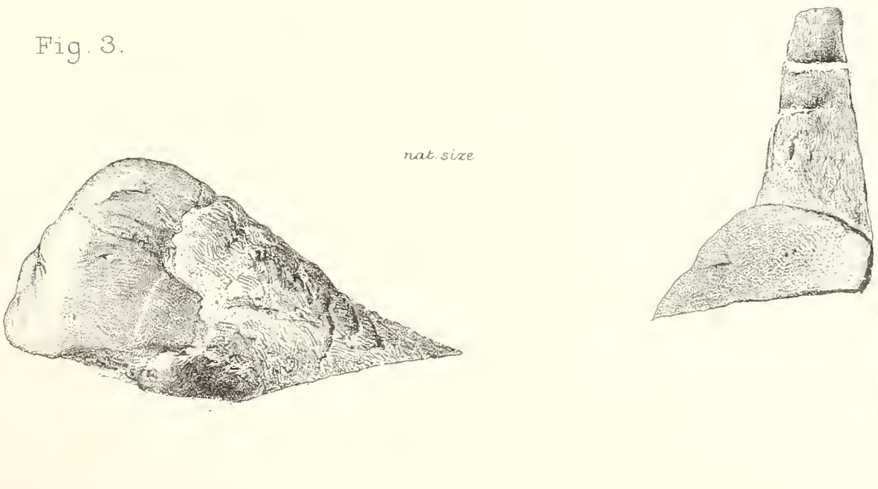
Fig. 1.

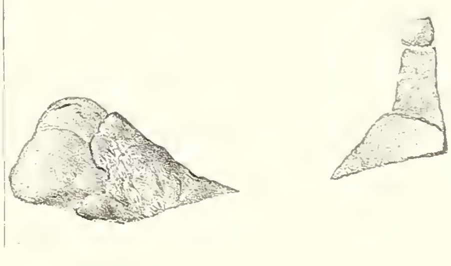


½ nat. size

Fig. 3.

nat. size



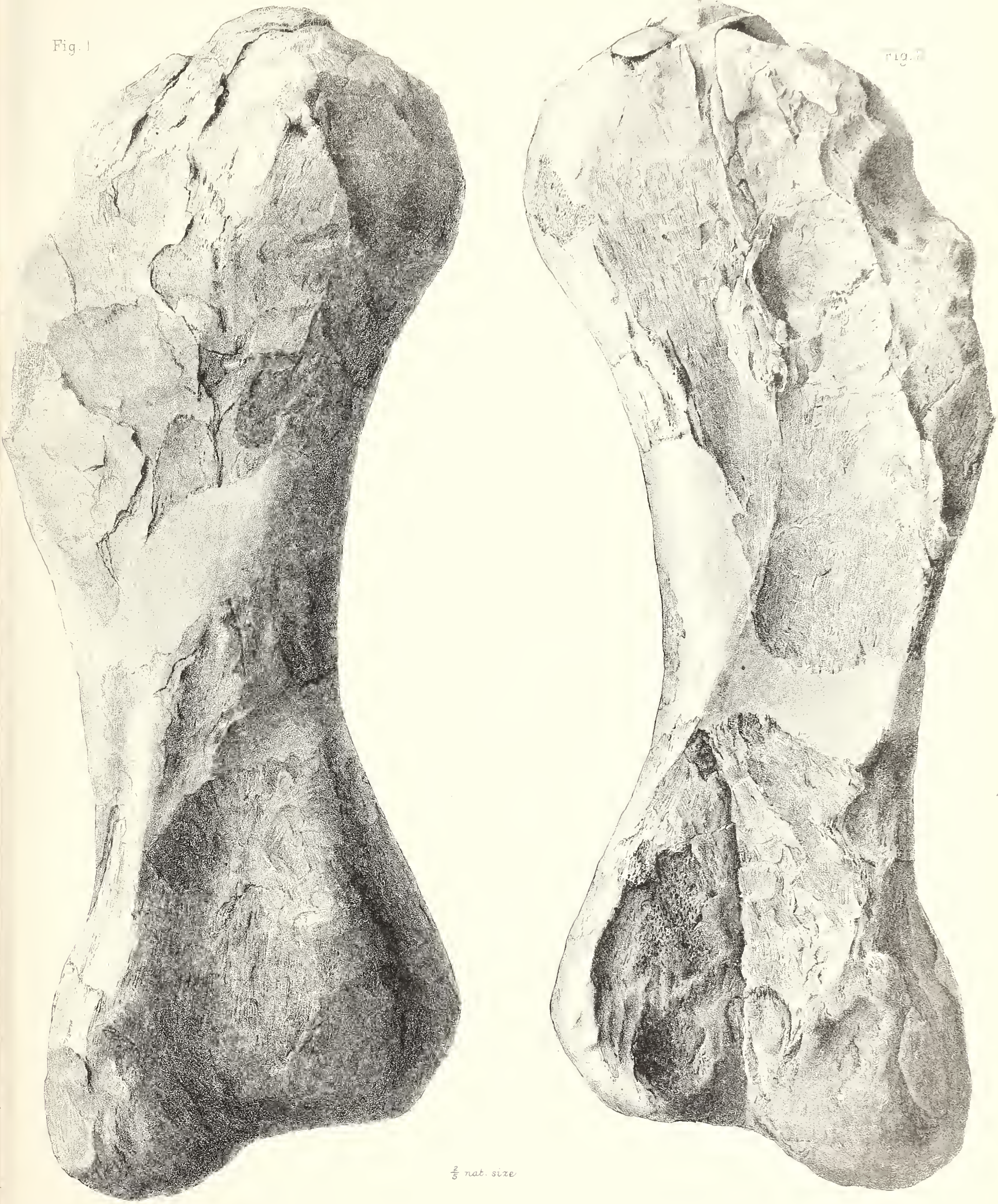


W.H. Wesley, ad nat. lith.

West, Newman imp.

Eurycarpus Oweni.

Fig. 1 Fig. 2



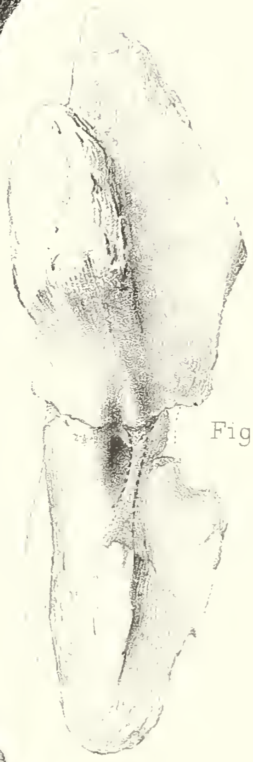
$\frac{2}{5}$ nat. size

W.H.Wesley, ad nat lith. West Newman imp.

Femur — Titanosuchus ferox.

Fig. 1.

Fig. 2.



$\frac{5}{11}$ nat. size

W.H.Wesley, ad nat. lith.

West, Newman imp.

Humerus—Titanosuchus ferox.

Fig. 1.

$\frac{3}{5}$ *nat. size*

Fig. 2.

Fig. 4.

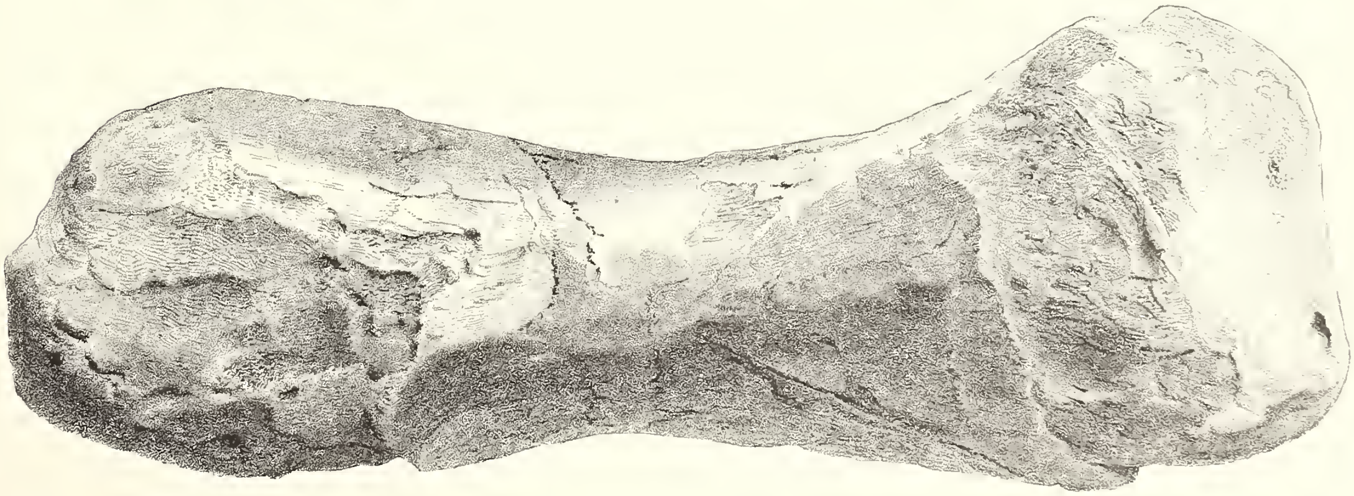


Fig. 3.

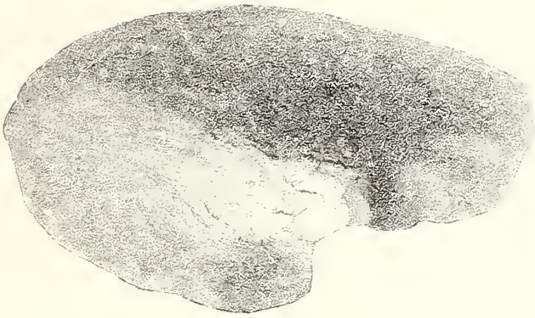
W. H. Wesley, ad nat. lith.

West, Newman imp.

Fibula — Titanosuchus ferox.

Fig. 2

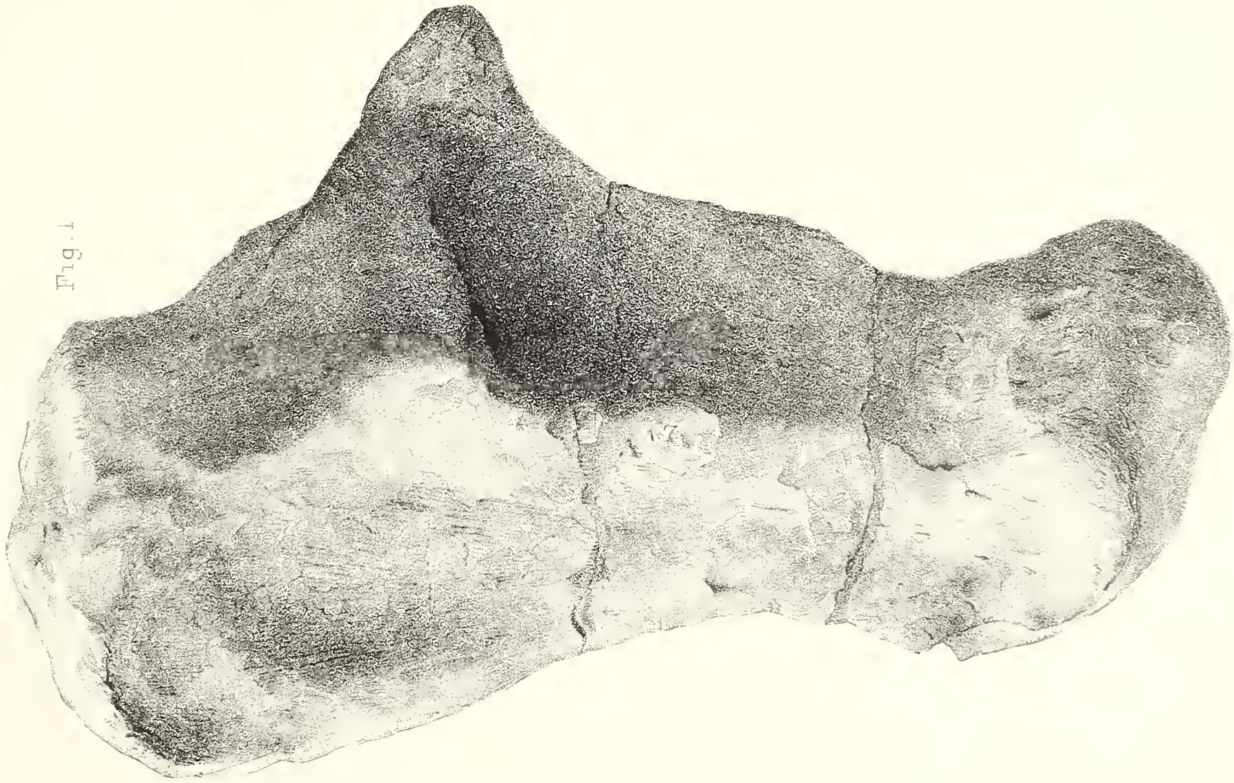
Fig. 4.



½ nat. size

Fig. 3

Fig. 1



W. H. Wesley, ad nat. lith.

Ulna

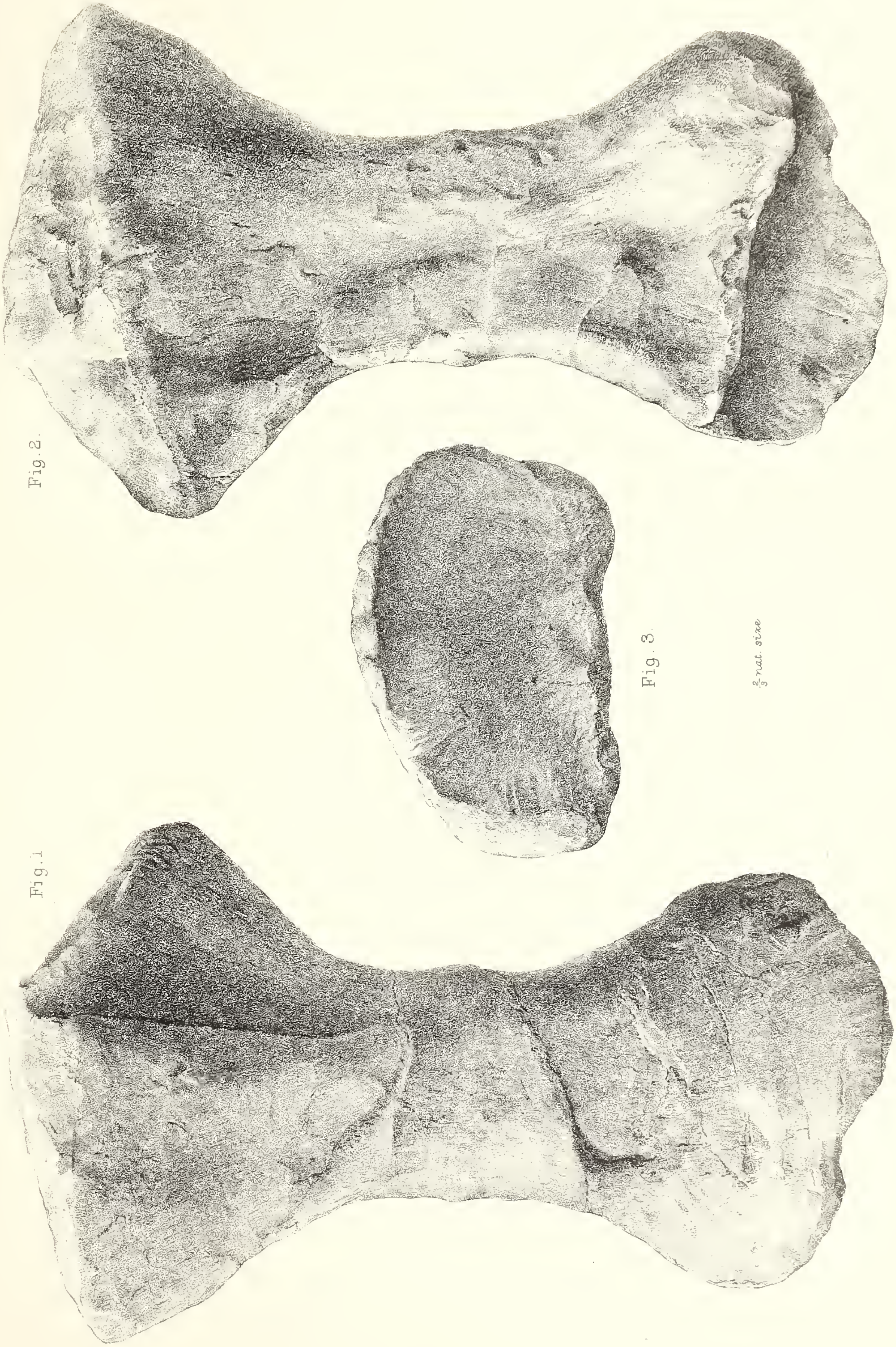


Fig. 2.

Fig. 3.

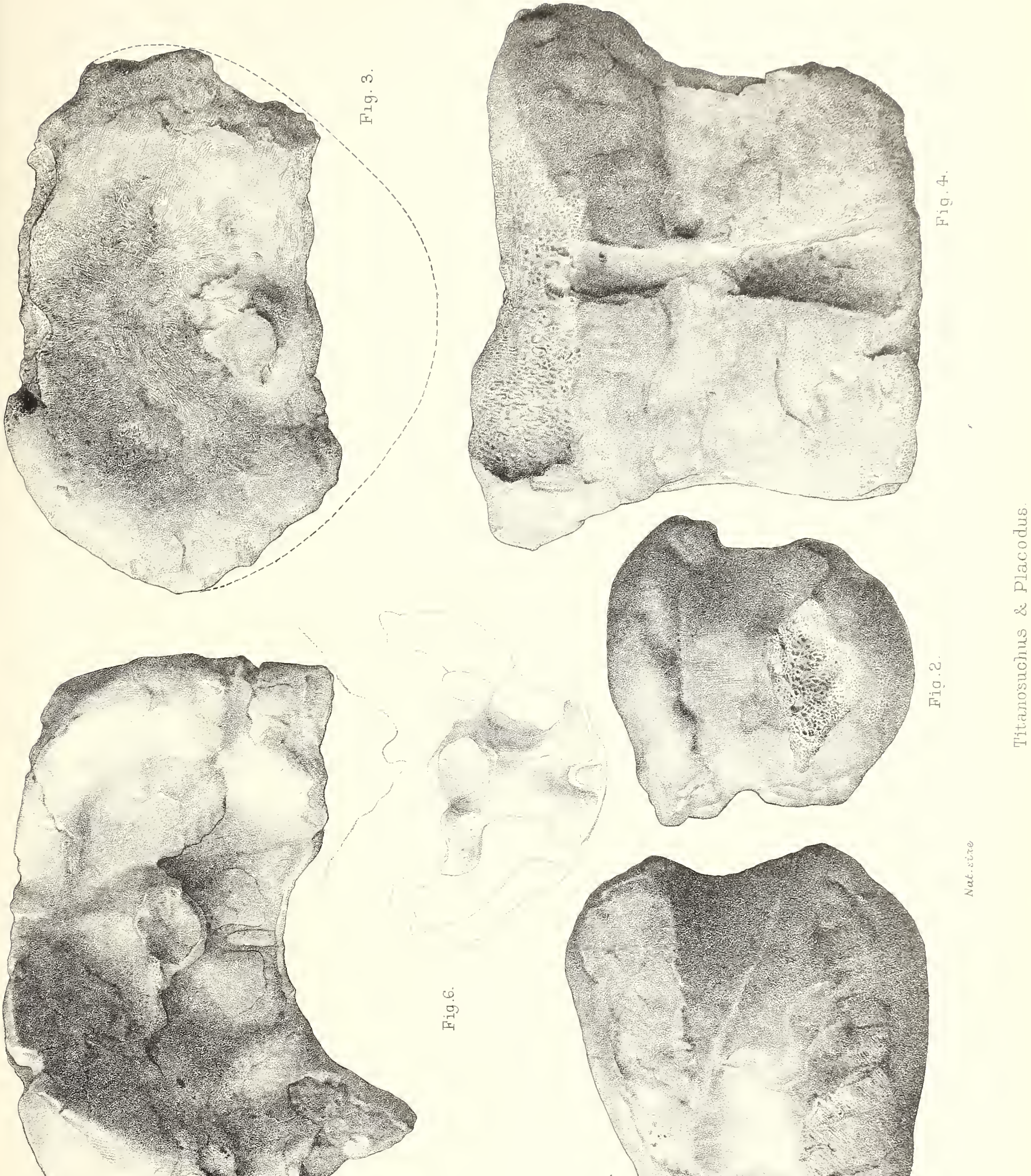
Fig. 1.

$\frac{2}{3}$ *nat. size*

W. H. Wesley, ad nat. lith.

West, Newman & Co. imp.

Ulna.



Titanosuchus & Placodus.

Nat. size

G. M. Woodward ad nat. lith.

West, New

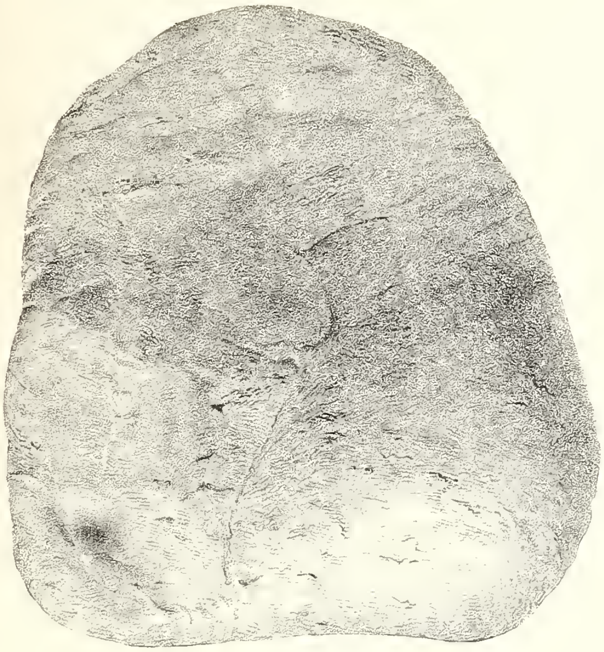


Fig. 2.

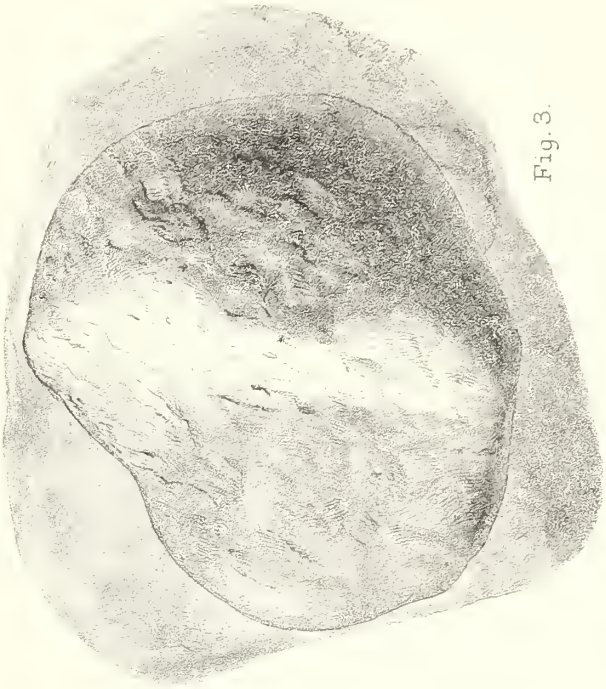


Fig. 3.

Fig. 1.

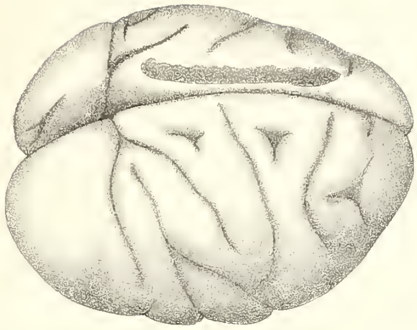
$\frac{1}{2}$ nat. size

W.H. Wesley, ad nat. lith.

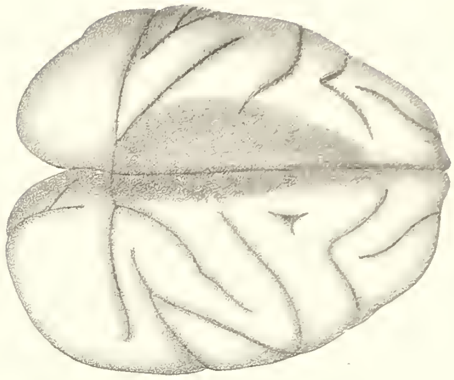
West, Newman imp.

Tibia.

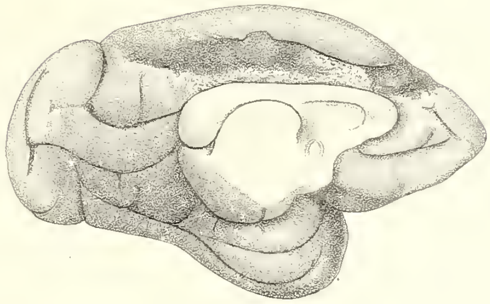
1 *a*



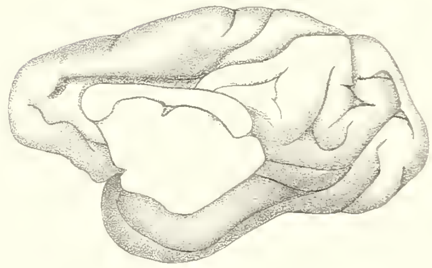
2 *a*



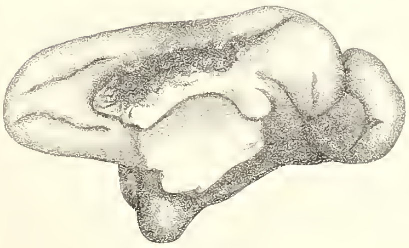
3 *a*



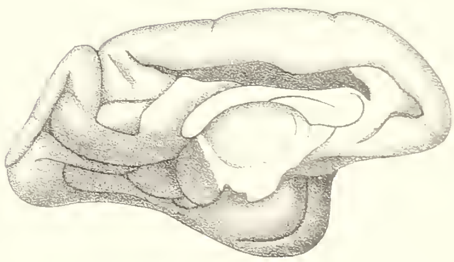
4 *a*



5 *a*



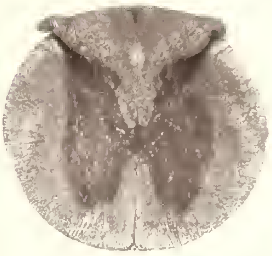
6 *a*



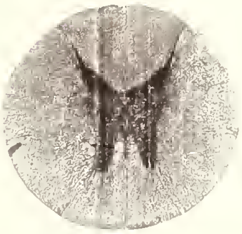
West Newman lith.

1. *b.*

1. *c.*



1. *d.*



1. *e.*

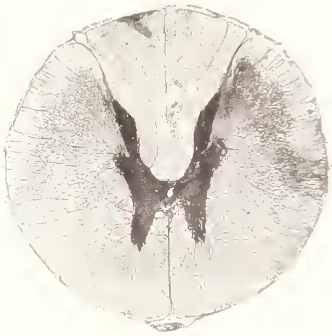
4. *b.*

4. *c.*

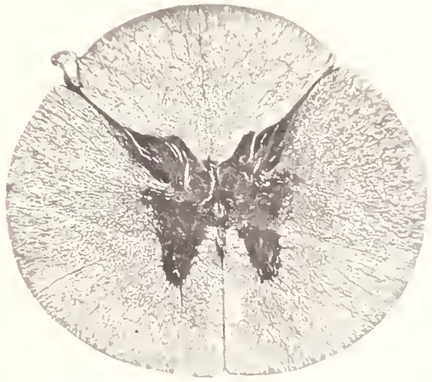
2. *b.*

2. *c.*

2. *d.*



3. *b.*

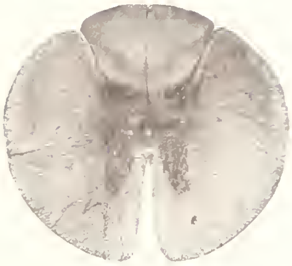


5. *b*

5. C.

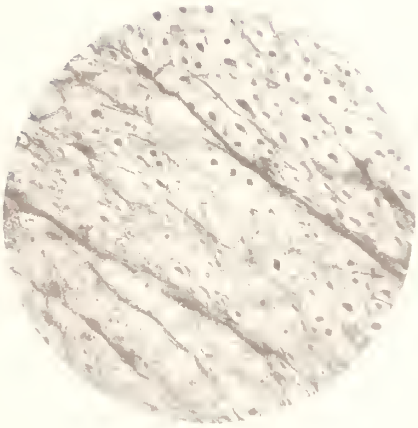
6. *b*.

6. *c*.



7.

8.



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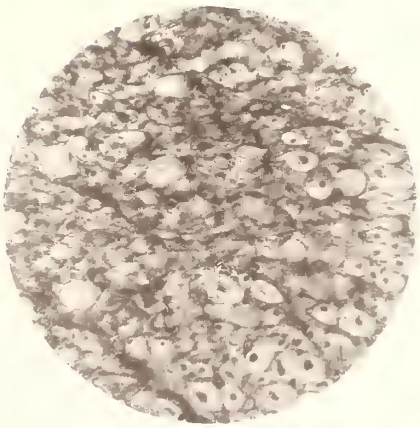

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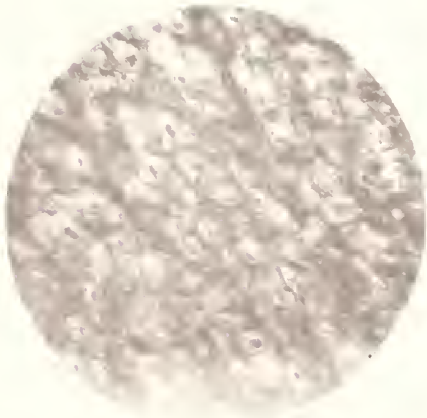

THE PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY.

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—— Part II...	0	17	6
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1845. Part I...	0	16	0
—— Part II...	1	0	0
1846. Part I...	0	7	6
—— Part II...	1	12	0
—— Part III...	1	12	0
—— Part IV...	1	12	0
1847. Part I...	0	14	0
—— Part II...	0	16	0
1848. Part I...	1	0	0
—— Part II...	0	14	0
1849. Part I...	1	0	0
—— Part II...	2	5	0
1850. Part I...	1	10	0
—— Part II...	3	5	0
1851. Part I...	2	10	0
—— Part II...	2	10	0
1852. Part I...	1	0	0
—— Part II...	2	5	0
1853. Part I...	0	18	0
—— Part II...	0	12	0
—— Part III...	1	2	0
1854. Part I...	0	12	0
1854. Part II...	0	16	0
1855. Part I...	0	16	0
—— Part II...	1	6	0
1856. Part I...	2	0	0
—— Part II...	1	4	0
—— Part III...	1	4	0
1857. Part I...	1	8	0
—— Part II...	1	4	0
—— Part III...	1	2	0
1858. Part I...	1	8	0
—— Part II...	3	0	0
1859. Part I...	2	10	0
—— Part II...	2	5	0
1860. Part I...	0	16	0
—— Part II...	2	1	6
1861. Part I...	1	3	0
—— Part II...	1	5	0
—— Part III...	1	7	6
1862. Part I...	2	14	0
—— Part II...	3	0	0
1863. Part I...	1	14	0
—— Part II...	1	7	6
1864. Part I.*..	0	11	0
—— Part II...	1	7	6
—— Part III...	1	10	0
1865. Part I...	2	2	0
—— Part II...	1	5	0
1866. Part I...	1	14	0
—— Part II...	2	7	6
1867. Part I...	1	3	0
—— Part II...	1	15	0
1868. Part I...	2	5	0
—— Part II...	2	0	0
1869. Part I...	2	10	0
—— Part II...	3	3	0
1870. Part I...	1	10	0
—— Part II...	1	18	0
1871. Part I...	1	10	0
—— Part II...	2	5	0
1872. Part I...	1	12	0
—— Part II...	2	8	0
1873. Part I...	2	10	0
—— Part II...	1	5	0
1874. Part I...	2	8	0
—— Part II...	3	0	0
1875. Part I...	3	0	0
—— Part II...	3	0	0
1876. Part I...	2	8	0
—— Part II...	2	8	0
1877. Part I...	1	16	0
—— Part II...	2	5	0
Vol. 168 (extra)	3	0	0
1878. Part I...	1	16	0
—— Part II...	3	0	0
1879. Part I...	2	0	0
—— Part II...	1	12	0
1880. Part I...	2	5	0
—— Part II...	2	0	0
—— Part III...	1	1	0
1881. Part I...	2	10	0
—— Part II...	1	10	0
—— Part III...	2	2	0
1882. Part I...	1	14	0
—— Part II...	2	0	0
—— Part III...	2	10	0
—— Part IV...	1	0	0
1883. Part I...	1	10	0
—— Part II...	2	10	0
—— Part III...	1	12	0
1884. Part I...	1	8	0
—— Part II...	1	16	0
1885. Part I...	2	10	0
—— Part II...	2	5	0
1886. Part I...	1	8	0
—— Part II...	1	15	0
1887. (A.)	1	3	0
—— (B.)	1	16	0
1888. (A.)	1	10	0
—— (B.)	2	17	6
1889. (A.)	1	18	0
—— (B.)	1	14	0

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