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PROCEEDINGS

OF

THE ROYAL SOCIETY.

"On the Relation between Tropical and Extra-tropical Cyclones." By Hon. RALPH ABERCROMBY, F.R.Met.Soc. Communicated by R. H. SCOTT, M.A., F.R.S. Received February 7,—Read February 24, 1887. Revised April, 1887.

The author has long been engaged in the study of cyclones in the temperate zone, as illustrated by those in Great Britain; but as doubts have been expressed by many meteorologists as to the identity between tropical and extra-tropical cyclones, he visited, in the year 1886, the observatories at Mauritius, Madras, Calcutta, Manila, Hong Kong, and Tokiyo, so as not only to procure more published materials for the investigation, but still more to learn from conversation with those who have had great experience of tropical hurricanes, some minute details of weather which were of primary importance, but which could not always be extracted from existing reports.

Though he was not fortunate enough to experience a hurricane himself, still he obtained sufficient information to enable him to arrive at a very definite conclusion on the matter, and now has the honour of laying before the Royal Society the results of his investigations.

He wishes to acknowledge here the assistance he has received from Mr. Meldrum at Mauritius; Mr. Pogson at Madras; Messrs. H. F. Blanford and A. Pedler at Calcutta; Padre Faura at Manila; Dr. Doberck at Hong Kong; and Messrs. Knipping and Wada at Tokiyo.

It will be convenient to sketch first the character of a British cyclone; then to detail the researches in India, the China Seas, and Mauritius; and, finally, to compare the results so as to arrive at a conclusion as to the identity or otherwise of the different kinds of cyclones.

The typical shape of a British cyclone is certainly oval, the successive isobars being non-concentric as in the diagram, fig. 1. The longer diameter of the oval may lie in any direction relative to the path of the cyclone, and often shifts during the existence of the same depression; but on the whole the tendency of the longer diameter is to approximate in direction to the line of the path of the cyclone.

Fig. 1.

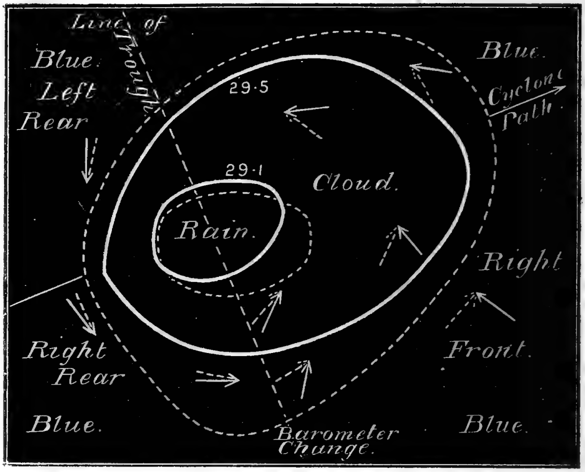


Diagram of cloud and rain in a British cyclone. The full lines are isobars, while the dotted lines define the areas of rain and cloud. The full-line arrows denote the direction of the surface winds, the dotted arrows that of the highest currents.

The side of the cyclone towards which the isobars are packed may also vary indefinitely, but is usually either in front or rear of the longer diameter. On the whole, the compression of the isobars in a typical cyclone seems to be towards the rear of the longer diameter.

The recognition of the oval form of cyclones is of the utmost importance in discussions as to the compass bearing of the centre relatively to the direction of the wind in any part of a storm field.

Another most important point is the fact that a cyclone is not an isolated phenomenon, but an episode in the general circulation of the atmosphere. For instance, most British cyclones are formed on the northern edge of a great anti-cyclone which constantly covers the North Atlantic, while tropical hurricanes are always controlled by their surrounding areas of high pressure.

Before every cyclone, temperate or tropical, there is always a complicated re-arrangement of pressure surrounding the true storm field; and many of the errors which occur in the handling of ships in hurricanes arise from a confusion between the premonitory and actual disturbances. For instance, in the normal distribution of pressure over the West Indies, these islands lie on the S.W. edge of the great Atlantic anti-cyclone, and it is manifest that a hurricane cannot develop as an isolated phenomenon without disturbing the distribution of pressure beyond the immediate sphere of cyclone activity. It is for this reason that a rise of pressure so often precedes both tropical and extra-tropical cyclones.

The typical rotation of the wind round a cyclone is undoubtedly that of an in-going spiral, counter-clockwise in the northern, clockwise in the southern hemisphere. In the commonest British type, when the general distribution of pressure surrounding the cyclone is higher to the south than to the north, the greatest incurvature is in the right or south-east front of the depression. It is important to note this, for we shall find a different position for the most incurved winds in the China Seas and the South Indian Ocean.

The author has not yet found a satisfactory instance of the centre of the wind rotation not coinciding with the *minimum* of the barometer on a synoptic chart.

But when the sequence of wind and barometer trace at any single station is looked at, it is sometimes found that the sudden shift of the wind belonging to the centre occurs either before or after the actual *minimum*. The author has investigated the details of several examples, and found that the apparent anomaly is to be explained on the supposition that the depth of the cyclone was either increasing or decreasing rather rapidly.

For instance, in one case he found that the *minimum* of the barometer preceded the change of wind by three hours, because though the mercury was falling at the rate of 0·03 inch per hour owing to the passage of the cyclone, the depression as a whole was filling up at the rate of 0·07 inch per hour. The balance of rise was therefore 0·04 inch per hour. Full details of this example are given in a work by the author on "Weather," which is now in the press.

The rotation of the upper winds may be briefly stated thus:—At a level of about 4000—6000 feet the wind is nearly parallel to the isobars, and above that height tends to blow more and more outwards; but the amount of out-curvature varies in different parts of a cyclone, and need not be particularised here.

As a consequence of this we get the following law of the vertical succession of wind currents from the surface of the earth:—Stand with your back to the wind, and the successive layers of cloud will come continually more and more from your left hand. In the southern hemisphere the succession is reversed; that is to say, the upper currents come more and more from the right.

In fig. 1 the surface winds in a typical British cyclone are marked by full-line arrows, while the direction of the highest cirrus is denoted by dotted arrows.

The distribution of rain, cloud, and weather generally in a British cyclone may be described thus. An area of rain, surrounded by a ring-shaped district of cloud, is associated with every cyclone. The rain area is not exactly concentric with the isobars, as it usually extends further in front than in rear of the centre of the cyclone, but still both rain and cloud are obviously related to that point (see fig. 1). But, strange to say, the kind and character of the cloud and the relative warmth and dampness of the weather are not related to the centre but to the front and rear of a line drawn through the centre and more or less perpendicular to the line of propagation of the cyclone, marked "trough" or line of barometer change in the diagram, fig. 1. For instance, the whole of the weather in front of that line is warm, muggy, and damp, while all in rear is cold and dry. The effect of this is that when a cyclone drifts past an observer, he experiences a sudden change from warm and damp to dry and cold weather the moment the trough has passed over him.

Then as to the kind of cloud.—A thin ring of halo forming cirro-stratus or cirro-nebula fringes almost all the outside of the front edge of the main cloud ring, but this kind of sky is rarely seen anywhere in rear of the cloud ring of the cyclone. Moreover all the cloud in front of the trough is usually more or less stratiform—cirro-stratus and strato-cumulus—while that in rear is distinctly of the cumulus type.

The portion of the line of the trough that is drawn southwards from the centre marks out the position of a long line of squalls, and of a very much more sudden shift of wind than the symmetry of the cyclone might have suggested. Instead of veering steadily from S.E. to N.W. the wind jumps during the passage of the trough sometimes as much as from S.S.W. to W.N.W., and then veers more gradually.

Considering then both cloud and wind, the sequence of weather to a solitary observer as a cyclone is propagated over him, the centre passing to the N., will be as follows:—Cirro-stratus and halo will appear in a blue sky, while the wind comes from about S.E. Gradually the clouds get lower and heavier, as they gather into strato-cumulus; then rain comes on, while the wind has veered steadily to S.W.

All this time the barometer has been falling fast, but suddenly just as the mercury has reached its lowest point a violent squall comes on, during which the wind jumps suddenly three or four points from the S.W. up to W. or W.N.W. Almost directly after, unless very near the centre of the cyclone, the clouds take the form of cumulo-nimbus, and then of simple detached cumulus, till the sky is completely blue again.

Any number of observers, situated on the southern side of the path of the centre, will find their weather change suddenly just as the mercury has begun to rise; and we, therefore, conclude that a line drawn across a cyclone at any moment, through all points where the barometer has just turned upwards owing to the onward motion of the depression, divides the cyclone into two halves which have different physical characteristics.

This line may be called the "trough" of a cyclone; and the characteristic squall, the sudden rise of the barometer, and the jump of the wind associated with the passage of that line, together with the different character of the weather and clouds in front and rear of that line, may conveniently be classed together as the "trough phenomena" of a cyclone.

The word "trough" requires some explanation, as that term has been strongly objected to by some. The difficulty of realising the applicability of the word arises from the difficulty of realising the forward motion of a cyclone when looking only at a set of oval isobars. But when it is considered that a cyclonic vortex is propagated somewhat after the manner of a wave; and that a barograph during the passage of the cyclone traces out a wave-like curve, the word trough comes in naturally to denote the line drawn across the isobaric plan of a cyclone to show all the points where the *minimum* has been attained, owing to the motion of the depression.

A cyclone is really a complex moving eddy of air. For some reason or other, pressure decreases under this eddy in a manner which is conveniently mapped out by isobars. In ordinary language the oval isobars are called for shortness a cyclone, but of course they are really only the symbol of what is taking place in the air overhead.

Isobars give the plan, barograms the sections of a cyclone, and it is always difficult to realise the relation of one to the other; but there is no difficulty in the conception of a moving vortex of air, which of course would have a trough like an ordinary wave.

A stationary vortex, like a stationary cyclone, would have no trough. Suppose a stationary cyclone to form over an observer and then to die out. His barometer would first fall and then rise, but there would be no trough, and he would experience no trough phenomena.

It is important to notice that the line of the trough does not appear to be always at right angles to the path of a cyclone, whose longer diameter is not perpendicular to the line of propagation; and that the trough phenomena are far more marked on the southern than on the northern side of the centre in Great Britain.

Taking a general view of the phenomena of a cyclone, they appear to have a double symmetry—a symmetry round a point, and a symmetry about a line. The rain area, cloud ring, and general rotation of the wind are obviously related to a point, the centre of the cyclone. On the contrary, the quality of the heat, the relative humidity, the character of the clouds, and a particular line of squalls have nothing to do with a point, but are related to the front and rear of the line of the trough.

No attempt has been made as yet to discover the origin of these trough phenomena; but from the results of his researches in th tropics, the author was led to consider the relation between the intensity of the trough phenomena and the velocity of propagation of the cyclone. For this purpose he examined the meteorograms, published in the 'Quarterly Weather Reports' of the Meteorological Office, for all well-defined cyclones in the years 1872–76, and for part of the year 1879. The intensity of the trough phenomena was gauged by the amount of the effect of the squall on the barometric trace, and the suddenness of the shift of wind at the same time.

The result is given in the following table, by which it is evident that there is a decided relation between the velocity of the cyclone and the intensity of the trough phenomena.

Table showing the Connexion between the Intensity of the Trough Phenomena and the Velocity of a Cyclone.

Year.	Month.	Day.	Velocity of cyclone. Miles per hour.	Intensity of trough phenomena.	Remarks.
1872 ...	Aug.	10	16	Very slight	
" ...	Sept.	25	6	No trace	
" ...	Oct.	10	13	Very slight	
" ...	Dec.	8	19	Slight	
" ...	"	16	16	"	
1873 ...	Aug.	28	16	"	
" ...	Sept.	15	19	"	
1874 ...	April	13	27	Moderate	
" ...	Aug.	13	13	Slight	
" ...	Oct.	21	24	Moderate	
" ...	Nov.	29	12	Moderate (Kew)	Strong at Falmouth; earlier in course when velocity unknown.
1875 ...	Jan.	21	48	Strong	
" ...	"	24	25	Moderate	
" ...	Sept.	26	29	Strong	
" ...	Nov.	19	35	Moderate	
1876 ...	March	12	40	Strong	
" ...	"	14	6	Very slight	
" ...	"	25	10	No trace	
1879 ...	Dec.	28	70	Very marked	The Tay Bridge storm.

The author has made many efforts to find traces of a central spot of blue sky in the centre of a British cyclone. He has never observed one himself, and though such cases have been reported, he is not satisfied with the evidence. It is very common to see a patch of blue sky directly after the passage of the trough, some distance from the centre on the southern side of a cyclone, and soon to experience wind and rain continuing for some time afterwards; but this is quite different from the central calm and blue sky of a tropical hurricane. Though he cannot say that a clear-centre cyclone never occurs in this country, it is perfectly certain that the phenomenon is very rare.

Indian Cyclones.

Attention will now be directed to Indian cyclones.

The result of the author's investigations, both of the published records of the various Indian meteorological departments and from verbal communications, gives the following as the general character of Indian cyclones.

The shape is usually oval; but the side towards which the isobars are pressed, and the consequent lie of the longer diameter, varies much, even in the same cyclone on different days.

The diameter is smaller than in the Atlantic; but pressure diminishes much more rapidly near the centre than in Europe, though the actual *minimum* need not be very low.

The general surroundings, the formation, development and dissipation of cyclones seem to be essentially the same as in Europe. There are two types of cyclones in the Bay of Bengal. Those in May are associated with the breaking up of a belt of high pressure, in a manner to which we find many analogies in Great Britain; while those in October are formed in a general depression over the Bay, exactly analogous to the commonest conditions of cyclone formation in the Atlantic.

Secondaries sometimes form on the side of the primary cyclone, but not nearly so often as in the temperate zone.

The motion of the cyclone is almost invariably towards the N.N.W. or N.W. at first, with a marked tendency to recurve towards the N. and N.E. later on.

The velocity of translation is much less than in Europe, varying from about 3 to 20 miles an hour.

The rotation of the wind is always in an in-going spiral; but there does not seem to be any marked tendency to greater or less incurvature in any particular quadrant of the cyclone. The violence of the wind seems to increase as the centre is approached, which is not the case in the British Isles.

The author does not think that there is sufficient evidence for the assertion that the centre of wind rotation is not coincident with the barometric centre of the cyclone. In the cases that have been reported on board ship, which he has investigated, he is unable from the materials at his disposal to separate the barometric change due to the motion of the ship in her course from that due to the motion of the cyclone.

For instance, if a ship were motionless, and a cyclone passed over her, the whole of the changes in the height of her barometer would be due to the motion of the cyclone. If the cyclone were both motionless and of constant depth, while she moved, the barometric changes would be due to her passage from a place of higher to a place of lower pressure, or *vice versâ*. In practice, the ship and cyclone are always both moving, and as an additional complication the cyclone is often increasing or decreasing rapidly in depth. Hence the difficulty of dealing with observations near the centre of a small cyclone. Suppose a ship was lying-to a short distance in front of the central point of the cyclone, but that the depression was filling up very rapidly. Her barometer would begin to rise before the centre passed over her, while the wind would not change till the vortex had passed. Then it would probably be reported that the centre of wind rotation was not coincident with her barometric *minimum*. The centre of wind rotation would, however, have been always really coincident with the centre of the isobars at any particular moment. The whole question deserves further attention. It should be noted that the blue centre is also sometimes reported as non-coincident with the barometric *minimum*.

There are very few observations on the motion of the upper clouds, but so far as they go, the general circulation of the air in a Bengal cyclone appears to be identical with that in higher latitudes.

There are no sufficiently detailed observations to enable us to construct synoptic charts of the distribution of rain, cloud, &c., round a cyclone; but by generalising the sequence of weather in many such depressions, we find squalls, showers, and dirty-looking clouds all round the cyclone, with more violent squalls and torrential rain surrounding the centre. The actual centre is always calm; and though blue sky does not seem to have been always observed, a cessation of rain is usually reported.

This clear "bull's-eye" is the most characteristic feature of a tropical cyclone. Otherwise the only difference is that the rear of the disturbance is not so clear in Bengal as in temperate cyclones; and that squalls are formed all round the centre instead of only in the right rear of the depression.

The sequence of weather to any observer appears to be as follows:—The first indication is always a strange coloration of the sky at sunrise and sunset. The author has examined this point carefully, and discovered that abnormal colours are developed not only before the barometer has begun to fall at the place of observation, but before any appreciable depression can be found anywhere on the synoptic charts. He has observed the same in England, and the fact is important, as it proves that the examination of synoptic charts can never supersede the necessity of observations on the appearance of the sky.

As the cyclone approaches, a peculiar uneasy way of blowing of the wind is sometimes reported, and the sky grows dirtier and dirtier till the rain appears to grow out of the air. This has always been described to the author as characteristic of cyclone rain, in opposition to the showery rain from cumuloform clouds which is typical of the ordinary precipitation of the monsoon.

This peculiar cyclone rain seems to be only an intensification of the rain associated with cyclones in Great Britain. There we see the blue sky grow pale and sickly, and then grey, till rain falls from a uniform gloom, and not from any defined cloud. In thunderstorm rain, on the contrary, we see mountainous cumulus above the rain.

Sometimes the front of an Indian cyclone is accompanied by thunder and lightning; but all observers are agreed that the absence of electrical disturbance is a sign of very bad weather.

As the cyclone approaches the rain increases, and the wind rises into the characteristic squalls of a hurricane.

The author has been able to find very few signs of any trough phenomena during the passage of a Bengal cyclone. Sometimes a rather sudden shift of wind is described, and a squall with a sudden jump of the barometer, just as is so common in England; but there does not appear to be the immediate change in the character of the clouds which occurs in these islands.

Almost all reports agree that the weather is quite as bad in rear as in front of the cyclone; and the clouds in rear seem to retain their characteristic wild and dirty appearance. The bad weather, however, usually extends much further in front than in rear of the centre.

Besides these primary cyclones there is another class of small oval depressions, which might either be called primary or secondary, according to the judgment of the meteorologist.

These form in the Bay of Bengal during the rainy season—from June to September—and though they are much less intense than the storms we have just described, their shape and the rotation of wind round them unmistakeably define their cyclonic nature. The wind round them is never strong; their special characteristic is rain. It has also been observed that they traverse the land and moderate mountain chains without material disturbance of their shape, while the great cyclones are invariably broken up or deflected as soon as their centres touch the coast.

It has been suggested, with a great deal of probability, that these small cyclones are formed at a higher level in the air than the larger ones. They will not be further considered in this paper.

The following examples will illustrate these facts. In figs. 2, 3, and 4 we give synoptic charts for India and the Bay of Bengal for the three days May 16—18, 1877. These and the extracts from ships' logs are derived from an exhaustive memoir by Mr. J. Eliot, Meteorological Reporter to the Government of Bengal, entitled 'Report on the Madras Cyclone of May 1877.'

The broad features of these three days are very simple. In all the highest pressure lies over Burma and the eastern side of the Bay of Bengal, while a cyclone which has formed to the east of Ceylon passes up the western side of the Bay in a generally northerly direction. On every day the cyclone is oval, the longer diameter approximating in direction to the line of propagation; but while the isobars are packed towards the rear of the cyclone on the first two days, the centre is very decidedly pressed towards the front on the last day.

The cyclone increased half an inch in depth between the first and second days, and the last day especially the steepness of the central

Fig. 2.

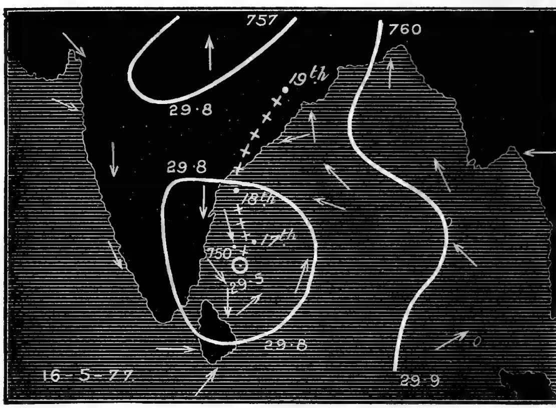


Fig. 3.

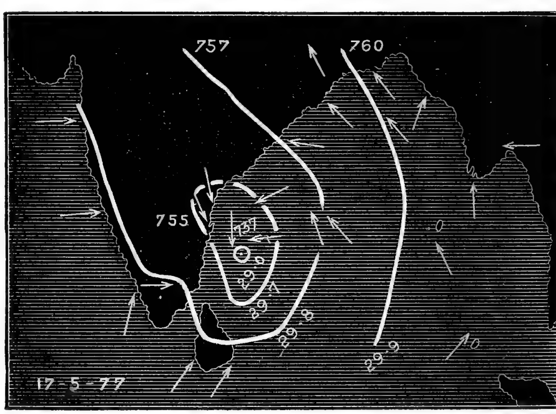
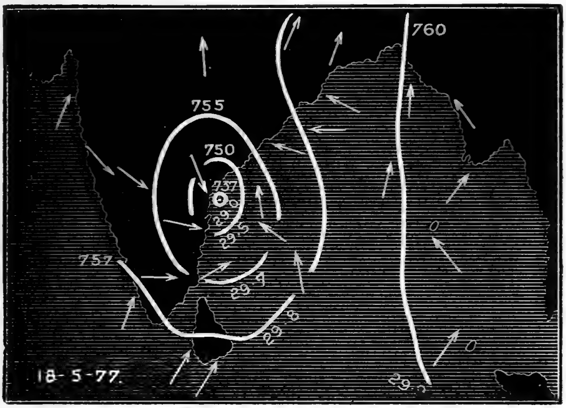


FIG. 4.



Figs. 2 to 4 : Cyclone in the Bay of Bengal. May type.

depression is very noticeable. The small scale of the charts does not permit of the isobars being drawn at regular intervals, or this fact would have been still more obvious. The numbers 760, 757, &c., are the approximate equivalents of the isobars in millimetres.

The path of the cyclone is given in fig. 2. The velocity of propagation was—

From 16th to 17th	......	4·3	miles per hour.	
„ 17th „ 18th	......	8·7	„	„
„ 18th „ 19th	......	17·5	„	„

The rotation of the wind as an in-going spiral, counter-clockwise, is very obvious, and calls for no remark.

The following extracts from ships' logs will sufficiently illustrate the sequence of weather in this cyclone:—

The "Mistly Hall," on the west side of the cyclone, remarked a fiery sunrise as early as the 14th May, before the barometer began to fall. On the 15th and 16th she experienced rain, lightning, and squalls from N.E. to N. On the 17th, two hours after the barometric *minimum*, the wind went from N.W. to S.W., with lightning, torrents of rain, and terrific gusts. Twelve hours later the storm moderated.

In this report we see little trace of trough phenomena. The apparent non-coincidence of the wind sequence with the barometric *minimum* cannot be fully discussed for want of sufficient observations. There does not appear to have been any rapid filling up of the cyclone this day, as, on the contrary, the lowest reading report on the 17th is 29·043 inches, and on the 18th rather less, viz., 28·95. But we may note a transference of the centre from the rear to the front of the depression, and we cannot determine the course of the ship from the published log.

The "Bride," also on the west side of the cyclone, reports a very similar sequence of weather, and no marked trough phenomena.

The "Witch," which passed through the vortex, ran out of Madras Roads on the 16th, with threatening weather, thunder, lightning, and St. Elmo's fire on her mastheads. On the 17th she ran before the N.W. wind, with a heavy gale and more thunder and lightning, right into the vortex. Then she experienced a calm for 10 minutes, when the wind flew to the S.W., with a sudden rise of 0·2 inch in the barometer, and thunder, lightning, and rain for 12 hours afterwards. Then the gale began to break, but the sun rose on the 18th with a pale sickly sky, like gold, green, and blue all mixed together.

What we have most to notice here is the amount of electrical discharge all across the cyclone, the sudden rise of the barometer as the first squall burst from the S.W.—exactly analogous to what we so often see in England,—and the dirty sky in rear of the whole disturbance.

The "Oxfordshire" also passed through the vortex only a day after the "Witch." The weather was similar in both cases; but the mercury began to rise on board the former at least an hour, and to the amount of 0·2 inch, before the wind jumped to the S.W. The cyclone was, however, filling up fast now, for while the lowest reading on the 18th was 28·95 inches, that on the 19th was only 29·36, a difference of nearly 0·4 inch.

The "Asia" passed on the east side of the cyclone. She experienced rain and squalls on the 16th, and on the 17th also rain, squalls, and lightning, till at 5 A.M. that day the wind veered suddenly from N.N.E. or N.E. to E.N.E., just at the lowest point of the barometer. About four hours later the storm seemed to collapse rather suddenly. Here we may note a sudden shift of wind at the passage of the trough.

There is only one observation on upper clouds. The Master Attendant at Masulipatam reports that on the 19th, while the wind was from the E., the scud had a more southerly motion. This is the normal vertical succession in the northern hemisphere.

The above exemplifies the May type of Indian cyclone; the next illustration will show the more violent October type.

In figs. 5, 6, and 7 are given reductions of a cyclone which passed up the Bay of Bengal from October 30 to November 1, 1876. These and the extracts from ships' logs are entirely derived from another exhaustive memoir by Mr. J. Eliot, entitled 'Report of the Vizagapatam and Backergunge Cyclones of October, 1876,' our diagrams referring to the latter cyclone.

On the first chart for October 30, fig. 5, a large oval area of low pressure covers the Bay of Bengal; the probable position of the vortex is marked by a small circle, but no reading can be given there for want of observations.

By next day, October 31, fig. 6, the centre has moved towards the N.N.E., and the cyclone has increased enormously in depth. The lowest reading for the day, about 20 miles west of the vortex, was 28·1 inch. As the next isobar is marked 29·6, the steepness of the

FIG. 5.

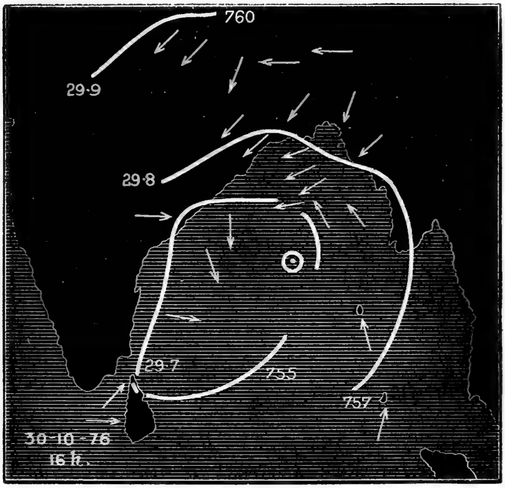


FIG. 6.

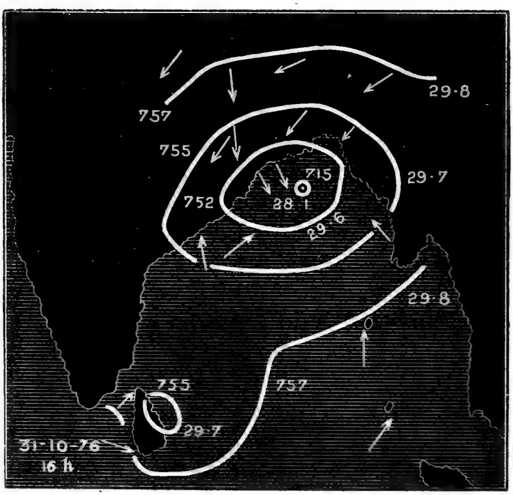
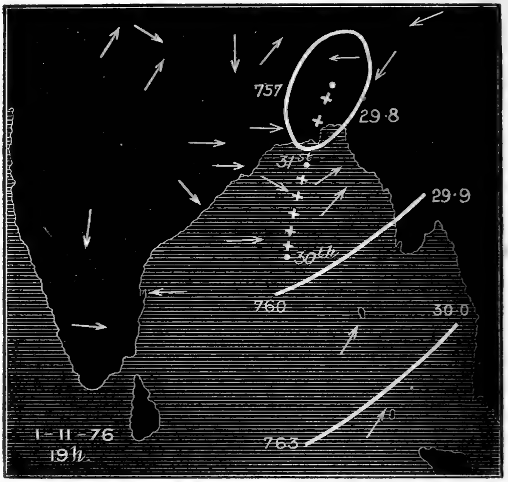


Fig. 7.



Figs. 5 to 7 : Bengal Cyclones.
The Backergunge Cyclone.

central depression is of an amount unknown in higher latitudes. The longer diameter of the cyclone lies about east and west, or nearly at right angles to the path of the disturbance. The portion of the general depression which lay near Ceylon on the previous day has now developed into a well-defined secondary over that island. This is very interesting, as we very often see precisely the same thing in our own country, when a long oval depression gathers itself up into two regular cyclones, or into a primary and secondary.

By the third day, November 1, fig. 7, the cyclone is dying out over the delta of the Ganges, and the longer diameter is now nearly in a line with the direction of propagation.

The velocity of translation appears to have increased irregularly, but on the mean the rate was 9 miles an hour between the 30th and 31st, though during the last six hours the rate increased to 12 miles an hour, and no less than 22 miles an hour between the 31st October and 1st November.

The ship "Tennyson" passed about 20 miles west of the vortex. So early as the 28th the clouds had a bad appearance at daybreak. On the 29th the weather looked very bad, with lightning and squalls, while the barometer had begun to fall fast. During the 30th she had rain with furious gusts, but no lightning. By 11 A.M. on the 31st the wind went from N.E. to N.N.E., by noon to N., and by 1.30 P.M.—just as the barometer reached the *minimum* of nearly 28·0 inches—to N.N.W. with a perfect howl, but no rain fell from then to 6 P.M. No lightning was seen any time during the day, and next morning was beautifully clear.

In this description the absence of lightning in the "kernel" of the cyclone is very noticeable; also, to a certain extent, the difference between the threatening clouds a long way in front of the centre and the beautifully clear sky in rear.

Thus, when we combine this with the sudden shift of wind and increased howl just as the mercury touched its lowest point, we see slight traces of trough phenomena.

The "Palmas" also passed to the west of the centre, and experienced slight trough phenomena. On the 31st she encountered thick rain with increasing gale from N.E., while the barometer fell fast. At 6 P.M., when the mercury marked 28·20—apparently its lowest point—she had the heaviest blow, the wind hauling to N.N.W. and N.W., with fearful vivid flashes of lightning, thunder, and rain. By 7 P.M., one hour later, the gale began to abate.

The "Allahabad" passed on the east of the centre. By the 29th a brickdust sunrise was observed, and by the 30th the weather became threatening, the wind from S. to E., but the clouds from S., with squalls and constant rain. On the 31st the cyclone commenced with fury from E.S.E., barometer falling fast with rain and lightning, and by 8 to 10 P.M., when the mercury touched its lowest point, the wind veered to S.E. and S.S.E. to S., with lightning and heaviest blow. By noon the next day the weather was fine.

Here, and in all the other logs, we should notice how much further the bad weather stretches in front than in rear of the cyclone. This also is exactly in accordance with our experience of temperate zone cyclones.

At Noakholly, on the Sunderbund coast, the moon shone clear in the "bull's eye" of the cyclone; and the observations point to the existence either of two separate vortices, or of a single oval one whose longer diameter was perpendicular to the line of propagation of the whole cyclone or parallel to the trough.

Typhoons in the China Seas.

Manila.—The author will now consider the nature of typhoons in the China Seas. These are very valuable, as they can be traced from their origin in the Philippines till they gradually acquire an extra-tropical character in the Japanese Islands. The notice will therefore commence with the author's researches in Manila, where, through the courtesy of Padre Faura, he obtained an immense amount of information, which has not hitherto found its way to England.

The general shape of typhoons in the Philippines is undoubtedly oval, but the side towards which the centre lies is not only variable, but changes during the course of the same cyclone. On the whole, the centre seems to have a tendency to lie towards the front of the typhoon.

There seems to be very little tendency for cyclones to form secondaries in these latitudes, and therein they differ greatly from depressions in the Atlantic.

On the other hand, we find the same tendency for two typhoons to follow one another closely, and along the same path, which is such a characteristic feature of extra-tropical cyclones. For instance, the typhoon of October 20, which will be illustrated presently, had scarcely died out on the 22nd to the south of Hainan, before a new cyclone formed on the 23rd to the south of Panay—a little south of Manila—which also died out near Hainan, on the 27th. Then by November 3rd, another typhoon formed to the east of the Philippines, and traversed a line almost coincident with the path of the first cyclone.

The intensity of typhoons in the China Seas appears to be about the same as the October cyclones in the Bay of Bengal, but less than that of the hurricanes of Mauritius and of the West Indies.

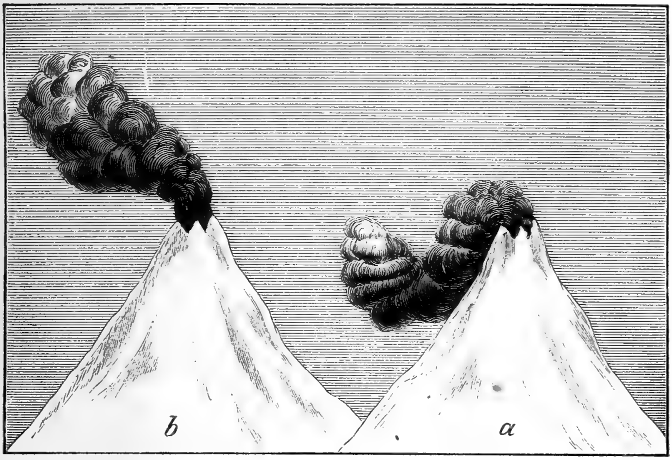
In the Philippines, as elsewhere in the tropics, a rise of the barometer very frequently precedes a typhoon; and much has been written about the relation of this high pressure area to the cyclone itself. No synoptic charts which have yet been constructed in the tropics are sufficiently detailed to enable us to say much on the subject; but so far as they go, the changes, or re-adjustment of pressure surrounding such an abnormal occurrence as a tropical hurricane, are precisely analogous to the changes which precede a temperate cyclone.

The weather in this high-pressure area is reported as beautifully fine, often with great visibility; and the formation of feathery cirrus on the blue sky is one of the first and most certain forerunners of a typhoon. This is exactly analogous to the "wedges" of high pressure, and very fine weather, which we find in front of many European cyclones.

But there is one phenomenon connected with the outskirts of a typhoon that has been observed by Faura, and which is so important and interesting that it may be described here. In the periphery of the cyclone, that is to say in the winds blowing two or three days before the storm from N. and N.W., the smoke of the chronically active volcano of Abayon, in the island of Albay, descends. During the typhoon no observations have been possible, owing to the low-lying nimbus; but when this breaks up, after the passage of the hurricane, the smoke is rising.

Fig. 8*a* represents the appearance of the smoke before, fig. 8*b* that after a typhoon, as shown in some sketches belonging to Padre Faura.

FIG. 8.



Volcano of Abayon.
a. Before typhoon.
b. After typhoon, and usually.

These sketches were shown to the author as proofs of a descending current in front of a typhoon, but he is not altogether satisfied with the evidence. It is very difficult to decide on such a question without personal observation. It has been suggested that the downward curl of the smoke was merely the vortex eddy on the lee side of the volcano, and that a similar phenomenon is obvious in any factory chimney during a high wind. The original sketches, however, did not the least convey that impression; and there must be some reason why the smoke does not curl that way with other winds. In England there is a well-known saying, that when smoke falls down rain may be expected. This is often true, and is undoubtedly due to some peculiarity in the way of blowing before certain kinds of rain, and not merely to the velocity of the wind. The author believes that the downward curl of the smoke of Abayon is due to some peculiarity in the wind preceding a typhoon, though he is not prepared at present to assume the existence of a regular downward current in the high pressure area that precedes the typhoon.

Some physicists have apparently doubted the existence of downward currents at all, on the ground that the stream lines of an air current must be tangential to the earth's surface, and that wind cannot slant downwards in the ordinary sense of the word. This would be perfectly true, if air were a perfect fluid, and wind blew like a continuous theoretical current; but in practice wind blows irregularly, and forms eddies and vortices that break the continuity of the stream lines. As a matter of observation, nothing is commoner than to see a gust of wind blow down from a mountain on to a lake, and then ricochet up again.

The wind round a typhoon is always an in-going spiral. The incurvature is usually strong, but differs in every segment, not only in each cyclone, but even in the same cyclone at different periods of its existence. So much is this the case, that it seems impossible to give any mean incurvature for the Philippines. This is curious, for farther north the variation of incurvature seems more constant.

The motion of the lower nimbus is always more nearly parallel to the isobars, and therefore more nearly eight points from the bearing of the centre, than the surface winds. This is identical with the experience of European cyclones, and has an important bearing on the rules for handling ships at sea, though that question cannot be discussed here.

The observations of Padre Faura on the variation in the destructive effects of the wind in different parts of a typhoon, and the manner in which he uses this knowledge in forecasting, are so important and so unknown in this country, that the author proposes to devote one or two paragraphs to their consideration.

Faura finds that the position of the strongest winds relatively to the centre of the cyclone varies not only in every typhoon, but in the same one; and also that for the same velocity, the wind will unroof houses in one cyclone, but do comparatively little damage in another. He assumes that the unroofing wind is blowing slightly upwards, and the wind that does little damage moves horizontally or slightly downwards.

If then he can find evidence that the wind is rising up in front of a typhoon, he can be pretty certain that it will descend in rear; and therefore can and does forecast that the wind in that portion of the storm will be less destructive.

From all this, he has been led to the conception that the axis of a typhoon is inclined, and that it nutates during the progress of the vortex.

For instance, in the typhoon of October 20th, 1882—figured below—he believed that the wind in front was certainly rising and low; a beam lying near the sea, at a distance of 300—400 metres, was thrown upon the observatory (height 34 metres) and destroyed the Beckley's anemometer, while the roofs of many houses were pulled off. From this Faura concluded that the rear would not have the same force as the front, because the wind would be high and falling; and he published a notice at noon on the 20th—"The S.W. winds will be very short."—This prognostication was justified by the event.

The contrary took place in the typhoon which followed a few days later on November 4th and 5th, the wind falling, as he considered, in front, and doing comparatively little damage.

Again in the typhoon of August 19th, 1881, the wind blew during eight hours with a rapidity of 44 metres a second, and nevertheless did not damage the roofs, because it did not hit from beneath, but from above.

These facts are very interesting, as similar observations on the variable destruction for equal velocities have been made by Piddington in Bengal; and the same has been observed on a much less pronounced scale in Great Britain. The utilisation of the idea in forecasting has, we believe, never been suggested by anyone except Faura; and his views are worthy of the most attentive consideration from those engaged in forecasting hurricanes.

Faura thinks that this can all be explained by the supposition that the axis of a typhoon is inclined to the earth's surface, instead of being truly vertical, and that the whole system nutates like a top.

The author, however, believes that there are insuperable difficulties in the conception of a nutating axis of a cyclone, though he does not propose to discuss the question in this paper. The idea that the variation of destruction for the same velocity was due to a slant in the wind has often been suggested before, and though slanting gusts undoubtedly occur, he is not prepared to pronounce a definite opinion on the question, from the evidence at present available. His object in introducing the subject here, was to point out how the variability of destruction can be used in practical forecasting, even if Faura's theory cannot be maintained as a whole.

The weather in and surrounding a typhoon is very characteristic. The whole storm is surrounded by a ring of cirrus. Halo is an almost constant precursor of a cyclone, and is sometimes seen in rear. The author made special enquiry on this point, for herein tropical differ very much from extra-tropical cyclones. In Great Britain we rarely see cirrus of any description in rear of a cyclone, still less the formless ice-dust called *cirro-nebula* by Ley, which is almost essential to the formation of halo.

The form of cirrus in this ring, especially in front, is typically that known as "cat's tails" or "cock's plumes" (*rabos de gallo*); and there seems to be a tendency for the lines of cloud to radiate from the vortex. This idea was first suggested by Padre B. Viñes of Havana, but requires further investigation, for the author was informed at Hong Kong that they had not observed that allineation there.

All observers are agreed that here, as elsewhere, cirrus is seen before the barometer begins to fall.

Inside the cirrus comes a ring of dense black cloud, and then a smaller ring of heavy rain and dense nimbus. When a typhoon can be observed approaching at a distance, mountainous cumulus are seen over this central rain.

But, just as in higher latitudes, the clouds in front are dirtier than those in rear of the trough; for when, in the latter portion of the typhoon, the fracto-cumulus begins to break, either blue sky or firm fracto-cumulus becomes visible.* Thus trough phenomena appear to be only slightly marked in the Philippines, but very much information on this point could not be procured.

Lastly in the vortex of the typhoon we find the "bull's-eye" or calm clear spot, a few miles in diameter, surrounded on all sides by the fury of the hurricane. Many accounts received agree in saying that light cirri are usually seen over this area, that near land this space is full of birds taking refuge from the terrific surrounding squalls, and that the heat is suffocating. During the passage of the vortex over Manila, on the 20th October, the thermometer rose rapidly, and the relative humidity decreased to an extent hardly known in the driest months. The universal exclamation was "the air burns." In the succeeding typhoon, November 4th and 5th, no such central heat was observed, but the intensity of the cyclone was much less.

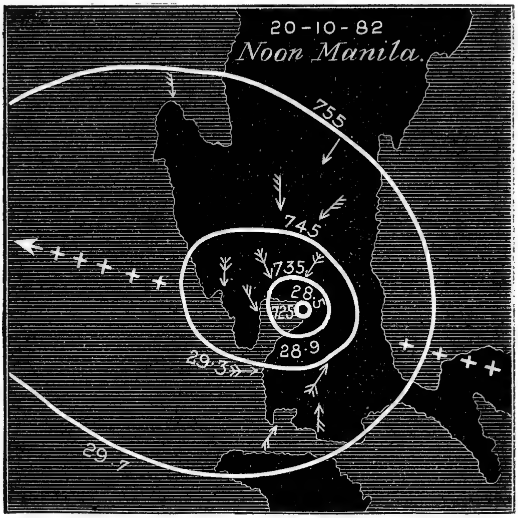
The instrumental observations at Manila on these two occasions are almost unique; but there is no doubt that a clear hot central spot is a normal feature of tropical cyclones. The oppressive heat and great dryness would probably point to the existence of a slight down draft in the core of the cyclone. It is extremely difficult to form a definite conception of such a system of circulation, for there is not the slightest doubt that the main mass of air in the centre of a cyclone is rising. The only reasonable suggestion which has yet been made, has been proposed by Vettin and Sprung, who think that under certain conditions a local downward eddy is formed in the centre of an atmospheric whirl. We must, however, consider the question to be as yet unsolved, and there is no point in the mechanism of a cyclone to which more attention should be directed.

But the most remarkable point is that the blue centre does not always coincide with the barometric vortex. Sometimes the blue is in front, sometimes behind, sometimes to one side of the absolute *minimum;* and this has been considered as a proof of the inclination of the axis of the typhoon. This non-coincidence of the blue and of the barometric centre of the vortex has been noticed in every other hurricane country, but at present it is impossible safely to do more than record the fact for further investigation. The supposed non-coincidence of the centre of wind rotation with the barometric *minimum* has been noticed when discussing Indian cyclones.

All the characteristics of a typhoon are well exemplified in figs. 9

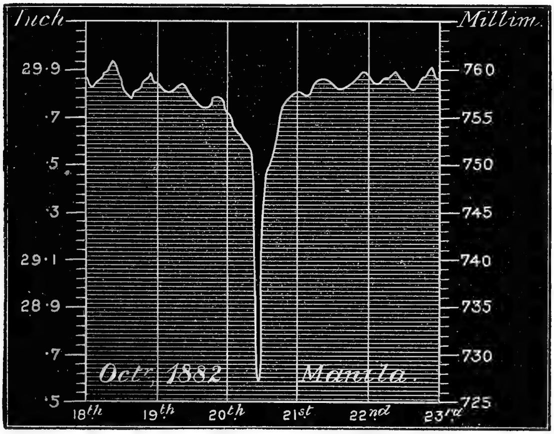
* Fracto-cumulus is a name applied at Manila to scud and irregular broken cumulus.

FIG. 9.



Typhoon in the Philippines, October 20th, 1882.

FIG. 10.



Barogram in Typhoon.

and 10, where reductions are given from two diagrams in the 'Observatorio Meteorológico del Ateneo Municipal de Manila. Observaciones verificadas durante el año de 1882.'

In fig. 9, the oval form, and centre displaced towards the rear, are almost identical in general character with the British cyclone given in fig. 1; only the sharpness of the central depression is much greater. The isobars are drawn at intervals of 0·4 inch, and as the longer diameter is only about 315 miles, and the shorter one 245 miles, the intensity of the depression can be readily realised.

But perhaps this will be even more strikingly exhibited if we look at the barogram during the typhoon at Manila, as given in fig. 10. There we find many of the typical features of a tropical cyclone, in the strong diurnal variation of pressure on the top of the general diminution of pressure which preceded the typhoon by several days, and in the very sudden depression near the centre of the hurricane.

The incurvature of the wind is very pronounced; and it is, perhaps, important to note that the strong supposed rising impulse in front of the centre, to which we have already alluded, is associated with a rearward compression of the isobars.

The vortex was almost exactly centred over Manila at the moment for which the chart is constructed, and though the rain ceased and the overcast sky grew higher, there was no blue visible.

The velocity of the whole typhoon appears to have been about 19 miles an hour.

Hong Kong.—From Manila the author went to Hong Kong, where Dr. Doberck, of the Observatory at Kow-long, has been giving much attention to the subject of typhoons.

Among other interesting points, Dr. Doberck has remarked that he has never found any indications of secondaries dependent on a primary cyclone; that the vertical succession of upper currents is the same as obtains in higher latitudes; and that the incurvature of the wind is less in front than in rear of a typhoon. The last is identical with what Meldrum has found in the South Indian Ocean, but the contrary of what usually occurs in Great Britain. Dr. Doberck also considers that the incurvature of the wind in a typhoon decreases as the depression recedes from the equator.

There are many observations to the effect that the central calm does not coincide with the *minimum* of the barometer; but nothing has yet been remarked with reference to trough phenomena.

Thunder and lightning do not occur in the heart of the typhoon; but 600 to 800 miles to the S. or S.W. of the centre thunderstorms may be experienced.

Japanese Typhoons.

From Hong Kong the author proceeded to Tokiyo, in Japan, to study the transitional district between tropical and extra-tropical cyclones; and through the courtesy of Mr. E. Knipping and Mr. Y. Wada, he succeeded in obtaining most valuable information.

The result of all his researches may be briefly summarised as follows:—

The shape of Japanese cyclones is usually oval; but well-defined typhoons are more nearly circular than the ordinary depressions of that country. The latter are, however, as a rule, far more pronounced ovals than in Europe. As typhoons move into higher latitudes, they certainly tend to become larger, more irregular in shape, and to move with greater rapidity. The centre is almost always more or less displaced, but the longer diameter tends in a marked degree to lie nearly parallel to the line of propagation.

Secondaries seem to be rare on the sides of the primaries; but the latter have the same reluctance to traverse land, and the same tendency to follow one another along the same path, which are found so frequently in European cyclones.

The velocity of translation is greater than in the Philippines; but the July typhoons usually move more slowly than those in August or September.

Mr. Knipping thinks from observations on the upper clouds that the height of some typhoons does not exceed three-quarters of a mile, or about 4000 feet; but the author considers this estimate far too low.

The wind is usually less incurved in front than in rear of the centre; and at some distance in front, with a S.E. wind, the centre may bear S. This is a very important point with reference to handling ships, but cannot be discussed here for want of sufficient information. It is, however, quite certain that though the barometer may have begun to fall, a ship may not really be within the sphere of the typhoon.

Much information could not be got on the movements of the upper clouds. Some of the observations at Nagasaki are very discrepant.

The author believes, however, that there is not the slightest doubt that the general circulation of a typhoon is exactly similar to that in an extra-tropical cyclone, for Mr. Harries ('Quarterly Journal of the Royal Meteorological Society,' vol. 12, p. 10) has traced a typhoon from the Philippines across the Pacific and the United States into Europe. This, like all other long-lived cyclones, received accessions of intensity from time to time by fusion with other cyclones which had formed outside the tropics; and it is inconceivable that two eddies, circulating on different systems, could coalesce without destroying one another. Cyclones are supposed to have the same general circulation, or to circulate on the same system, when the whole body of the storm circulates in the same manner—in-going counter-clockwise below, tangential to the isobars at low levels, outgoing at the highest altitudes. Two such cyclones, near one another, can and do easily coalesce; but if the upper currents in a typhoon were essentially different from those in extra-tropical cyclones, two adjacent cyclones could not coalesce without destroying each other. Cyclones south of the equator circulate on an opposite system to those in the northern hemisphere, and it is certain that a hurricane generated south of the line could not coalesce with one developed north of the equator.

All accounts agree that cirrus is seen all round a typhoon in Japan as at Manila, and that rain extends much further in front than in rear of the centre, as in higher latitudes. In some typhoons the development of squalls is far greater in front than in rear of the trough; and this is the opposite of what is found in England.

Lightning is sometimes seen in front of the centre of a typhoon, but apparently rarely in the true storm field.

Cirrus cloud is observed over the blue of the "bull's-eye," and Mr. Knipping informed the author that the clear central spot is not seen in quick-moving cyclones, while it is a very marked phenomenon of those whose progress is slow. This is a most important observation.

The "bull's-eye" and the centre of the wind's rotation do not appear to be always coincident with the barometric centre of the cyclone; but there are not enough land observations to enable the author to do more than note this point.

There are certainly traces of trough phenomena, though not strongly defined. Mr. Wada told the author that the clouds sometimes brighten a little about the passage of the trough, and then become dark again; but he had never noticed a line of squalls along the line of the trough.

Temperature is usually higher in front and lower in rear of

Fig. 11.

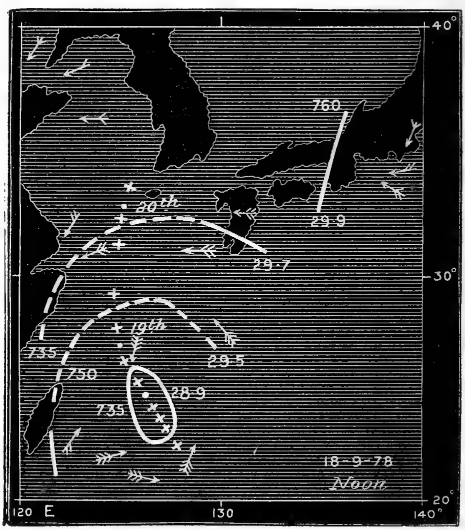


Fig. 12.

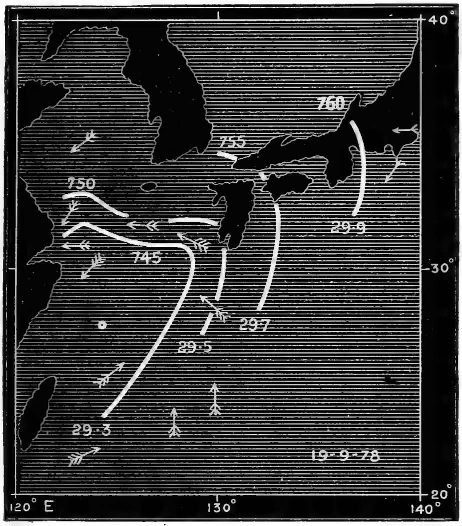
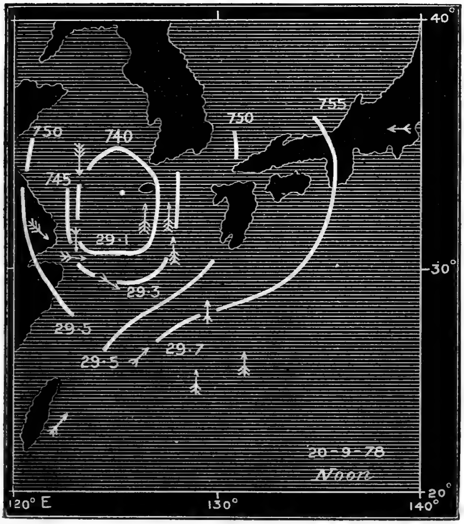


Fig. 13.



Figs. 11 to 13: Typhoon in China Seas.

Japanese typhoons, and here therefore they approximate more nearly to the European type of cyclones.

Most of the above characteristics of cyclones in the China Seas are well illustrated by the diagrams given in figs. 11, 12, 13 of a typhoon which raged from September 18th to 20th, 1878, which are taken from Mr. Knipping's paper "The September Taifuns, 1878," in the 'Mittheilungen der Deutschen Gesellschaft für Natur- und Völkerkunde Ostasiens,' Heft 18, p. 333.

In figs. 11 and 13 the usual oval form and pressing of the centre to one side are very obvious; and on the intervening day, fig. 12, we find the typhoon in a transitional irregular form, with all the indications of secondaries. The intensity of the whole is also much less than in our example from Manila.

The mean velocity of translation was 10 miles an hour, but varied at times from 2·3 to 25 miles per hour.

The incurvature of the wind is obviously much less than at Manila; and the squalls were far more pronounced in front than in rear of the typhoon.

The rain also extended much further in front than in rear; and the improvement in the weather after the passage of the trough was very rapid.

The amount of precipitation was so great that Mr. Knipping calculates that no less than 30,000 million tons of water fell on the 19th September on the portion of the earth's surface lying between 30° and 35° N. lat., and 120°—130° E. long.

Mauritius.

The author does not propose to detail here his researches on hurricanes in the Mauritius, as they bear more on the great value of Mr. Meldrum's rules for handling ships in cyclones than on the subject of this paper. All that need be said here is that allowing for difference of wind rotation due to the southern hemisphere, the phenomena of a Mauritius hurricane are exactly analogous to those in the Bay of Bengal or in the Philippines.

He finds the same oval shape, with displaced centre, and the same variations in the shape during the progress of any particular hurricane. The wind is also very slightly incurved in front, and very markedly so in rear of the cyclone. Cirrus extends all round the storm field; a blue bull's-eye is almost constant; and any "trough phenomena" are very slightly marked. But, just as at Manila, when the nimbus breaks in rear the clouds are harder there than in front; and M. Bridet at Réunion has noticed that the squalls are rather worse when the barometer turns to rise, *i.e.*, along the trough of the cyclone.

The propagation of these hurricanes is usually very slow.

Western Pacific.

The author has also visited New Caledonia and Fiji to gather information as to the character of hurricanes in that part of the world. He was not, however, able to collect sufficient materials to justify him in saying more than that those hurricanes seem to differ from those in the Mauritius in little except their smaller intensity.

The subject of wind in cyclones has already been fully investigated in many countries, but the points on which further research is urgently required are:—1. The nature of the central "bull's-eye"; 2. The phenomena of the trough; and 3. The nature of the high pressure areas which immediately surround a cyclone.

[*Note.*—The difference of temperature in front and rear of all tropical hurricanes is much less than in extra-tropical depressions. At the outskirts of a hurricane, about the time that cirrus first begins to form in the blue sky, the heat is sometimes very oppressive. The thermometer does not rise much; but as the ordinary breezes have failed, and been replaced by a suffocating calm, and the increasing humidity diminishes the evaporation of perspiration, the quality of the heat is peculiarly distressing.

As the sky gets overcast and the rain commences, temperature always falls, and continues relatively low till the sun shines again after the disturbance has passed away. This cold appears to be simply due to the obscuration of the sun's rays by cloud, and possibly partly also to a little cold air being brought down by the heavy rain.

All this is very different to the temperature disturbance of a British cyclone. The thermometer rises in England rapidly after the sky has become overcast, and remains high until the trough has passed, when a notable diminution of temperature suddenly takes place.

In a tornado, the rise of temperature in front, and diminution in rear of the disturbance, are very marked, and so far diminish the analogy between a tornado and a hurricane.

Since this paper was in type the author has had an opportunity of studying Padre B. Viñes' 'Apuntes relativos á los Huracanes de las Antillas.' That work confirms all the peculiarities of tropical cyclones found in other countries.

In Cuba hurricanes have the same oval shape and displaced centre as in other tropical countries; and the same rise of pressure with unusually fine weather occurs just before the advent of the depression.

The wind is little incurved in front, but very much so in rear; and a clear "bull's-eye," surrounded first by a ring of squally rain and then by a fringe of feathery cirrus, is the normal distribution of weather round the centre of the hurricane. No trough phenomena appear to have been observed by Padre Viñes.—*Added June*, 1887.]

Conclusions.

The conclusions as to the relation of tropical to extra-tropical cyclones which the author has derived from the researches of which this paper gives an account may be stated thus:—

All cyclones have a tendency to assume an oval form; the longer diameter may lie in any direction, but has a decided tendency to range itself nearly in a line with the direction of propagation.

The centre of the cyclone is almost invariably pressed towards one or other end of the longer diameter; but the displacement may vary during the course of the same depression.

Tropical hurricanes are usually of much smaller dimensions than extra-tropical cyclones; but the central depression is much steeper and more pronounced in the former than in the latter.

Tropical cyclones have less tendency to split into two, or to develop secondaries than those in higher latitudes.

A typhoon, which has come from the tropics, can combine with a cyclone that has been formed outside the tropics, and form a single new, and perhaps more intense, depression.

No cyclone is an isolated phenomenon; it is always related to the general distribution of pressure in the latitudes where it is generated. The concentric circles, which are usually drawn to represent a cyclone, ignore the fact that a cyclone is always connected with and controlled by some adjacent area of high pressure.

In all latitudes pressure often rises over a district just before the advent of a cyclone. The nature of this rise is at present obscure; but the character of the unusually fine weather under the high pressure is identical both within and without the tropics.

In all latitudes a cyclone which has been generated at sea appears to have a reluctance to traverse a land area, and usually breaks up when it crosses a coast line.

After the passage of a cyclone in any part of the world there is a remarkable tendency for another to follow very soon, almost along the same track.

The velocity of propagation of tropical cyclones is always small, and the average greatly less than that of European depressions.

There is much less difference in the temperature and humidity before and after a tropical cyclone than in higher latitudes. The quality of the heat in front is always distressing in every part of the world.

The wind rotates counter-clockwise round every cyclone in the northern hemisphere; and everywhere as an in-going spiral. The amount of incurvature for the same quadrant may vary during the course of the same cyclone; but in most tropical hurricanes the incurvature is least in front, and greatest in rear, whereas in England the greatest incurvature is usually found in the right front. Some observers think that, broadly speaking, the incurvature of the wind decreases as we recede from the equator.

The velocity of the wind always increases as we approach the central calm in a tropical cyclone; whereas in higher latitudes the strongest winds and steepest gradients are often some way from the centre. The portion of a cyclone which is of hurricane violence forms, as it were, a kernel in the centre of a ring of ordinarily bad weather. In this peculiarity tropical cyclones approximate more to the type of a whirlwind tornado; but the author does not think that a cyclone is only a highly developed whirlwind, as there are no transitional forms of rotating air.

The general circulation of a cyclone, as shown by the motion of the clouds, appears to be the same everywhere.

All over the world unusual coloration of the sky at sunrise and sunset is observed not only before the barometer has begun to fall at any place, but before the existence of any depression can be traced in the neighbourhood.

Cirrus appears all round the cloud area of a tropical cyclone, instead of only round the front semicircle as in higher latitudes. The allinements of the stripes of cirrus appear to lie more radially from the centre in the tropics, instead of tangentially to the isobars, as indicated by the researches of Ley and Hildebrandsson in England and Sweden respectively.

The general character of the cloud all round the centre is more uniform in than out of the tropics; but still the clouds in rear are always a little harder than those in front.

Everywhere the rain of a cyclone extends farther in front than in rear. Cyclone rain has a specific character, quite different from that of showers or thunderstorms; and this character is more pronounced in tropical than in extra-tropical cyclones.

Thunder or lightning are rarely observed in the heart of any cyclone, and their absence is a very bad sign of the weather. Thunderstorms are, however, abundantly developed on the outskirts of tropical hurricanes.

Squalls are one of the most characteristic features of a tropical cyclone, where they surround the centre on all sides; whereas in Great Britain squalls are almost exclusively formed along that portion of the line of the trough which is south of the centre, and in the right rear of the depression. As, however, we find that the front of a British cyclone tends to form squalls when the intensity is very great, the inference seems justifiable that this feature of tropical hurricanes is simply due to their exceptional intensity.

A patch of blue sky in the centre of a cyclone, commonly known as the "bull's-eye," is almost universal in the tropics, and apparently unknown in higher latitudes. This blue patch does not apparently always coincide exactly with the barometric centre. The author's researches show that in middle latitudes the formation of a bull's-eye does not take place when the motion of translation is rapid; but as this blue space is not observed in British cyclones when they are moving slowly, it would appear that a certain intensity of rotation is necessary to develop this phenomenon.

The trough phenomena—such as a squall, a sudden shift of wind and change of cloud character and temperature just as the barometer turns to rise, even far from the centre—which are such a prominent feature in British cyclones, have not been even noticed by many meteorologists in the tropics. The author, however, shows that there are slight indications of these phenomena everywhere; and he has collated their existence and intensity with the velocity of propagation of the whole mass of the cyclone.

Every cyclone has a double symmetry. One set of phenomena such as the oval shape, the general rotation of the wind, the cloud ring, rain area, and central blue space, are more or less related to a central point. Another set, such as temperature, humidity, the general character of the clouds, certain shifts of wind, and a particular line of squalls, are more or less related to the front and rear of the line of the trough of a cyclone.

The author's researches show that the first set are strongly marked in the tropics, where the circulating energy of the air is great and the velocity of propagation small; while the second set are most prominent in extra-tropical cyclones, where the rotational energy is moderate and the translational velocity great.

The first set of characteristics may conveniently be classed together as the rotational; the second set as the translational phenomena of a cyclone.

Tropical and extra-tropical cyclones are identical in general character, but differ in certain details due to latitude, surrounding pressure, and to the relative intensity of rotation or translation.

"Conduction of Heat in Liquids." By C. CHREE, B.A., King's College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received March 31,—Read April 21, 1887.

The conduction of heat in liquids has of late years been considered by several observers in Germany. In this country Mr. J. T. Bottomley and Professor Guthrie carried out experiments a good many years ago, but in neither case do the results agree well with those obtained abroad. In all the more recent methods the conduction has taken place through thin layers of the liquid, and thus in interpreting the results, the conditions at the surfaces limiting the liquid layer are of primary importance. It has been assumed by each observer that contiguous surfaces of any two media are in all circumstances at the same temperature. This, however, is contradicted by some high authorities, so it would seem important to have independent results based on experiments in which the liquid layer is of considerable thickness.

It should also be noted that in methods employing thin layers the temperature varies so rapidly in passing from one surface to the other that the liquid forms a by no means very homogeneous medium. This is the more important because experiments indicate that the conductivity of most if not all liquids increases rapidly as the temperature rises.

The following experiments were carried out in the Cavendish Laboratory at the suggestion of Professor J. J. Thomson, to whom I am much indebted for suggestions as to the form of the apparatus and the methods to be employed.

Two series of experiments were made with different apparatus. In the earlier series it was found that the apparatus was too large to be conveniently worked, and few results of a satisfactory nature were obtained. In the second series the apparatus was much reduced in size, though otherwise closely resembling that first employed. It will thus be sufficient to describe the second form and supply data as to the size of the first.

The liquid was contained in a wooden tub with vertical sides, 19·15 cm. in diameter, which was carefully fitted up by the mechanic at the Cavendish Laboratory. Not far below the rim and at equal distances apart were fixed three conical wooden pegs. The axes of the pegs formed parts of radii of a horizontal section of the tub, projecting inwards from the cylindrical surface to a distance somewhat exceeding 2 cm. The pegs supported a flat, thin-bottomed dish of tin-plate, 14·85 cm. in diameter, whose base was thus maintained horizontal. The liquid was poured into the tub till it reached the base of the dish. The liquid surface being strictly horizontal, it was easy to judge by the eye whether the dish was so also; if not the tub had to be adjusted till it was so. It was then advisable to stir the liquid to make sure that no air bubbles remained clinging to the dish. The bottom of the dish was about 5·2 cm. above that of the tub.

The method required the temperature to be measured at a known depth below the liquid surface. This end was secured by measuring the electrical resistance of a fine straight platinum wire 6·6 cm. long, which was supported at a depth of 2·61 cm. below the surface by two small trestles of glass. These were fixed in the bottom of the tub and projected upwards. The platinum wire was drawn tight over them, each end being fused to a much thicker piece of copper wire, so that the junctions were exactly at the same depth and immediately below the level of the platinum wire. The copper wires were tied with silk to the vertical parts of the trestles, and, passing straight down, were led through the bottom of the tub. The middle point of the platinum wire was vertically below the centre of the dish, and thus its ends were much nearer the axis of the tub than was the rim of the dish. The strictly horizontal position of the wire was tested by pouring in water first to the level of the wire and then to that of the dish. It was thus made certain that when the tub was placed so that the dish was horizontal, the platinum wire was so also. For most liquids the pegs, wires, &c., were secured by sulphur, but for bisulphide of carbon this was replaced by asbestos. During the experiments the tub was placed inside a double-walled wooden box, the space between the walls being stuffed with packing. The box was provided with a double lid similarly stuffed. To the ends of the copper wires were attached binding screws, the wires leading from which passed through grooves cut in the rim of the box. Thus the wires were in no way disturbed in moving the lid. This was a point of some importance, as even the small variations in the electrical resistance produced by slightly disturbing the binding screws was apt to affect the accuracy of the observations.

The platinum and its connecting wires formed one of the resistances of a Wheatstone's bridge arrangement. One of the others was a fixed resistance, and the remaining two were supplied by a wire bridge with a sliding-piece. The resistance of the platinum wire varies with the temperature, and—at least for small variations—its change is proportional to the change of temperature. The current was supplied by a single element—a small Daniell or Leclanché. A galvanometer, whose resistance could be reduced to 0·12 of an ohm, measured the variation from a balance between the resistances. The usual precautions in dealing with small resistances had to be taken; in particular it was found difficult to avoid producing thermoelectric currents if the sliding-piece were moved.

In the first apparatus the tub was 38·2 cm. in diameter, and the dish 30·2. The platinum was coiled in a spiral round a fine horizontal glass tube at a mean depth of 6·45 cm. below the liquid surface and about 7 cm. above the bottom of the tub. The length of the spiral was less than the radius of the tub.

The method of conducting the experiment was as follows:—The tub was put inside the box and filled with the liquid to the level of the dish. The box having been adjusted till the dish was horizontal, the lid was put on. The sliding-piece of the bridge wire was then moved till no current traversed the galvanometer. As the tub and liquid were in general at slightly different temperatures to begin with, some time elapsed before the galvanometer reading became constant.

When this had occurred the sliding-piece was again moved till there was no current through the galvanometer. It was then unnecessary to move the sliding-piece again, unless the deflection became greater than was usual in the experiment. When the galvanometer reading had remained some time constant, the lid of the box was removed and some hot water rapidly poured into the dish, care being taken that none splashed over into the tub. Sometimes the lid was immediately replaced and left on during the whole of the experiment; on other occasions the water was after a certain interval removed by a siphon, ready filled for the purpose. This always left a small quantity of water sufficient to cover the base of the dish without separating into drops.

The battery was connected with a key, and there was another in the galvanometer circuit. In the earlier experiments these were depressed in close succession at intervals of one minute, and the consequent kick or deflection of the needle observed. Subsequently it was found more convenient to use a constant battery, and to keep both keys down during the whole course of the experiment and for some time previously. Both methods were employed for most of the liquids examined, and no difference was detected in the results. The temperature of the platinum wire was seldom raised as much as 2° C. during the experiment, and consequently the disturbance of the balance in the Wheatstone's bridge was small. Thus the current through the galvanometer could be taken as directly proportional to the change in the resistance of the platinum, and so to the rise in its temperature.

Immediately subsequent to the application of the hot water there was a decided increase of the galvanometer reading which ceased very shortly. The reading then remained almost stationary for several minutes. It then began to increase rapidly and continued to rise for a considerable time, though the rate of change began comparatively soon to decrease. At first with the larger tub it was attempted to determine the interval that elapsed before the reading ceased to increase. This was, however, found impracticable, as it required several hours to reach this epoch; and after an hour and a half the rate of change was so slow that the least variation in the temperature of the laboratory was sufficient to upset the experiment. Even with the smaller apparatus this was not a quantity to be conveniently observed. It was found much easier to determine the much shorter interval that elapsed before the platinum wire was being most rapidly heated. This interval could also be expressed conveniently by means of the mathematical theory in terms of the conductivity and other properties of the liquid, and so its determination was sufficient for the purpose in view.

The galvanometer could be made so sensitive that with a single small cell a deflection exceeding 300 divisions of a millimetre scale could be obtained for a rise of one degree in the temperature of the platinum. When so sensitive as this, however, the galvanometer was too much exposed to the disturbing influences of adjacent currents or the movement of magnets in neighbouring rooms. From eighty to a hundred scale divisions to a degree usually gave the best results, and had the advantage of keeping the spot of light near the centre of the scale during the whole experiment, without any movement of the sliding-piece. The sensitiveness was most easily determined by finding how far the sliding-piece on the bridge had to be moved, when the battery was on, to produce a given change in the galvanometer reading. This test was usually applied at the beginning and end of each experiment, as a change in the sensitiveness during the observations might lead to erroneous conclusions. A slight displacement of the controlling magnet of the galvanometer may occur without affecting to any noticeable extent the position of the zero, and so without some such test as the above a change in the sensitiveness might escape detection.

Theory.

Let v denote the temperature, ρ the density, c the specific heat, and k the conductivity of a given liquid. Suppose the liquid to extend to infinity in every direction, and over the entire plane $x = 0$ a uniform supply of heat to be distributed at a rate given at the time t, counted from the first application of the heat, by the function $f(t)$ per unit area; then at the distance x from the plane of application the temperature at time t is given by

$$2\sqrt{\frac{\pi k}{\rho c}}\, v = \int_0^t \frac{e^{-x^2\rho c/4k(t-\chi)}}{\sqrt{(t-\chi)}} f(\chi)d\chi \quad . \quad . \quad . \quad . \quad (1)$$

In the present experiment the base of the dish answers to the plane $x = 0$, and $f(t)$ is to be regarded as proportional to the rate at which heat is conveyed from the dish to the liquid. It is true of course that the liquid exists only on one side of the plane $x = 0$, and does not extend to infinity in any direction. Doubtless the base of the tub tends to reflect the heat that has passed downwards through the liquid, but in the apparatus actually used any such reflected heat would be extremely small, and only the most trifling part of even this effect would show itself within the time of the experiment. Since the length of the platinum wire was much less than the diameter of the dish, which was in turn considerably less than that of the tub, the limitation in the horizontal direction would appear of small consequence. In fact when a larger quantity of hot water than was usually employed was poured into the dish, a delicate thermometer indicated a temperature in the liquid that was sensibly constant at a constant depth except close to the sides of the tub. The absence of liquid on the negative side of the plane $x = 0$ might appear a radical defect. It is clear, however, that in the supposed infinite liquid $\frac{1}{2}f(t)$ will pass into the liquid on each side of this plane, and the existence of the liquid on the one side merely ensures that $\frac{1}{2}f(t)$ is the precise amount passing into the liquid on the other side. But the law of diffusion on either side of the plane can depend only on the heat supplied to that plane, and must be independent of the precise mechanism by which the supply is regulated. For our present purpose it is sufficient to know that $f(t)$ is proportional to the rate at which heat passes into the liquid from the dish, which may be determined by a double observation as follows.

The tub being filled with liquid up to the level of the dish, a certain quantity of water heated to a definite temperature is suddenly poured into the dish. By means of a watch, and a delicate thermometer, raised initially to the temperature of the heated water and with it transferred to the dish, the law of cooling of the water is determined. The quantity of heat lost by the dish per unit of time at any required temperature can be easily deduced. If now the dish be placed on a non-conducting material, and the law of cooling be observed when the other circumstances are the same as before, the quantity of heat which leaves the dish per unit time in the first experiment without passing into the tub is at once obtained for the whole range of temperature. From these two experiments it is not difficult to calculate the amount of heat passing into the liquid in the tub at every instant in that form of the experiment in which the water poured into the dish was left there. When a siphon was employed the capacity for heat of the water left in the dish and the dish itself was so small that the heat subsequently transferred to the liquid was negligible.

To a clear understanding of the use of (1) some knowledge of the expression $t^{-\frac{1}{2}}e^{-x^2\rho c/4kt}$ is desirable. This is proportional to the temperature existing at a depth x in an infinite liquid, originally at zero temperature, at a time t subsequent to the application over the entire plane $x = 0$ of a unit of heat per unit of area. The first and second differential coefficients of the above expression are respectively—

$$t^{-5/2}\, e^{-x^2\rho c/4kt} \left(\frac{x^2\rho c}{4k} - \frac{t}{2}\right),$$

and

$$t^{-9/2}\, e^{-x^2\rho c/4kt} \left\{ \tfrac{3}{4}t^2 - 3\,\frac{x^2\rho c}{4k}t + \left(\frac{x^2\rho c}{4k}\right)^2 \right\}.$$

Thus the temperature at depth x, counted from the plane $x = 0$, gradually commences to rise and continues to do so for a time

$t = x^2\rho c/2k$, after which it steadily falls. The times at which the increase and decrease are fastest are respectively the smaller and the greater root of the quadratic equation

$$\tfrac{3}{4}t^2 - 3\frac{x^2\rho c}{4k}t + \left(\frac{x^2\rho c}{4k}\right)^2 = 0; \quad \ldots \ldots \quad (2)$$

and are approximately $$\cdot 0917\,\frac{x^2\rho c}{k}, \quad \ldots \ldots \quad (3)$$

and $$\cdot 908\,\frac{x^2\rho c}{4k}. \quad \ldots \ldots \quad (4)$$

Supposing it were possible suddenly to supply a quantity of heat to the surface of the liquid in the tub and to ensure that no commensurable quantity was subsequently gained or lost, an observation of the time at which the temperature at a given depth was rising fastest, or was stationary, would enable k to be determined at once.

In the actual case the problem is more complex as $f(\chi)$, though diminishing rapidly as χ increases, is different from zero; the principle however is practically unchanged. By differentiation we obtain from (1)

$$2\sqrt{\frac{\pi k}{\rho c}}\,\frac{d^2v}{dt^2} = \int_0^t f(\chi)\frac{d^2}{dt^2}\left\{\frac{e^{-x^2\rho c/4k(t-\chi)}}{\sqrt{(t-\chi)}}\right\}d\chi$$

$$+\text{terms at the limits.} \quad \ldots \ldots \quad (5)$$

Now when t is moderately large the terms at the limits may be neglected. This follows from a consideration either of the mathematical form or the physical meaning of those terms. They are proportional, one to the temperature instantaneously produced at the depth x at the time t by the heat at that instant passing into the liquid from the dish, and the other to the rate of change of this instantaneous effect. Now even when heat is being very rapidly communicated to the liquid, as at the commencement of the experiment, the rise in temperature due to conduction at a moderate depth is for a minute or two insignificant. Thus when the heat is being communicated very slowly, as is the case at the time at which we shall employ (5), the terms at the limits are for all practical purposes negligible.

When the temperature of the liquid at depth x is rising most rapidly, dv/dt is a maximum, and so $d^2v/dt^2 = 0$. From the above reasoning it follows that the time in question must satisfy the equation—

$$0 = \int_0^t f(\chi)(t-\chi)^{-9/2}e^{-x^2\rho c/4k(t-\chi)}\left\{\tfrac{3}{4}(t-\chi)^2 - 3\frac{x^2\rho c}{4k}(t-\chi)\right.$$

$$\left.+\left(\frac{x^2\rho c}{4k}\right)^2\right\}d\chi. \quad \ldots \ldots \quad (6)$$

This equation cannot be exactly solved, but an approximate solution of sufficient accuracy can be obtained. This gives t as a function of x, ρ, c, and k; but t is determined from the galvanometer readings, and x, ρ, and c can be otherwise determined, thus k is at once obtainable.

With the smaller apparatus, when the dish remained unemptied, the value of t, when water was in the tub, exceeded ten minutes, and for nearly all other liquids it is greater. The integral can be replaced by the summation—

$$\Sigma\,(t-\chi)^{-9/2}\,e^{-x^2\rho c/4k(t-\chi)}\left\{\tfrac{3}{4}(t-\chi)^2-3\frac{x^2\rho c}{4k}\,(t-\chi)+\left(\frac{x^2\rho c}{4k}\right)^2\right\}Q_\tau = 0, \quad \ldots \quad (7)$$

where Q_τ is proportional to the heat transmitted to the liquid during the interval τ, and $t-\chi$ is the time between the middle of this interval and the epoch of swiftest rise of temperature. It is not necessary to take τ the same throughout; thus at the beginning of the experiment when $f(t)$ varies rapidly, τ must be taken smaller than subsequently. The terms in the summation answering to the last few minutes of the experiment are always very small.

When the water was siphoned from the dish, any gain or loss of heat through the dish subsequent to the operation was very small compared to that given up to the liquid previously. Thus no serious error will be introduced by supposing $f(t) = 0$ after the siphoning.

It will be observed that what the galvanometer readings give is the time when the platinum wire is heating fastest, while the equation gives this epoch for the liquid at the same depth as the wire. Since the temperatures of the media are changing very slowly, it is scarcely conceivable that they could differ by a finite quantity, or that their rates of change should not be practically alike. The assumption made in the present method is of a totally different order from that made by previous observers dealing with thin layers of liquid. Their assumptions would be equalled only by supposing the dish and the liquid touching it to be always identical in temperature.

Theoretically the absolute quantity of heat initially given to the dish is of no importance, except in so far as it modifies the rate at which heat is subsequently communicated to the liquid. Experimentally it was found that both the quantity and the temperature of the water poured into the dish could be varied to a considerable extent without sensibly altering the epoch of quickest rise of temperature. When the water was siphoned the initial quantity of heat was of still less importance. With most liquids, however, the water was heated to a fixed temperature, viz., 75° C., and a measured quantity was poured into the dish. With bisulphide of carbon of course a much lower temperature was employed, but even then its heated top layer evaporated so fast as to affect the contact of the liquid with the dish. For these reasons in all experiments on the bisulphide the water was siphoned out of the dish.

In the earlier part of each experiment the heat was of course concentrated chiefly in the upper layers. Still as about an inch intervened between the dish and the platinum, the variation of temperature in by far the greater portion of the liquid layer was comparatively small. Thus the error due to treating the conductivity as independent of the temperature cannot be great.

There are two possible disturbing agencies which require comment. Any difference of temperature between the two copper-platinum junctions in the liquid would produce a thermoelectric current. Care was taken, however, that the junctions should be as nearly as possible in the same horizontal plane. At the depth in question the greatest possible difference between the temperatures at two points differing a few millimetres in depth could not exceed a small fraction of one degree. Thus the thermoelectric current, if existing, must have been very small, and necessarily its variation, from which alone any error could arise, must have been very trifling. Further, the neutral point of copper and platinum is only about 70°, so they would under the circumstances form a very weak couple.

An attempt was in fact made to employ a thermoelectric couple of iron and lead, whose neutral point is over 350°, one junction being in the liquid and the other maintained at a constant temperature. This, however, failed completely, owing to want of sensitiveness. Thus there are various *à priori* grounds for neglecting the thermoelectric effect in the actual experiment. This view was further justified by actual trial, first by finding the deflection that followed when one of the junctions was suddenly heated to a considerable temperature, second by cutting out the battery during the usual experiment, and observing whether shunting the platinum wire affected the galvanometer. Finally, in the various experiments on any one liquid the battery current traversed the platinum wire sometimes in one direction sometimes in the other; and thus any possible thermoelectric effect must have tended sometimes to increase and sometimes to diminish the rate of variation of the galvanometer reading. The small variation in the observed times of most rapid variation is thus sufficient proof of the small disturbing action of the thermoelectric effect, and the variation in the direction of the battery current would further tend to eliminate any such small effect if existent.

Another disturbing cause existed in the case of the sulphuric acid solutions. These conducted electrolytically, and also attacked the copper wires. By covering these wires with shellac varnish this was prevented. With the weaker solutions a slight coating was sufficient, but with the stronger it had to be frequently renewed.

In the cooling experiments, if we suppose the mass of water in the dish to remain constant, the rate at which the thermometer falls at any instant is directly proportional to the rate at which heat is leaving the dish. Of this heat some passes into the material below the dish, and some is lost by radiation from the water and the sides of the dish. The material on which the cooling was slowest was packing, and the heat given up to it seemed very small. Of this by far the greater part occurred in the first minute, and this is precisely the time when a small error in calculating the heat given to the liquid in the experiments on conductivity is of least importance. Thus in default of more accurate knowledge, the loss of heat by the dish when on packing was taken as representing the loss by radiation when on a liquid. Since the absolute amount of heat given to the liquid is not required, but only the ratio of the quantities given up for each minute or half minute of the experiment, absolutely no error would be introduced by neglecting the conductivity of the packing, provided the heat passing into the packing followed the same law as that passing into the liquid.

The liquids whose conductivities were determined are water, sulphuric acid solutions of various strengths, bisulphide of carbon, one solution of methylated spirit, paraffin, and turpentine oils. For water, methylated spirit, and paraffin two series of observations were made, the water being siphoned from the dish in one case, and left in the other. In the case of turpentine no observations were made with the water siphoned. For the sulphuric acid solutions and the bisulphide of carbon the siphon was always used. It was found that the conductivity and the rate of cooling of the dish were nearly independent of the strength of the sulphuric acid solution, and differed little from the corresponding quantities for water. The law of cooling on bisulphide of carbon also closely resembled that on water. The relative conductivities of these liquids would thus in all probability be most correctly obtained by referring them to the value obtained for water by the method employing the siphon. The following table gives the quantity of heat given up to the liquids in consecutive minutes or half minutes of the experiment, so far as is required in calculating the conductivity. The unit employed is arbitrary, but is the same throughout:—

Table I.

Time in minutes.	Water.	Methylated spirit.	Paraffin oil.	Turpentine oil.	Bisulphide of carbon.	Sulphuric acid solution, density 1·2.
0— ½	606	416	133	170	510	587
½— 1	386	327	136	137	270	422
1— 1½	241	157	110	93	80	250
1½— 2	185	119	79	67		
2— 3	255	170	100·7	82		
3— 4	181	100	72·1	43		
4— 5	138	79	50·2	32		
5— 6	112	67	34·4	22·6		
6— 7	92	53	28·4	16·6		
7— 8	74	38	17·0	6·3		
8— 9	64	26	13·8	·7		
9—10	58	21	11·5			
10—11	..	18	7·3			
11—12	..	16	6·7			
12—13	..	15	..			
13—14	..	..	..			

The times required for the dish to cool from 75° to 30° on water and on the sulphuric acid solution were almost identical. Excluding the first half minute, the cooling on the solution was slightly but decidedly faster for the first half of the period. In the bisulphide of carbon the experiment was made at a much lower temperature, which accounts for the comparatively small quantity of heat given to the liquid in the third half minute. In fact, considering the small temperature excess the rate at which the dish lost heat on the bisulphide was initially extremely rapid. On turpentine for the first few minutes the loss of heat was much faster than on packing; the rates then began to approach, and after the first nine minutes could not with certainty be said to differ. This coincidence lasted for the next five minutes or more, during which the observations were continued. Excluding the first minute, the cooling on turpentine was very decidedly slower than on any other liquid. This is due to the low conducting power and small specific heat of the liquid, in virtue of which the top layer soon became a sort of barrier to the penetration of the heat.

The following table gives the density ρ, and specific heat, c, of the liquids, and the time, t, in minutes, after the heating commenced before the temperature of the platinum wire was rising fastest:—

Table II.

Liquid.	ρ.	c.	ρc.	t. Water siphoned.	t. Water not siphoned.
Water	1·0	1·0	1·0	9·0	10·7
Sulphuric acid solution	1·054	0·935	0·985	8·75	
Do.	1·1	0·877	0·965	8·5	
Do.	1·14	0·843	0·961	8·5	
Do.	1·18	0·802	0·946	8·25	
Bisulphide of carbon......	1·276	0·247	0·315	6·65	
Methylated spirit	0·849	0·675	0·573	10·75	13·2
Paraffin oil	0·803	0·50	0·401	10·25	12·25
Turpentine oil............	0·875	0·43	0·376	..	14·6

In each case the density was taken with a hydrometer at or near the temperature of the experiment, and the specific heats are taken directly from 'Watts' Dictionary of Chemistry,' or obtained by interpolation from tables given there. In the case of the last three liquids the specific heat was also obtained experimentally, as these liquids vary somewhat in composition. The results so obtained were somewhat rough, but were sufficiently good to act as a check on the values taken. Small errors in the specific heat are not of much importance, as the probable error in the experiments on the conductivity amounts to at least several per cent. of the numbers taken. Further, the mean temperature to which the conductivity should be referred is also a somewhat doubtful matter.

The introduction of small impurities in the liquids did not appreciably alter t. For instance, small quantities of salt were put into the methylated spirit, and small quantities of the latter into water without producing any apparent effect. It would thus appear that the absolute purity of the liquids used is not of much consequence. Care was, however, taken to keep them as pure as possible, the bisulphide of carbon in particular being redistilled before use.

In every case the value t is the mean of a good many experiments, and, as a rule, the individual experiments agreed well together. Thus, when the siphon was not used the values obtained for t varied from 10·3 to 11 for water, from 12·75 to 14 for methylated spirit, and from 14·25 to 15 for turpentine. When the siphon was used the extreme differences in the numbers obtained for t were about as large as in the other method, and thus the agreement between the experiments was really not quite so good. This was only to be expected, as there was necessarily some slight variation in the time taken to siphon and in the result of the operation.

In obtaining a mean value for t the following method was adopted :

—A table was formed giving the increase in the galvanometer readings for each minute of each experiment. If there had been much variation in the sensitiveness of the galvanometer, the numbers obtained from each experiment were multiplied by a number varying inversely as the total increase in the readings during the fifteen minutes of that experiment subsequent to the application of the heat. The numbers for each minute were then added together, and the sum gave the mean rate of heating for the minute in question. From these rates the time of fastest heating could be easily calculated, or could be obtained graphically by constructing the curve whose abscissæ were the times elapsed since the heating, and whose ordinates were proportional to the rates of heating.

It will be best to consider first the experiments in which the water was siphoned from the dish, as the arithmetic required to obtain the conductivity from the equation (7) is then comparatively simple. It is assumed that the heat passed into the liquid for the first three half-minutes according to the law given in Table I, and that subsequently no heat at all was either given or lost through the dish. As it took some time to perform the siphoning, and there were no doubt slight variations in the small quantity of water left in the dish, the above is only approximately true, but the multiplication of the observations would tend to eliminate the errors.

If $x^2\rho c/4k$ be denoted by X, then from (7) and Table I, since $t = 9$, we have for water—

$$\begin{aligned} &606(8{\cdot}75)^{-9/2}\,e^{-\mathrm{X}/8\cdot75}\{\mathrm{X}^2-\tfrac{3}{4}\mathrm{X}(8{\cdot}75)+\tfrac{3}{4}(8{\cdot}75)^2\} \\ +&386(8{\cdot}25)^{-9/2}\,e^{-\mathrm{X}/8\cdot25}\{\mathrm{X}^2-\tfrac{3}{4}\mathrm{X}(8{\cdot}25)+\tfrac{3}{4}(8{\cdot}25)^2\} \\ +&241(7{\cdot}75)^{-9/2}\,e^{-\mathrm{X}/7\cdot75}\{\mathrm{X}^2-\tfrac{3}{4}\mathrm{X}(7{\cdot}75)+\tfrac{3}{4}(7{\cdot}75)^2\} = 0. \end{aligned}$$

From this equation X must be obtained by trial. If U stand for the left-hand side of the equation, it will be found that the corresponding values of X and U are as follows:—

X.	U.
22	$-88{\cdot}6\times10^{-3}$
22·7	$-\ 8{\cdot}5\times10^{-3}$
22·8	$+\ 1\ \times10^{-3}$

The value of U is best found by considering the logarithm of the several lines in succession. The following are the values of these lines:—

X.	First line of U.	Second line.	Third line.
22	$-101{\cdot}8\times10^{-3}$	$-12{\cdot}7\ \times10^{-3}$	$+25{\cdot}9\ \times10^{-3}$
22·7	$-\ 60{\cdot}5\times10^{-3}$	$+10{\cdot}21\times10^{-3}$	$+41{\cdot}77\times10^{-3}$
22·8	$-\ 55{\cdot}1\times10^{-3}$	$+12{\cdot}05\times10^{-3}$	$+44{\cdot}05\times10^{-3}$

From the values of U a very close approximation to X may be obtained by Maclaurin's theorem, which gives X = 22·79. This is a much closer degree of approximation than is at all necessary, considering the possible size of the experimental errors.

Since x = depth of wire = 2·61 cm.,

we get finally, the units being centimetre and minute,

$$k = \frac{6{\cdot}8121}{91{\cdot}16}$$
$$= 0{\cdot}0747.$$

This corresponds to a temperature of about 18° C.

The law of cooling of the dish on sulphuric acid solutions so nearly resembled that on water, that it will be sufficient to take the results given in Table I for a solution of density 1·2, and combine them with the corresponding results for water in a ratio proportionate to the strength of the intermediate solution. We thus obtain as proportional to the heat given to the liquid in the first three half-minutes the following values:—

Density of solution.	First half-minute.	Second half-minute.	Third half-minute.
1·054	601	396	243
1·11	596	404	245
1·14	593	411	247
1·18	589	418	249

Employing U and X in the same sense as for water, it will be found, precisely as in the previous case, that the following results are true for the various solutions. The last column gives the temperature to which the conductivity belongs.

Table III.

Density of solution.	X.	U.	Mean value of U.	k.	Temperature, centigrade.
1·054	22·0	$-10^{-3} \times 12{\cdot}4$	22·095	0·0759	20½°
..	22·1	$+10^{-3} \times 0{\cdot}64$			
..	22·2	$+10^{-3} \times 13{\cdot}58$			
1·10	21·4	$-10^{-3} \times 2$	21·413	0·0767	20¼
..	21·6	$+10^{-3} \times 28{\cdot}4$			
1·14	21·4	$-10^{-3} \times 1$	21·407	0·0765	19¾
..	21·6	$+10^{-3} \times 29{\cdot}6$			
1·18	20·6	$-10^{-3} \times 17$	20·703	0·0778	21
..	20·8	$+10^{-3} \times 16$			

Neither the method nor the theory is so extremely accurate that any value can be attached to the third significant figure in the value for k in assigning absolute values for the conductivity. In assigning relative values, the third figure would have some weight in liquids in which the heat was applied so similarly as in the case of water and the above solutions. Since, however, the temperatures of the experiments were not identical, and the conductivity unquestionably increases with the temperature, it would probably be unsafe to deduce from the above numbers any more precise conclusion than that the presence of a very considerable quantity of sulphuric acid produces an extremely small change in the conductivity for heat of water.

It might also be considered almost certain that the time at which the temperature was rising fastest diminished as the density of the solution increased. This signifies that the velocity with which heat-waves travel, or the *temperature conductivity* of Weber, is greater the stronger the solution. The liquid in the tub in these experiments was in general stirred up fifteen minutes after the heat had been applied. It is pretty obvious that the ratio of the galvanometer reading after the stirring to that before should diminish as the temperature conductivity increases. The ratios so obtained for water and the above solutions were in order, 1·36, 1·34, 1·32, 1·28 and 1·23.

No very great accuracy can be claimed for these numbers as the determination was somewhat rough, but as independent evidence of the truth of the above statement as to the temperature conductivity they are of considerable weight.

For bisulphide of carbon, methylated spirit, and paraffin oil, the values from Tables I and II substituted in equation (7) lead to the following results:—

Table IV.

Liquid.	X.	U.	Mean value of X.	k.	Mean temperature.
Bisulphide of carbon.	16·5 16·6 16·7	$-10^{-4}\times 36·16$ $-10^{-4}\times 15$ $+10^{-4}\times 9·26$	16·66	0·0322	$15\frac{1}{2}°$
Methylated spirit....	27·5 27·6	$-10^{-4}\times 26·5$ $-10^{-4}\times 13·8$	27·566	0·0354	$19\frac{1}{2}$
Paraffin oil	25·8 26·0	$-10^{-4}\times 18·66$ $+10^{-4}\times 26·66$	25·88	0·0264	19

There still remain to be considered the experiments in which the water was not siphoned from the dish. As an example of the application of (7), it may be as well to give its form for one of the liquids. The following equation is for the methylated spirit, X being $= x^2\rho c/4k$ as previously.

$$
\begin{aligned}
&416(12{\cdot}95)^{-9/2}\, e^{-X/12\cdot95}\{X^2-3X(12{\cdot}95)+\tfrac{3}{4}(12{\cdot}95)^2\}\\
&+327(12{\cdot}45)^{-9/2}\, e^{-X/12\cdot45}\{X^2-3X(12{\cdot}45)+\tfrac{3}{4}(12{\cdot}45)^2\}\\
&+157(11{\cdot}95)^{-9/2}\, e^{-X/11\cdot95}\{X^2-3X(11{\cdot}95)+\tfrac{3}{4}(11{\cdot}95)^2\}\\
&+119(11{\cdot}45)^{-9/2}\, e^{-X/11\cdot45}\{X^2-3X(11{\cdot}45)+\tfrac{3}{4}(11{\cdot}45)^2\}\\
&+170(10{\cdot}7)^{-9/2}\, e^{-X/10\cdot7}\{X^2-3X(10{\cdot}7)+\tfrac{3}{4}(10{\cdot}7)^2\}\\
&+100(9{\cdot}7)^{-9/2}\, e^{-X/9\cdot7}\{X^2-3X(9{\cdot}7)+\tfrac{3}{4}(9{\cdot}7)^2\}\\
&+\ \ 79(8{\cdot}7)^{-9/2}\, e^{-X/8\cdot7}\{X^2-3X(8{\cdot}7)+\tfrac{3}{4}(8{\cdot}7)^2\}\\
&+\ \ 67(7{\cdot}7)^{-9/2}\, e^{-X/7\cdot7}\{X^2-3X(7{\cdot}7)+\tfrac{3}{4}(7{\cdot}7)^2\}\\
&+\ \ 53(6{\cdot}7)^{-9/2}\, e^{-X/6\cdot7}\{X^2-3X(6{\cdot}7)+\tfrac{3}{4}(6{\cdot}7)^2\}\\
&+\ \ 38(5{\cdot}7)^{-9/2}\, e^{-X/5\cdot7}\{X^2-3X(5{\cdot}7)+\tfrac{3}{4}(5{\cdot}7)^2\}\\
&+\ \ 26(4{\cdot}7)^{-9/2}\, e^{-X/4\cdot7}\{X^2-3X(4{\cdot}7)+\tfrac{3}{4}(4{\cdot}7)^2\}\\
&+\ \ 21(3{\cdot}7)^{-9/2}\, e^{-X/3\cdot7}\{X^2-3X(3{\cdot}7)+\tfrac{3}{4}(3{\cdot}7)^2\}\\
&+\ \ 18(2{\cdot}7)^{-9/2}\, e^{-X/2\cdot7}\{X^2-3X(2{\cdot}7)+\tfrac{3}{4}(2{\cdot}7)^2\}\\
&+\ \ 16(1{\cdot}7)^{-9/2}\, e^{-X/1\cdot7}\{X^2-3X(1{\cdot}7)+\tfrac{3}{4}(1{\cdot}7)^2\}\\
&+\ \ 15({\cdot}7)^{-9/2}\, e^{-X/\cdot7}\{X^2-3X({\cdot}7)+\tfrac{3}{4}({\cdot}7)^2\} = 0.
\end{aligned}
$$

The solution must of course be obtained by trial, but it is comparatively easy to form a pretty accurate idea of its value from considering the value of the coefficients in square brackets. Further, when a solution has been obtained for one equation, its magnitude enables an idea of the magnitude of the solutions of the other similar equations to be readily obtained. The necessary arithmetic is best performed by finding the value of each line of the left-hand side separately by means of logarithms. The first four or five lines will in each case be negative, and the rest positive. The last two lines at least will be found extremely small. The following table, in which the letters have their previous significations, gives the results obtained:—

Table V.

Liquid.	X.	U.	Mean value of X.	k.	Temperature.
Water	21 22	$+10^{-3}\times 13\cdot4$ $+10^{-3}\times 149\cdot1$	20·9	0·0815	$19\frac{1}{2}°$
Methylated spirit....	27 28	$-10^{-3}\times 57\cdot45$ $-10^{-3}\times 8\cdot3$	28·17	0·0346	18
Paraffin oil	25 25·1 26	$-10^{-3}\times 1\cdot04$ $+10^{-3}\times 2\cdot52$ $+10^{-3}\times 30\cdot28$	25·029	0·0273	20
Turpentine oil	33 34	$-10^{-3}\times 12\cdot36$ $+10^{-3}\times 1\cdot97$	33·86	0·0189	18

As an example of the arithmetical results and the value of successive lines of U, it will be as well to take as an example the results in the case of methylated spirit, which are embodied in the following table :—

Table VI.

Number of line.	X = 27.	X = 28.
1	$-10^{-6}\times 99223$	$-10^{-6}\times 84211$
2	71974	59235
3	30661	24104
4	19382	14098
5	16479	8377
6	$+10^{-6}\times 3110$	$+10^{-6}\times 8043$
7	17016	20528
8	30864	32878
9	39736	39657
10	38564	36556
11	28695	25796
12	17346	14560
13	4827	3636
14	109	67
15	$10^{-12}\times 1$	$10^{-13}\times 2$

Taking into consideration the nature of the investigation, the agreement between the results obtained by the two methods seems on the whole satisfactory. In the case of methylated spirit and paraffin oil, the agreement could hardly be closer, and the fact that it is so good must indeed in considerable measure be a pure matter of chance. In the case of water, there is a decided though not very serious discrepancy. The difference in the mean temperature of the two experiments could account for only a small part of this. The experiments on water were the earliest in which the siphon was used, and the operation took slightly longer and its results were not quite so uniform as in later experiments. Further, when no siphoning took place, the heat passing into the liquid at the end of the experiment was much larger in the case of water than for the other liquids, and the terms at the limits in (5) would thus be of slightly greater importance for water than for the others. Also an error of given amount in the experimental determination of the time of most rapid heating would produce the greater effect the shorter the time, and would thus modify the results for water more than for any other of the liquids, except bisulphide of carbon. Thus it was only to be expected that the greatest discrepancy between the results of the two methods should occur in water.

With the larger apparatus results were obtained for water and methylated spirit, of the same constitution as in the experiments already described, which, though not pretending to great accuracy, may be of interest as independent evidence of the correctness in the main of the theory. For the intervals in minutes that elapsed after the application of the heat before the temperature of the wire was rising fastest, the mean of several experiments gave $52\frac{1}{4}$ for water, $67\frac{1}{2}$ for the spirit. The water was left undisturbed in the dish, and no accurate observations of the rate of cooling were made. It was noticed, however, that the dish parted rapidly with its heat, and being only slightly deeper than the dish in the small apparatus, it is pretty clear that by far the greater part of the heat was given to the liquid in the tub in the first few minutes. Thus the experiment would be pretty much akin to the case when the water was siphoned in the smaller apparatus. Though ignorant of the law of cooling, we can thus obtain an inferior limit to the conductivity, of a moderately close kind, by supposing the heat to have been instantaneously communicated. This gives from expression (3), viz., $k = 0{\cdot}0917x^2\rho c/t$, for water $k = 0{\cdot}0730$, and for methylated spirit $k = 0{\cdot}0324$, corresponding to temperatures of about 18° C. These results as being essentially inferior limits, agree fairly with those of the smaller apparatus.

On the whole, the results of this series of experiments resemble those obtained by Herr Weber.* The values obtained for the conductivity of water agree fairly well with his. The smaller value obtained by Weber for bisulphide of carbon, viz., 0·0250, would be partly accounted for by the very considerably lower temperature of his experiment. As this liquid boils at a very low temperature, the

* 'Wiedemann, Annalen,' vol. 10, pp. 103, 304, 472; see specially table on p. 314.

rate of variation with the temperature of its thermal conductivity is very probably much above the average.

To reduce the results of the present paper to the C.G.S. system of units, it is only necessary to divide them by 60.

"On Rabies." By G. F. Dowdeswell, M.A., F.L.S., F.C.S. Communicated by Professor Victor Horsley, F.R.S.

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[Plate 1.]

CONTENTS.

Numerous as are the communications upon the subject of rabies, the paucity of experimental investigation is remarkable; the disease has remained for upwards of 2000 years, since the first recorded mention of it by Aristotle, exceedingly obscure in many essential points. The unparalleled and variable length of its incubation period has offered the greatest obstacle to systematic examination; in the words of John Hunter in the last century, "It has defied alike scientific investigation as to its intimate nature, and all remedial measures for its successful treatment."

Lately, however, the results announced to have been attained by M. Pasteur, have promised to remove these obstacles, and encouraged research by new methods and with fresh views.

This investigation was commenced early in 1885, during the prevalence of rabies in and around London. Two well-marked cases in dogs were obtained, and inoculations with their saliva, taken both during life and shortly after death, were made into the subcutaneous tissue of other animals, but failed to produce infection.

At that time I was not sufficiently conversant with the results of M. Pasteur's investigations to place reliance upon his methods of intracranial inoculation with the cerebro-spinal substance of a rabid animal, and I must admit that his statements seemed to me to be improbable and inconsistent with the facts which were previously well established in this disease.

The outbreak of the epizooty shortly afterwards subsiding, I was unable to resume experiments till the summer of 1886, when it had become necessary to examine the results said to have been attained by M. Pasteur. His statements, now widely known, communicated to the Academy of Sciences, Paris, from time to time, and published in their 'Comptes Rendus,' are essentially these: (1) That the virus of rabies and hydrophobia resides in the cerebro-spinal tissues, and is not confined to the salivary glands as hitherto supposed. (2) That by inoculation of this substance upon the brain of another animal by trephining, or by intravenous injection, infection follows infallibly and much more quickly than by subcutaneous inoculation. (3) That the virus from a rabid dog by passing through a series of animals of a different species is modified in virulence; in monkeys it is attenuated and ultimately lost, in rabbits on the contrary, it is intensified, and after a certain number of inoculations in these animals reaches a maximum, which it maintains unaltered; these modifications of activity being shown by the duration of the incubation period following inoculation. (4) That by successive inoculations with virus, the activity of which is progressively diminished either by passing through a series of monkeys, or by the action of dry air upon the spinal cords which contain it, it is possible to confer upon dogs and other animals, together with man, immunity against subsequent infection with the most active lyssic virus.

In reference to these statements, the first points for investigation were now, the effects produced by inoculation with the cerebro-spinal substance of a rabid animal upon the brain of another, and whether the symptoms stated to be produced thereby were those of infective rabies—lyssa—or, as some contended, merely a neurosis resulting from the injection of foreign matter.

In the methods adopted in these experiments, I have followed those described by M. Pasteur in his published statements, but for imparting to me many details of manipulation, I am greatly indebted to Professor Horsley, F.R.S., who learned them from M. Pasteur himself in Paris.

II. *Methods of Preparation and Inoculation with Virus.*

The animal from which it is desired to inoculate having died or been killed, a part of the spinal cord is exposed, and the portion desired removed, with precautions against contamination, the requisite instruments, vessels, and other apparatus having been previously sterilised by the recognised methods; the medulla is then carefully ground up to a homogeneous pulp in a glass mortar, and triturated with the proper proportion of sterilised beef-bouillon, as prescribed by

M. Pasteur. Salt solution or any other indifferent fluid would no doubt answer as well, but bouillon has the great advantage of showing at once the occurrence of any septic change in the fluid, by the turbidity which it occasions.

In order that the conditions of experiment might be strictly similar, I have myself always used definite proportions of cord and bouillon; as 1 inch of the former, of an average-sized rabbit, weighs about 0·8 gramme, I have mixed or "diluted" this quantity with four times its weight or bulk—their specific gravity being very nearly the same—viz., 3·2 c.c. of bouillon.

In order to free the infusion or "mash" thus prepared, from any portion of the membranes investing it, or grosser particles of its substance unreduced, it is strained through fine muslin, sterilised by passing over the flame of a spirit-lamp.

In the earlier experiments with rabbits, the animal to be inoculated was anæsthetised by æther; it was soon found, however, that this was unnecessary, inducing a great mortality, and being productive of pain to the animal, whilst coming under and recovering from the influence of the drug. I then used a solution of cocaïne as a local anæsthetic, with apparently satisfactory results, but ultimately found that nothing whatever is required beyond the 5 per cent. solution of carbolic acid, with which, after clipping the hair closely, the head is washed, as an antiseptic; if this is rubbed in for a short time, complete anæsthesia is produced locally, the animal in the large majority of cases remaining perfectly quiet, frequently with its eyes closed, during the slight operation of trephining and inoculation, and not requiring confinement or restraint in any way, save by a hand lightly laid upon it.

The bone is then trephined in the usual manner, a small incision being made in the skin and periosteum, a little behind the coronal suture, and on one side of the median line. The virus is injected with a Pravaz syringe either between the bone of the skull and the dura mater, or by perforating the latter with the curved point of the needle the requisite quantity is injected into the sub-dural lymph space.

The effect of either method is much the same, but by the former the incubation period is slightly but appreciably longer than by the latter, the difference being, with intensified virus, one or two days.

III. *Symptoms and Post-mortem Appearances of Rabies.*

1. *In the Dog.*—These in the dog have been described by numerous writers from the time of Cælius Aurelianus,* nevertheless considerable misapprehension still generally prevails upon some points. The

* 'De Morbis Acutis' (Amsterdam, 1722). His account is short, but accurate in most points, and is the earliest extant. The period at which he lived is uncertain.

symptoms will vary in some respects in an animal kept quietly in confinement from those found in the mad dog of the streets that has lost its home and been hunted about.

In most cases the first change observed is a dulness and sullenness, with an indisposition to move, the animal lying crouched up in a corner; probably this is invariably the first symptom, though, especially in dogs at large, it may be overlooked, and the symptoms, or as sometimes termed, stage, next following may be the first to attract notice. In this, a shy and suspicious or threatening look is a most characteristic feature; the previous dulness is succeeded by irritability and constant restlessness, with, usually, a disposition to fly at any strange object and bite. A depraved appetite is frequently noticeable in the early stages, natural food being most usually rejected, hay and straw, bits of cloth, wood or cinder frequently being eaten. This is one of the most constant and best recognised symptoms, though not absolutely invariable.

Hydrophobia, or dread of water, is never present in the dog; there is sometimes increased thirst, but in dogs in confinement this is not generally marked: there is often inability to swallow from paralysis of the muscles of deglutition and those of the lower jaw, which in an early stage is usually observed drooping, with inability to close it, though the extent and duration of this is variable, and it passes off at a later period.

Excessive salivation is not usual; when observed it is in the hot weather, and occurs from loss of the power of deglutition. One of the most characteristic and best recognised symptoms, occurring generally in an early stage, is a remarkable change in the voice, the bark becomes a hollow howl, commencing with a short low note and ending in a higher one prolonged; it has always a peculiar metallic ring, which once heard cannot be mistaken.

The further symptoms developed depend chiefly upon the temperament of the animal, modified somewhat as above remarked, by its external conditions; an aggresive disposition is usually found, but is not invariably present, though in an irascible savage animal it may realise the popular idea of furious rabies, attacking and tearing everything; in confinement, however, this extreme is not usual, and some dogs are with difficulty induced to bite anything presented to them, even a rabbit or another dog, and the fury said to be excited by the sight of the latter is not generally found in confinement.

The last stage is that of paralysis, which, more or less developed, is invariable; it commences in the hind limbs, its first indication often is the animal standing with its hind legs wide apart; when it moves it is unsteady, swaying from side to side, as this progresses it becomes unable to stand, is ultimately completely paralysed, and comatose in the large majority of cases. The tail in confinement is never carried depressed between the legs, as is described by some of the earlier writers as a character of this disease, if it occurs in a street dog it is the result of exhaustion.

It has been usual to describe rabies in the dog as of two forms, the furious and the dumb, or paralytic. Fleming, however, and others of the best authorities recognise that there is no real distinction between the two, every case of rabies probably, if permitted to run its course and terminate naturally in death, develops symptoms, more or less marked, of paresis; there is no constant distinction between the two forms, the difference consists merely in one or the other class of symptoms, rage or paresis, being the more preponderant, according to the part of the cerebro-spinal system which is principally affected.

Whatever the disposition of the animal may be, it almost invariably recognises its master or attendant, and is in some degree amenable to his control until completely paralysed and unconscious. There is a danger in this feature that not being well known it may occasion the presence of a virulent disease to be overlooked or mistaken, as indeed frequently does happen.

The post-mortem appearances of rabies in the dog have frequently been described as mainly negative, characterised by the absence of any distinct lesions; this, however, is only very exceptionally correct. In animals which die naturally at the termination of the disease, the appearances are in the majority of cases sufficiently diagnostic; in those killed at an earlier stage, as necessarily occurs in the large majority of cases of "street rabies," the condition of the stomach as to its contents may be the only diagnostic character.

The general condition is frequently wasted, to an extent dependent upon the duration of the symptoms and the inability to feed.

The brain and spinal cord being now recognised as the essential seat of the virus, it is to the appearances they present that attention is first directed. In most cases the dura mater of both is distinctly congested, occasionally intensely so; I have seen one case at least of a street dog killed in an advanced stage of the disease, where this membrane in a portion of the spinal cord received, was most intensely congested and livid in colour. This, however, is exceptional. The pia mater of the hemispheres is likewise most frequently injected, and in the greater number of cases, capillary congestion is apparent in the cortex in microscopical sections, with extravasation of lymph cells through the walls of the vessels and perivascular lymph spaces into the surrounding tissues. In the cerebellum this occurs to a more limited extent. It is by no means confined to the floor of the 4th ventricle as has been sometimes stated. Throughout the medulla oblongata it is constant, and often occurs in the spinal cord. In the latter, extravasation of red corpuscles or minute hæmorrhages are frequent; in some cases these are of large size and quite apparent to the unaided eye. In one case of street rabies, in a part of the cervical portion of the cord I found this so extensive as to obliterate nearly the whole of the gray substance for some length of the cord, the hæmorrhage becoming distinctly organised, with the formation of vessels or channels; I have found similar appearances in other parts of the cord in different cases, but none so extensive as this. These seem to originate in the vessels running in the anterior fissure, in which clots forming thrombi are often apparent, presenting the appearances figured by Gowers ('Pathol. Soc. Trans.,' vol. 28, 1876–77, p. 10, &c.), though frequently of greater extent proportionately to the size of the vessel. Extensive extravasation and, in stained preparations, much granular matter is always apparent, the formation presenting every appearance of being caused by microparasites: in the majority of cases I have been unable to demonstrate their presence, from causes mentioned below, but in some few sections, as described, I have clearly found the microbes in the pericellular and perivascular lymph spaces, accompanied by appearances of embolism, and extravasation in the capillaries of their immediate neighbourhood. The occurrence of these hæmorrhages in different situations, involving the roots of different sets of spinal nerves, will obviously affect the symptoms of paralysis according to the muscles supplied by the nerves involved.

In the alimentary canal and respiratory organs conspicuous changes are constantly present. The tongue is generally dry and discoloured, often brown; the epiglottis is frequently conspicuously injected, the lower part of the larynx so deeply congested as to appear crimson. This often involves the greater portion of the trachea and extends to the bronchi. the lungs are generally congested, though to a variable extent, most usually they are bright red, with portions deeply injected, very frequently on the margins of the lower lobes; parts of them sometimes are consolidated and livid. Though some of these changes may be due to causes independent of rabies, congestion however is most usually present: œdema I have not found.

The pharynx and œsophagus less frequently show congestion than the trachea, but the stomach, as generally remarked, shows very constant and typical changes; in every case, excepting one, in the dog, I have found it devoid of solid food; in that case, as in every other excepting one, it has contained some hay; in the greater number of the cases of street rabies there have also been found other foreign substances—cinders, coal, wood, cloth, &c. In cases, however, of experimental inoculation, a dog confined in a cage throughout the course of the disease can have but little opportunity of eating any indigestible substances, excepting hay and straw.

The stomach frequently contains a thick, dark-brown fluid, which is also found in the duodenum; the mucous membrane if not discoloured by the fluid present, is usually redder than normally; congestion of the veins is generally very apparent on the exterior or serous surface, being most marked towards the cardia, and on their ramifications are apparent hæmorrhages or ecchymoses of very variable size and number, from the most minute up to usually 2, 4, or 6 mm. in width or more. When very minute they are more easily distinguished on the interior or mucous coat where they appear as black specks or spots prominent upon the surface, generally on the summits of the rugæ: they are in fact small clots. They have been described by previous writers,* and are figured by Fleming in a coloured drawing of a stomach with a portion of the mucous membrane exposed; I have, however, usually found them more distinct and clearly defined than those there shown, not having observed the mucous membrane as highly coloured as in his drawing, the contrast consequently being greater. They are, too, correctly described by Youatt ('The Dog,' p. 143) as "effusions of bloody matter, or spots of ecchymosis on the summits of the rugæ," and regarded by him as very pathognomonic.

I have found these present in nearly all the dogs I have examined in which the disease has run its course; in those that are killed during its progress they are necessarily less developed.

Their appearance is very diagnostic; they occur in some few other cases,† but then only, I believe, of small size; when large or well defined, as above described, in conjunction with the presence of foreign bodies, cinders, wood, cloth, &c., in the stomach, no doubt of the nature of the case can exist.

The presence of hay alone, though suspicious, is not of itself conclusive, for in the "Brown Institution" it has lately been found in dogs not rabid and apparently healthy, in some cases entirely filling the stomach; this may be accounted for by the inability of these dogs in confinement to get grass, which when at large they constantly eat. I may add that the pylorus is invariably hyperæmic, sometimes intensely so; this is best observed in the serous coats.

The appearance of the liver is variable, usually it is very dark and congested; the spleen I have found normal in all cases except one, when it was somewhat enlarged, but unchanged in other respects.

The salivary glands have hitherto been regarded as the seat of the virus, and received much attention, but they do not present any constant pathognomonic appearances; in one case I found the submaxillary gland somewhat hypertrophied and vascular, with the

* They have been described by some as "hæmorrhagic erosions;" the term is not appropriate, though "erosions" may apply to the appearances of post-mortem digestion, which are sometimes observed, but not constantly.

† Viz., in swine fever (Dr. Klein), in some cases of experimental tuberculosis, and in anthrax in rabbits caused by "capillary embolism by masses of bacteria," as recorded by M. Feltz ('Comptes Rendus,' vol. 95, 1882, p. 859).

parotid normal; but this is not constantly the case, and as often as not these glands are normal both to the eye and in microscopical sections.

The kidneys are frequently but not invariably congested; the urinary bladder is generally so, and in the dog is frequently empty or contains a small quantity of urine.

The blood is always very dark coloured; in about half the cases it is fluid without any, or with very little clot; its reaction very shortly after death and within the vessels is neutral. No changes are apparent in the tissues of the heart; it is generally moderately distended with blood, whether fluid or clotted. In the morphological elements of the blood no alteration can be detected by the microscope, excepting in some cases an increase in the number of leucocytes. In the microscopical appearances of the other organs or tissues the changes which may occur, as has been described by some writers (*e.g.*, the granular appearance of the liver cells, by Bollinger), are to my observation by no means constant, nor can they be regarded as pathognomonic.

2. *In the Rabbit.*—The occurrence of rabies in this animal has till recently been a matter of some doubt. The first authentically recorded case of the successful transmission of rabies to the rabbit is where Mr. Simonds (22nd April, 1838) at the Royal Veterinary College* inoculated two rabbits subcutaneously behind the ears with the saliva of a rabid sheep. After an incubation period of four days, they showed symptoms of infection, being found dull, hanging their heads and inclining them to one side; one shortly afterwards showed excitement; they then became comatose and died. The occurrence of paraplegia is not recorded. The incubation period is unusually short, but there appears no doubt that it was true rabies that was developed.

In the rabbit, as in the dog, infection is very uncertainly produced by inoculation even with active cerebro-spinal substance into the subcutaneous tissues, and, when it does occur in the former animal, the symptoms are materially different from those previously regarded as typical in the dog, and nothing can be less appropriate than the application of the term rabies or lyssa to them.

* Reported in the 'Proceedings of the Veterinary Medical Association' for 1838–39, p. 369, and in 'The Veterinary Record" for 1845; also in the 'Veterinarian' for March, 1881, p. 189; and referred to by Fleming in the appendix to his work, before mentioned. Youatt also (*op. cit.*, p. 149) refers to cases of asserted rabies in the rabbit, mentioned in evidence before a Royal Commission on that subject, but considers them doubtful.

In 1879 M. Galtier ('Paris, Acad. Méd. Bull.,' vol. 8, p. 1114) inoculated a rabbit with the saliva of a case of hydrophobia in man, producing rabies with great excitement. From its submaxillary gland two other rabbits were inoculated, and became paraplegic. In the same year ('Comptes Rendus,' vol. 89, p. 444) in twenty-five cases he transmitted rabies from a dog to rabbits, the incubation period being from four to forty-three days, the average in twenty-five cases being eighteen.

The results of intracranial inoculation with virulent medulla, or with the secretion of the salivary glands, of either rabid dog or rabbit, are, as before stated, in all essential respects identical with those that follow subcutaneous inoculation of the same matter—in the small proportion of instances where this is successful—or with those induced experimentally by the bite of a rabid dog, though in these, as described below, the incubation period is of very variable and uncertain duration and much prolonged.

The first symptom of infection in rabbits is usually, as in the dog, dulness; the animal sits up with its eyes closed, its head frequently thrown back and inclined to one side. In some few cases, though exceptionally, and not exceeding 3 or 4 per cent., the animal is restless and excitable, running round and round its cage, and altogether hyperæsthetic; still more rarely is it aggressive, in one case and one only out of upwards of 200, have I seen a disposition to bite, and in two or three others an inclination to butt. This as in dogs depends no doubt on the disposition of the animal; tame rabbits are usually quiet and familiar enough, but those used to the care of them state that occasionally a normal rabbit in confinement will attempt to bite a hand put into its cage.

Concurrently with this change, there is a rise of the rectal temperature of about 1° C. from 39·2—39·8°, the normal, to between 40° and 41° C., seldom exceeding the latter. This rise is, I believe, invariable, in the regular course of the disease, that is, if not influenced by the action of drugs or other circumstances; it is very transient, and may occur during the night and easily be unobserved. Usually it lasts about twenty-four hours and then begins to fall more or less quickly, *pari passu* with the progress of paresis, which is the essential feature of this disease in the rabbit. At first the animal moves slowly and with reluctance, its gait becomes unsteady, the loss of power usually commencing in the hind limbs; it then entirely loses the use of them; they are dragged after it if it moves, scrambling along by its fore-legs; it lies on its side with its hind-legs stretched out; respiration which was at first accelerated becomes slow and feeble, the muscles of the trunk and those of the fore limbs are successively paralysed, lastly those of the head and neck, the animal continuing to feed to the very last, frequently dying with hay in its mouth and between its teeth. The motor nerves alone appear to be affected in the rabbit, the reflexes remaining unimpaired to the last. A comatose state always precedes death, which is very gradual and imperceptible, the temperature continuously falling to a very low point. The immediate cause of death appears to be paralysis of the respiration, in those animals of which I have witnessed the death. I have found the heart continue to contract for some time afterwards, in one case for nearly half an hour.

The post-mortem appearances in the rabbit are better marked and more constant than in the dog. The brain and medulla are more frequently hyperæmic; in the majority of cases they are materially softened, which is not altogether dependent upon the duration of the symptoms; sometimes the spinal cord especially is so soft that it is difficult to detach a portion of it entire. The microscopical appearances are similar to those described in the case of the dog, but hæmorrhages in the substance of the cord, so frequent in the latter animal, are not found in the rabbit.

Continuing to feed till the very last, the stomach is usually full of partially digested food, as is frequently the gullet, in this differing markedly from the dog. The stomach constantly shows congestion, with hæmorrhagic spots in almost every case; they may be minute and very few in number, only two or three, but are always present unless in those exceptional cases where death has followed very shortly after the appearance of the first symptoms. These hæmorrhages are similar to those in the dog, occurring in the same situation, viz., chiefly towards the cardia and on the greater curvature, but are usually more conspicuous, attaining a larger size and sometimes becoming confluent, covering a large portion of the wall of the stomach.*

The small and large intestines are generally normal, the fæces in the lower bowel being firm; in summer, however, diarrhœa is sometimes present, though this is probably due to other concurrent causes, and not to the specific action of the virus. I have never observed its occurrence during the winter months. The same remark as to its cause applies also to the loss of condition and emaciation that is sometimes found.

The subcutaneous tissue is generally very vascular, and small patches of congestion are found, which to superficial observation appear as red spots of variable extent.

The larynx and trachea are almost invariably hyperæmic, frequently intensely so; the lungs are as frequently congested to a variable extent, the margin of the lower lobe being usually the seat of the greatest changes; sometimes portions may be found consolidated or cyanotic, though this not unfrequently occurs in tame rabbits kept in confinement, and independently of experiment.

The liver is frequently enlarged, almost invariably congested, and often engorged with dark blood; in only two cases out of upwards of 100 noted have I observed it perfectly unchanged and healthy.

The spleen in nearly one-third of the cases is small. I have never observed it materially enlarged or softened.

The kidneys are frequently congested, and the urinary bladder is

* These are correctly represented in the accompanying drawing, fig. 3, of a very well marked case.

always very vascular, generally distended with urine, frequently to an enormous extent. In one case I observed it of nearly the size of an ordinary soda-water bottle, filling and distending the abdominal cavity; the urine is strongly acid, and in other respects normal.

The blood, as in the dog, is always dark coloured, sometimes fluid, but as often clotted, and I have observed in several instances that the clot in the cavities of the heart was colourless and hyaline. Its morphological characters are unchanged, and as in the dog its reaction is neutral.

IV. *Seat of the Virus and Results of Inoculation.*

In July last, having obtained a rabid "street dog," upon its death another dog and a rabbit were inoculated by intracranial injection of a portion of its medulla, prepared as above described.

The dog was unaffected till the seventh day, when it was found dull, lying crouched up in a corner of the cage; the next day evident symptoms of rabies were apparent, the animal being restless and irritable, flying at and biting anything presented to it, with commencing paresis of the hind limbs; it was never heard to bark, and died on the following day, the 9th. As this was Sunday no further changes had been noted.

Examined the next day, the stomach and small intestine were found devoid of solid contents, containing only a dark-brown fluid; congestion was apparent in the outer wall of the stomach; the appearances of the other organs were characteristic of rabies, as hereinbefore described, but less marked than in many other cases, owing to the very rapid course of the disease, of the nature of which there could be no doubt.

To prove this further, from its medulla a rabbit was inoculated intracranially, this animal showed an incubation period of only four days, when with a scarcely appreciable rise of temperature paresis commenced, and was complete on the sixth day, the animal being found dead on the morning of the seventh.

The brain and spinal cord were found much softened, and with their membranes distinctly congested; the lungs presented typical appearances; the stomach was highly vascular but showed no hæmorrhages, in accordance with the rapid course of the disease, which, with the remarkably short incubation period, confirmed the view of the very active character of the virus, which the previous cases had suggested.

From the medulla of the first above-mentioned case of street rabies, the rabbit (narcotised by chloral hydrate) which was inoculated intracranially, similarly to the dog, on the fifth day showed commencing paraplegia; this continued for the next two or three days; the animal then partially recovered. It however ultimately relapsed and died on the 23rd day.

On post-mortem examination the appearances were found to be very distinctly marked and diagnostic, the brain and spinal cord congested and softened, the stomach showing moderately large and distinct hæmorrhagic spots (ecchymoses); the condition of the other organs, too, was typical.

In this case, as in the dog, the incubation period was remarkably short. The temporary recovery is unparalleled in my observations, but is recorded by M. Pasteur as sometimes occurring. Other inoculations were made in the same manner intracranially with virus from different sources, all with similar results as to infection and the symptoms produced, varying only in the length of the incubation period.

These cases in dogs and rabbits proved sufficiently that by cerebral inoculation of a healthy animal with portions of the medulla of a rabid street dog, or an animal infected from it, paralytic rabies is produced, which in the dog does not differ in its essential characters from ordinary street rabies; in the rabbit, however, its occurrence was not so well recognised previous to M. Pasteur's experiments, and the symptoms are different from those in the dog.

In order, therefore, to meet the objection that these symptoms are not those of infective rabies or lyssa, subcutaneous inoculations with infective medulla were practised.

With this object a young healthy dog was injected under the skin of the back with half a c.c. of the mashed cord of a rabbit that had just died with the usual symptoms of paralytic rabies.

The dog, beyond at times an apparently increased irritability and disposition to bite, which may have been merely the result of confinement, showed no appreciable change until the thirty-ninth day after inoculation, when it was observed to be markedly snappish and irritable; on the following day it was very dull and indisposed to move or notice anything; this increased, and it became paralysed in the hind limbs, lying on its side; there appeared constant irritation of the skin, at which it was perpetually scratching, with continued twitching of the muscles of the neck and trunk; it frequently uttered a short yelp, altered in tone and characteristically metallic, but not the typical prolonged howl of rabies.

It died during the night of the forty-second day, with post-mortem appearances that were sufficiently characteristic; it was clearly a case of rabies with tetanic symptoms more pronounced than usual, but in its essential characters did not differ from street rabies in the same animal.

In similar manner rabbits were inoculated subcutaneously; many experiments failed to produce infection, as did also one in another dog. In some cases the rabbits died of sapræmia (septic intoxication), to which these animals are extremely liable.

One rabbit, however, inoculated October 18th, 1886, subcutaneously with virulent medulla, on the twelfth day showed symptoms of infection, with weakness of the hind limbs, the temperature being below the normal and falling. On the fifteenth day it was completely paralysed and died in the afternoon, the post-mortem appearances being highly characteristic, the stomach showing numerous large well-defined hæmorrhages, before described, conspicuous from the serous surface as well as on the mucous membrane.

In another rabbit inoculated in similar manner subcutaneously, the result was precisely the same, excepting that the incubation period was shorter.

The results of these inoculations in both dogs and rabbits showed conclusively that rabies is produced in both animals, alike by subcutaneous and by intracranial inoculation of infective medulla of both dogs and rabbits, confirming M. Pasteur's statements in this respect.

It was subsequently found that in the rabbits and dogs thus inoculated subcutaneously without producing infection, no protection was afforded against the effect of subsequent intracranial inoculation, which in every instance produced fatal infection.

Still further to dispose of the objection that the symptoms following intracranial inoculation are not due to specific infection, a rabbit was inoculated sub-durally by trephining with the usual quantity (0·1 c.c.) of mashed spinal cord of a healthy rabbit. The animal remained perfectly unaffected in any way for upwards of a month; it was then again inoculated intracranially with virulent medulla, by which it was infected, and died after a short incubation period with the usual symptoms and post-mortem appearances.

I have also made several other inoculations intracranially in rabbits, employing two or three animals at the same time, with medulla of suspected cases of rabies in different animals. In many of these specific infection and death, with typical symptoms and appearances, have followed, but in those cases where the material used has not been specifically infective, the injections have been perfectly innocuous, the animals being in no wise affected by the operation.

I have used for intracranial and sub-dural inoculation quantities of medulla, mashed and diluted in bouillon, of from 1 to 10 minims,* with the same results, without, in the large majority of cases, any disturbance, previous to or beyond the regular symptoms of infection, following the operation, and without any perceptible difference in the incubation period. Some few animals, especially during the hot weather of August and September, died from accidental causes,

* The latter quantity, however, is much greater than it is necessary or desirable to use.

parasitical, lung disease, or other ailments, such as diarrhœa, and an epizootic form of nasal catarrh, invariably and rapidly fatal, to which rabbits in confinement seem to be very liable.

For the sake of uniformity I have latterly always used in these intracranial inoculations 0·1 c.c., or one minim and a half of the mashed medulla.

Another experiment was as follows:—Two dogs were inoculated, 18/9/86, from the medulla of a rabbit of short incubation period. The one a rough terrier, D 8, intracranially by trephining, the other a smooth terrier, D 9, by injecting half a c.c. of mashed medulla into the tibial vein.

Two rabbits were inoculated intracranially from the same cord; they both died infected, with typical symptoms and appearances, after an incubation period of seven days.

Both the dogs, however, D 8 and D 9, remained unaffected. The one, D 8, after the lapse of four months was then bitten sharply by a rabid dog in several places on the fore-leg, which had been previously shaved; but again in upwards of two months more has shown no symptoms of infection, though some rabbits bitten by the same dog were infected and died in the usual manner. The other dog, D 9, after the lapse of some months was again injected in the tibial vein with half a syringeful of virulent rabbit's medulla, but it also up to the present time (five months after inoculation) has shown no disturbance, though two rabbits inoculated intracranially from the same virus died infected in the usual course.

This result was quite unexpected, both from my own experiments with rabbits and from the statements of others; it shows how very much more strongly refractory to the infection with the virus of rabbit rabies dogs are than are rabbits themselves, in which, by intracranial inoculation, infection is produced almost invariably.

All immunity from, or refractoriness to, infection is relative, as in the original case of vaccination against variola, and also in rabies, as shown conclusively long ago by Hertwig (*loc. cit.*, *infra*), also by Chauveau in the refractoriness of Algerian sheep to anthrax ('Comptes Rendus,' vol. 90, 1880, p. 1525), and stated in express terms by Pasteur himself in reference to the general theory of protection, (*ibid.*, vol. 90, 1880, p. 953). Its bearing upon testing the results of inoculation in dogs, with the object of prophylaxis, is referred to below.

To examine the infectivity of the peripheral nerves, I took a portion of the sciatic nerve of a rabbit recently dead, one of a series of six or seven days' incubation period, and triturated it with bouillon in the usual manner, but as it was more viscid or tenacious than the medullary substance, I was obliged to dilute it more than in the usual proportion, in order to render it sufficiently fluid to inject.

With it I inoculated three rabbits intracranially by trephining; all three showed a rise of temperature towards the end of the sixth day, and died with typical symptoms and appearances of infection shortly afterwards.

As I had diluted the nerve substance about twice as much as I usually did medulla, and only injected the same quantities, producing infection without any variation in the incubation period, it is shown to be fully as virulent as the cerebro-spinal substance; and we may conclude that the tissues both of the central and peripheral nervous systems are equally the seat of the virus.*

At an early stage of the investigation I made a series of experiments upon the relative activity of the virus of the spinal cord and medulla oblongata of the same animals, and I found that as shown by the duration of the incubation period, there was no appreciable difference whatever in the infective virulence of the two.

I have tried the infectivity of the tissues of the salivary glands of a rabid dog, or the secretion expressed from them, taking portions of the parotid and submaxillary, crushing each in a mortar, adding a small portion of bouillon, with which it was macerated for a time, the fluid then being injected intracranially in rabbits.

Of two rabbits inoculated from the submaxillary gland one died on the 2nd day apparently from an accidental cause, the other was found dead in the morning of the 4th day, no symptoms of infection having been apparent nor any pathognomonic appearances on post-mortem examination. Two other animals were inoculated intracranially from its medulla and both remained unaffected. The two rabbits inoculated from the parotid both developed symptoms of infection on the 17th day, and died during the 20th with typical appearances, showing apparently that the tissues or secretion of the parotid gland are infective, but much less actively so than the medulla.

* Hertwig (*op. cit., infra, vide* p. 65) failed to produce infection in six dogs inoculated with the crural and sympathetic nerves of other rabid animals.

Rossi, of Turin ('Torino, Accad. Sci. Mem.,' 1805–1808, p. 94) asserts that he produced rabies in a dog after an incubation period of eighteen days by inoculating it in the tail with a portion of the crural nerve of a rabid cat just killed. This cat, however, was one of "several" which he states that he rendered rabid by confining in a room without food or drink; then killing them, he inoculated the different fluids of the body to ascertain which besides the saliva most readily induced rabies, and found that only that secretion *and the nerves while warm,* did so. In a subsequent communication ('Memorie,' &c., vol. 30, 1826, p. 22) he further states that two dogs became rabid by being bitten by a cat confined as described, but that by similar means he was unable to excite rabies in dogs.

His experiments appear to have been numerous, but his statements on this point are difficult to understand if his own conclusion be rejected, that rabies in the cat is spontaneous and may be produced experimentally.

The infectivity of the blood of rabid animals has been a moot question, some accounts having asserted infection to have been produced by inoculation with it, the experiments having, of course, been made by subcutaneous inoculation.

I have taken blood from the heart of a rabbit recently dead of rabies, defibrinated it by whipping with a sterilised glass rod, and injected portions of 0·1 c.c. subdurally in rabbits, but have not succeeded in producing infection, the animals as long as observed being unaffected in any way.

It seems probable that, as in other analogous cases, this fluid is but exceptionally infective, or only so in large quantities, as it is not the primary seat of the virus, which we now know to be principally in the tissues of the central nervous system.

The value of these methods of intracranial inoculation with rabbits, from the greatly curtailed incubation period and practically certain resulting infection, for the purpose of determining whether a suspected case is one of true rabies or not, is obvious.

In one instance a dog was destroyed at Caterham in a state, apparently, of furious rabies, after having bitten several persons and other dogs; as it was very desirable to ascertain positively the nature of the case, I inoculated from a portion of its medulla a rabbit, which after sixteen days developed symptoms of infection, and died shortly afterwards of paralytic rabies.

In another instance I received a portion of the spinal cord of a dog that had bitten several persons at Grantham, but which, as was stated, showed no symptoms of rabies; from the cord I inoculated a rabbit by trephining, and after nineteen days symptoms of infection appeared, the animal dying in the usual manner, leaving no doubt as to the dog having been rabid.

The duration of the incubation period, too, being proportionate to the activity of the virus, which varies from different sources, may in case of death from hydrophobia, afford a means of determining the source whether, *e.g.*, infection arose from the bite of a rabid street dog, or was caused by inoculation with rabbit virus.

This must, however, be received with some reservation, the incubation period resulting from inoculation with the virus of street rabies in some cases, though very exceptionally, being even shorter than that of the Pasteurian or constant rabbit virus, as shown in two of my experiments (*supra*, p. 58) where in a dog and rabbit inoculated subdurally from the cord of a street dog, this period was respectively seven and five days, and in another rabbit similarly inoculated from the last-mentioned dog, it was only four days.

V. *On the Occurrence of Infectivity in the Tissues after Inoculation.*

The period at which the tissues of an animal inoculated may become virulent, or the bite of a dog be infective, is of importance, and as yet there are no observations on record to enable us to form an opinion on this point.

I had found in numerous experiments that if a rabbit was killed upon the termination of the incubation period, on the appearance of the first appreciable symptoms, its medulla was as actively infective as that of an animal which had died after the disease had run its full course.

To determine at what period this infectivity is developed I inoculated five rabbits, A, B, C, D, and E, intracranially in the usual manner from a medulla of six to seven days' incubation period.

Of these A was killed towards the termination of the 2nd day, about 44 hours after inoculation, and from its cord two others, A 2 and 3, were also inoculated intracranially. Another animal, B, was killed at the expiration of the 4th day, and B 2 and 3 were similarly inoculated. A third, C, first showed symptoms of infection towards the close of the 7th day, about 164 hours after inoculation; it was thereupon killed, and C 2 and 3 were inoculated intracranially. D and E, which developed symptoms during the same day as C, were allowed to die in the regular course of the disease; the one was found dead on the 10th, the other died during the 11th day, with typical symptoms and appearances. From the medulla of this latter two other animals, E 2 and 3, were inoculated.

Of these rabbits, A 2 and 3 as well as B 2 and 3 were altogether unaffected, with the exception of a slight and transient rise of temperature on the 5th day in A 2, which was probably accidental.

C 2 and 3 developed symptoms by the 7th day, which took their usual course. No. 2 was found dead on the morning of the 12th day, No. 3 dying during the same day.

D 2 and 3 showed an incubation period of six to seven days, and died shortly afterwards.

From this it appears that the spinal cord of an infected animal is not itself in anywise virulent till towards the close of the incubation period, concurrently with the appearance of the first symptoms of constitutional disturbance. I think we may conclude from this that the virus is latent at the site of inoculation till this period, when, somewhat suddenly, it bursts forth and pervades the tissues. In the case of bites from rabid animals this seems to suggest the possible utility of excising or deeply cauterising the wound, even at a subsequent period, and throws great doubt upon the authenticity of those cases where hydrophobia has been said to have been occasioned by the bite of an animal, which itself remained unaffected for a considerable time afterwards; and I should be disposed to conclude that if such an animal developed no symptoms of rabies for some days after having bitten a person or other animal, the latter would be safe from any danger of infection.*

* The most extensive and important observations and experiments upon this subject ever recorded before M. Pasteur's are those of Hertwig, made in the Veterinary School of Berlin between the years 1823 and 1827. They are published in Hufeland and E. Osann's 'Journal f. prakt. Heilkunde,' Berlin, vol. 67, 1828, (Beit. z. nähern Kentniss d. Wuthkrankheit, oder Tollheit d. Hunde, von Dr. Hertwig). They have been but little noticed by English writers, important as they are. Their chief results are :—

(1.) Of 16 dogs inoculated with the saliva of others rabid, by puncturing the skin of the head, 6 died infected.

(2.) Of 7 similarly inoculated with the secretion expressed from the parotid glands, 1 was infected.

(3.) Of 2 inoculated with the crural and 4 with the sympathetic nerve, no infection resulted.

(4.) Saliva put in the mouth of more than 20 dogs in no case produced infection.

(5.) Of 11 dogs inoculated with the blood of others rabid, taken during life and shortly after death, no specific infection occurred.

(6.) Of 15 caused to be bitten by others rabid, 5 died infected; but of 137 apparently brought to the infirmary bitten by others rabid or (qy.) supposed to be so, only 6 died infected.

His own pug was inoculated nine times during three years and resisted infection, but succumbed to a subsequent trial. He remarks (*op. cit.*, p. 172) that of the other dogs that died after inoculation, some withstood infection two, three, or four times, and one died at first, clearly showing how variable is the degree of refractoriness possessed by different animals.

His observations upon the symptoms and appearances in upwards of 200 cases that he examined are carefully recorded, and his statement that he compared the latter with those of healthy animals shows the scrupulous care with which they were made. Their description is fully given in his later work 'Die Krankheiten d. Hunde u. deren Heilung' (Berlin, 1853).

A series of observations made upon even a larger number—375 cases of street rabies, in the Veterinary Institute of Vienna, during twenty years—is that of Bruckmüller, recorded in his 'Lehrbuch d. Pathol. Anatomie der Hausthiere' (Wien 1869). He found a morbid appearance in the stomach in 254 cases, or nearly 70 per cent., with the presence of foreign substances in it in 199 cases, or 55 per cent.; it was "inflamed" in 125, or 33 per cent.

It appears probable, however, that some at least of these cases may have been destroyed during the progress of the disease, and not improbably some of them may not have been cases of true rabies, which circumstance would materially affect the proportions of pathognomonic appearances observable.

The best work extant on this subject, of both literary merit and scientific accuracy, is the well-known 'Rabies and Hydrophobia,' by George Fleming, LL.D., Principal Veterinary Surgeon to the Army (London, 1872), which gives a complete and excellent account of the disease in all its relations, with a notice of the principal publications up to that time. Of these the best in the English language are those of William Youatt, M.R.C.V.S. ('The Dog,' London, 1845, and 'On Canine Madness,' London, 1830), in which the account of this disease in the lower animals is given from the numerous cases observed in his own extensive practice.

VI. *Duration of the Incubation Period.*

The great variability of the incubation period and the extreme length to which it may extend after the bite of a rabid animal, is well known, and constitutes the most unaccountable feature of this disease. It is well established that both in man and the lower animals it may extend to at least several months, and even periods of some years have been recorded upon apparently good evidence. Subcutaneous inoculation with saliva taken from the mouth in all the experiments which I made failed. In intracranial inoculations with the secretion expressed from the parotid gland, as previously described, the incubation period was seventeen days.

By subcutaneous inoculation with the medulla of street rabies, it is uncertain and generally prolonged, both in dogs and rabbits, but by intracranial inoculation it is much shortened and more regular. I have had two cases above recorded in rabbits, where the first symptoms appeared on the fourth and seventh days respectively, after intracranial inoculation, but this is most unusual with virus from this source, *i.e.*,

The most important recent work upon the subject in English is the article by Bollinger, in the American translation of Ziemssen's 'Cyclopædia of the Medical Sciences,' which gives a good account of the ætiology of the disease, its symptoms, and other features, both in man and the lower animals, with a notice of the previous literature.

A copious bibliography is contained in the 'Dictionnaire Encyclopédique des Sciences Médicales,' ed. by A. Dechambre, Paris, 1874, article "Rage." The list is brought up to the date of the commencement of Pasteur's investigation and the inauguration of the present views upon the subject in the 'Nouveau Dictionnaire de Médecine et de Chirurgie Pratique,' by Dr. Jaccoud, vol. 30, Paris, 1881.

The most complete account of the literature of the subject, however, is that given in the invaluable 'Index Catalogue of the Surgeon-General's Office U.S. Army.' Washington, 1885, vol. 6. Art. "Hydrophobia."

The communications to the Royal Society upon rabies or hydrophobia have not been numerous or important; they are mainly records of cases in man and reports of asserted cures. One of these by Dr. James has been above referred to.

Amongst the more recent publications upon this subject may be mentioned that of M. Bourrel, a veterinary surgeon, formerly in the French Army, and director of the Institution for the Study of Canine Pathology in Paris. His observations ('Traité complet de la Rage, chez le Chien et chez le Chat, Moyen de s'en préserver, &c.,' Paris, 1874) are important on account of the very large number of cases in the dog which he had the opportunity of observing.

Between the years 1859 and 1872, as he states, out of 18,531 dogs admitted to the establishment 1219 were rabid. He advocates the prompt application of caustic to the bite of a rabid animal, and admits that the enforced muzzling of dogs had been beneficial; but his specific to abolish all risk to man from this disease is by filing down the points and sharp edges of the canine teeth and incisors of all dogs.

Since the publication of M. Pasteur's results the only independent investigations as yet recorded are those, before referred to, of Professor Frisch, in Vienna; and, more recently, in the 'Annales de l'Institut Pasteur,' Paris, March, 1887, those of Dr. Bardach, of Odessa, noticed below.

rabid street dogs. In other cases the period has been from seventeen to nineteen days, which appears to be about the average, and agrees nearly with that given by M. Pasteur and Prof. Frisch.

When the virus of street rabies is passed through a sufficient number of rabbits the period is further reduced to six or seven days, and becomes markedly constant. In a period of about six months I have carried this virus, originally obtained from M. Pasteur's Laboratory, through a series of twenty rabbits, inoculating two or more of each series. In the large majority of cases the first symptoms of infection have appeared between the sixth and seventh days, exceptionally not till the eighth day, in a few instances till the ninth. Latterly I have observed two cases in which the latent period was only four days.

In one case quite recently, two rabbits were inoculated intracranially from one of a Pasteurian series, that had died after a very unusually short incubation period, with characteristic symptoms, but of only some hours' duration, on the fourth day after inoculation. One of these so inoculated died on the third day, apparently from accidental causes, the other remained unaffected and healthy till the fortieth day, when it was observed to be paralysed, and was found dead the following morning, with post-mortem appearances that were very well marked and characteristic; the hæmorrhages in the stomach, though not perceptible on the serous coats, were larger on the mucous surface, though few in number, than any other I have observed, almost resembling, as has been described, "crushed currants." This duration of the incubation period is quite exceptional.

From the medulla of this case two other rabbits were inoculated by trephining; they both showed an incubation period of the usual length—six to seven days—with well-marked symptoms, thus proving that the remarkably protracted incubation period in the above case was due to some accidental cause, and that the virus had undergone no permanent modification.

I have had the opportunity of inoculating intracranially from the medulla of a rabid horse, in this case with an incubation period of seventeen days, and from a rabid ox, as also from a case in man, in all of which it was about the same, and the symptoms of infection and post-mortem appearances were identically similar to those following inoculation from the dog or rabbit.

I have also inoculated rabbits from the medulla of a furiously rabid cat,* which had been itself inoculated from a street dog. In this case the incubation period—in the rabbits—was between seventeen and eighteen days, with the regular symptoms and post-mortem appearances.

That in inoculating rabbits intracranially, the duration of the incubation period is usually determined by the activity of the virus,

* The same animal that bit the man, Joseph Smith, hereinafter mentioned.

and only very exceptionally by any reaction of the animal inoculated, is shown by the circumstance that, however many animals, of the same species, are inoculated from the same source, this period generally shows no variation at all in them, though the duration of the disease, and consequent occurrence of death, is evidently dependent mainly upon the age, condition, and vigour of the subject.

VII. *Preservation of the Virus and Methods of Modification.*

On the occurrence of septic decomposition in the medulla the virus is destroyed, but M. Pasteur has stated that by removing portions of virulent medulla with precautions against contamination, and suspending them in an atmosphere of pure carbonic acid, their infectivity is retained unimpaired for some weeks.

I have not, however, found this to be the case; I have in several experiments carefully removed a portion of the cord of a rabid rabbit, passing it and the ligature to which it was attached through the flame of a spirit-lamp and suspended it in a vessel, previouly disinfected, plugged with sterilised cotton-wool, and kept saturated by a constant current of CO_2, filtered through cotton-wool.

In every case I found that within a few days the virus was materially modified, and soon completely destroyed, the rapidity of the change probably depending upon the temperature. In summer on the third day this diminution in virulence is apparent in the results of inoculating rabbits with it. Septic microbes, however, do not develop in the medullas, as long as kept in this manner.

I consequently find this method unsuitable for preservation of the virus for even the shortest period.

The basis of M. Pasteur's present methods of protective inoculation consists in the asserted progressive modification and ultimate extinction of the virus which is produced by suspending a portion of infective medulla in a current of dry air. The methods adopted are to take out a portion of the spinal cord of a rabid rabbit soon after death, then passing it lightly through the flame of a spirit-lamp,* in order to destroy any septic germs which may have fallen upon its surface, to suspend it by a ligature similarly flamed, in a previously sterilised bottle, with tubulature at top and bottom, plugged with cotton-wool, and containing a quantity of caustic potash.

Thus prepared the cords are gradually dried; the potash, absorbing all moisture, prevents any development of septic organisms quite effectually. A portion of a cord dried for the length of time required for inoculation is then triturated with bouillon, strained as before described, and injected subcutaneously.

* The sterilisation however is superfluous, inasmuch as saprophytes do not develop in dry air.

Pasteur states ('Comptes Rendus,' vol. 101, 1885, p. 770) that the progressive modification of virulence in cords thus preserved is attested by the increasing length of the incubation period in rabbits inoculated intracranially with them, and that the duration of this period increases regularly with those dried up to seven days, but that from and after that period they are not virulent.

I have myself in several experiments invariably found the latter part of this statement correct, and that cords dried for seven days or more are absolutely inert, as are frequently those of six and of five days.

But I have not by any means found the progressive prolongation of the incubation period, with cords dried for a shorter time, as regular as he records, but on the contrary I have found it usually the same as with fresh cords, unaltered up to and including the fourth day; in one case only when inoculating from one dried four days, did the first symptoms of infection, which were not well marked, appear to be deferred till between the ninth and tenth day, the animal dying on the thirteenth.

As in cords thus treated the virus certainly becomes altogether extinguished, *teste* Pasteur, and, as I have myself found, somewhat suddenly, by the seventh day, it appears doubtful what benefit can result by inoculating subcutaneously with those of a longer period, with the object of prophylaxis.

The preservation of cords by this method, in an atmosphere kept perfectly dry by caustic potash, entirely prevents the occurrence of microbial putrefaction; its absence is plainly evinced by no fetor being perceptible in them, as saprophytes are unable to develop in a perfectly dry atmosphere, as well as in one of carbonic acid.

I have too examined with the microscope portions of cords thus preserved for different periods, but have never recognised any microphytes; if they had been present in any numbers they could not have escaped observation.

To prove their absence certainly, I took part of a cord preserved as described for five days, and snipping the outer surface, which was dry and firm, I plunged a sterilised platinum needle into its substance, which was moist and viscid, adhering in very appreciable quantities to the needle. With this I inoculated a tube of agar-agar bouillon peptone, and performed this operation three times; the tubes thus inoculated were placed in the incubator at 38° C., no organisms developed, and their contents remained altogether unchanged until they ultimately dried up, showing the total absence of septic microbes.

I may add that in a room of any ordinary laboratory it would, I believe, be practically impossible to remove any number of cords and transfer them to the requisite vessels, without some germs of septic organisms falling upon them during the operation; and that consequently the only reliable means of preserving them from septic changes is by keeping them under conditions where saprophytes cannot develop, such as that adopted by M. Pasteur and here followed. Another method would be to keep them at a very low temperature.

The result of attempting to protect rabbits by subcutaneous injections with medullas treated as above, is in accordance with these observations. Here, as recorded below, in the first series of experiments, where large quantities were injected, death shortly followed from sapræmia in every one of the animals inoculated ; in the second series, using smaller quantities for injection, fewer deaths from the same cause occurred, illustrating the distinction between infection with a specific bacterial virus and intoxication by a chemical poison, viz., that in the former case within certain limits, the result is independent of the quantity inoculated, one viable germ producing the same effect as an immeasurably greater number; but in the other case—the action of a soluble or chemical ferment or poison—the effect is directly and obviously proportionate to the quantity used for inoculation.

VIII. *Protective Inoculation.*

In the first series of experiments upon rabbits, five were taken and inoculated daily after M. Pasteur's original methods with half a Pravaz syringeful—about 0·7 c.c.—of mashed spinal cord, commencing with that dried as just described for 15 days; on the third day with one of 13 days; on the fifth with one of 11 days; the sixth with one of 10; and so on daily, or as often as a cord of the requisite age was available, till the thirteenth and last inoculation was made with a cord dried one day only, and as several previous experiments had shown, of unmitigated virulence, at least for rabbits.

Three of the rabbits, however, had died during the course of the inoculations; one, the youngest of the batch, which died first, apparently from accidental causes, the two others from sapræmia; but two remained for the concluding inoculation, and these both died a few days after it was made. None of them, however, showed any symptoms of infection with rabies, they were those of sapræmia or septic intoxication. The series of experiments was inconclusive therefore in its results, and it seemed possible that the quantity of matter injected was too large.

In a subsequent communication ('Comptes Rendus,' 2nd Nov., 1886) M. Pasteur, objecting to the results of similar experiments published by Professor Frisch, of Vienna (' Wiener Med. Wochenschr.,' referred to below), promulgated a new "rapid" or "intensive" method of treatment, which appeared likely to be more successful with rabbits, liable as these animals are to septic poisoning by inoculation with any foreign matter. Accordingly six rabbits were taken, all apparently healthy, and were inoculated in the following manner, with medullas of progressively increasing virulence, 0·15 c.c. of the mash prepared as above described being used in each subcutaneous inoculation.

On the	1st	day,	morning,	cord dried	13	days.
,,	,,	,,	evening,	,,	11	,,
,,	2nd	,,		,,	9	,,
,,	3rd	,,	morning	,,	6	,,
,,	,,	,,	evening,	,,	4	,,
,,	4th	,,	morning	,,	3	,,
,,	,,	,,	evening,	,,	2	,,
,,	5th	,,		,,	1	,,

Of the rabbits thus inoculated, one was found much wasted and partially paralysed behind, with a falling temperature, on the 6th day after the concluding inoculation, it died on the 9th day with well marked and unmistakable post-mortem appearances.

A second animal died on the 11th day after the last inoculation with symptoms and appearances that clearly showed infection. A third was first affected on the 19th day, and died on the 22nd, clearly of paralytic rabies.

Two others died some days after the completion of the inoculations with appearances of sapræmia. One remained in good condition and unaffected; this on the 24th day after the last inoculation, was injected intracranially with 0·1 c.c. of mashed medulla of a rabid rabbit just dead. On the 6th day following, the temperature, previously normal, rose to 40° C., and the following day was the same, with commencing paresis. The symptoms followed the usual course, and the rabbit was found dead on the morning of the 11th day; the duration of the disease—between four and five days—showed the animal to be very robust and healthy, consequently a most favourable subject for protection, but the shortness of the incubation period—the test rightly applied by M. Pasteur to the activity of the virus—proves that it was not in any wise modified by any refractoriness induced in the animal by the previous inoculation; and I think it must be concluded from these experiments that the method followed, essentially in accordance with M. Pasteur's last published rapid method, is, as far as rabbits are concerned, inefficient to confer any immunity against subsequent infection, and dangerous as likely to produce it.

It must, however, be understood that M. Pasteur has not asserted in his communications to the Parisian Academy, that rabbits are capable of being protected. He has confined his statement to dogs.

Protective Inoculation in the Dog.—The dog should be a far better subject for these experiments than the rabbit, being far more resistent to septicæmia and sapræmia, and much less liable to those accidental affections, parasitical and others, by which the latter animal is constantly attacked; moreover, it is rightly regarded as the typical subject, the *fons et origo* of the malady here under investigation.

Two dogs were taken for this trial, the one a mongrel hound of medium size, Pr. No. 1, the other a rough white terrier, Pr. No. 2, and treated as follows:

1886, October 4. Both were injected under the skin of the back with half a Pravaz syringeful (about 0·7 c.c.) of mashed medulla of a rabbit of a Pasteurian series, dried thirteen days.

On the	5th	October	0·4	c.c. of a similar cord	dried	11	days.
,,	6th	,,	0·5	,,	,,	9	,,
,,	7th	,,	,,	,,	,,	8	,,
,,	8th	,,	,,	,,	,,	7	,,
,,	11th	,,	,,	,,	,,	6	,,
,,	12th	,,	,,	,,	,,	5	,,
,,	13th	,,	,,	,,	,,	4	,,
,,	16th	,,	,,	,,	,,	3	,,
,,	18th	,,	,,	,,	,,	2	,,
,,	2nd Nov.		,,	,,	,,	1	,,

These dogs both remained unaffected in any way whatever, and on the 6th November the first, Pr. No. 1, had the fore-leg shaved, and was bitten by a rabid street dog, the teeth of which penetrated the skin in several places, drawing blood, the saliva also was evident upon the leg, and was spread with a scalpel over the marks of the teeth, and where the skin had been cut in shaving.

This dog remained perfectly unaffected, lively, and good-tempered for more than four months after being bitten; it was then again inoculated by injection into the tibial vein of half a c.c. of active virus, again without showing any symptoms of infection up to this present time (twenty days after inoculation).*

At the same time a fresh dog, a rough white terrier, D 10, was similarly inoculated in the tibial vein with the same virus; this animal also remains unaffected, though rabbits inoculated intracranially from the same cord died infected in the usual course. This animal is obviously strongly refractory to infection.

The second dog inoculated for protection, the rough terrier, Pr. No. 2, was kept under observation without showing any disturbance till the 24th January, 1887, when it was inoculated intracranially under æther, with a full quantity of the mashed spinal cord of a rabbit of the Pasteurian series recently dead.

Two rabbits were similarly inoculated with the same virus, they

* P.S.—And for three months subsequently. 29/7/87.

both died with usual symptoms of infection, after an incubation period of 6—7 days.

The dog, however, after recovering from the effects of the anæsthetic, remained perfectly well and unaffected in any way, and appears, as the first, to be completely refractory to infection by the most active method of inoculation.

From these two cases I should have concluded that the methods of protective inoculation introduced by M. Pasteur were successful and efficient in dogs, but the cases of the three unprotected animals described (viz., D 8 and D 9 *supra*, and D 10), which are equally refractory to infection, do not support this conclusion as a result of the limited number of my experiments upon this point. The virus of rabbit rabies, almost invariably infective by intracranial inoculation to animals of that species, would appear to be less certain in its action upon dogs, and it is only by the results of a series of numerous comparative experiments that a final conclusion can be formed, whether these methods have or have not any effect in increasing the constitutional refractoriness of the dog to infection with rabies. I would add, however, that it appears to me from the more numerous recorded experiments of others upon dogs, viz., those of Professor Frisch, of Vienna, and those of Professor Horsley at the "Brown Institution," exclusive of those on an extended scale by M. Pasteur himself, in all of which infection seems to have been invariably produced by intracranial inoculation, that the principle of protection is established, and that in some cases at least, judged by the results of comparative experiments, increased refractoriness to infection in the dog is produced by the methods indicated, which is as much as could be expected or hoped for, immunity, as above remarked, being always merely relative.

With regard to the protective inoculation of man, the end and object of M. Pasteur's work, this cannot be conclusively judged by the result of experiments upon the lower animals of widely different constitution; for in the rabbit and the dog its effect is very dissimilar; all that this can do is to establish or disprove the principle of the method. It is by the statistics of the treatment in man that it must be judged. These will no doubt be examined exhaustively by the Parliamentary Commission now sitting.

Taking, however, the accounts last published ('Comptes Rendus,' 24th January, 1887) in which the number of patients treated is stated at 2682, and the deaths amongst them from all causes 31, or only 1·15 per cent., it appears probable that the treatment has been successful in at least some cases, since all published statistics, widely as they vary, give a mortality from the bites of rabid dogs much in excess of this.

But beyond this, inasmuch as the last injection in each course is made by Pasteur with virus dried for one day only and not materially or at all modified. this would presumably be infective in a considerable proportion of cases unless the patients were protected by the preceding inoculations.

I cannot, however, as above stated, avoid the conclusion that the rapid method of inoculation is dangerous. This opinion is confirmed by the experiments of Professor Frisch, of Vienna, the only independent investigation of these methods yet recorded.*

The statistics of his treatment must very shortly show whether the mortality amongst his patients has or has not increased since the practice of his intensive methods.

Within the last few days, too, since the above was written, this opinion of the danger of infection from the intensive or rapid method of treatment is strengthened, by a report published in the current number of the 'Annales' of the Pasteur Institute, by Dr.

* His first report ('Wiener Med. Wochenschr.,' 1886, April 24th, No. 17) in the main confirmed the results of M. Pasteur; twelve dogs protected by the original method resisted intravenous injection of acute virus, while three out of six control animals were infected.

In a second report (*ib.*, 7th August, 1886, No. 32), of sixteen rabbits inoculated by trephining, with fifteen of which preventive inoculations were commenced immediately afterwards, and continued daily for eighteen days, all save two died infected.

M. Pasteur having attributed this unfavourable result to the inoculations having followed one another too slowly, and recommended his rapid or intensive method ('Comptes Rendus,' 2nd November, 1886), Professor Frisch repeated his experiments on a larger scale, following the new method of inoculation, with almost uniform failure, and consequently concludes decidedly that this is dangerous.

Nor can I overlook a case in man, all the stages of which, both before and after his treatment by the rapid method of M. Pasteur, were within my own observation. It is that of Joseph Smith, or Goffi, of this Institution, already noticed in different journals. He was bitten sharply on the hand by the furiously rabid cat above mentioned; within a few minutes the wounds were well washed under the tap, sucked by himself, and then, together with his mouth, washed with a strong solution of permanganate of potash, again with water, and then thoroughly treated with anhydrous carbolic acid—absolute phenol. Shortly afterwards the parts bitten were excised under chloroform. The same night he was taken to Paris, and the following day his treatment by M. Pasteur's intensive method commenced.

Shortly after the completion of the course and his return home, he developed symptoms of spinal paralysis and died under circumstances which suggested the probability of his having been infected by rabbit virus. A report of the case will, I believe, shortly be published by those who had charge of it.

The wounds caused by the bite were thoroughly cauterised so shortly afterwards, that there was certainly every prospect of his escaping infection from that source. The symptoms developed and other circumstances seem to point clearly to the circumstance of his having been infected by subsequent inoculation, and not by the original bite. The incubation period in the rabbits which were, I believe, inoculated from his medulla, will settle this conclusively. It must, however, be remarked that he was debilitated during the treatment in Paris by the effects of intemperance, and consequently, no doubt, rendered more susceptible to infection by the inoculations to which he was subjected there.

Bardach, Director of the Bacteriological Institute at Odessa, where of 15 dogs inoculated intracranially with lyssic virus from different sources, and immediately afterwards subjected to protective inoculation by M. Pasteur's intensive method, 6 developed rabies, 9 surviving. Of 6 control animals, similarly infected, all died. Of the 6 protected animals that died, 3, as is shown, were infected with paralytic rabbit rabies, the result of the subcutaneous inoculations; again showing the dangers of this method. The proportion, too, of the survivors, 9 out of 15, or 60 per cent. is not favourable.

IX. *The Action of some Drugs upon Infection.*

The various substances and measures that have been tried as remedies for rabies are innumerable, from viper's venom to plain water; from time to time certain cases of cure have been announced, but a large proportion of these may obviously be accounted for by the absence of infection; others in which distinct symptoms of the disease are recorded are more difficult to dispose of, though some of them in man probably were not true hydrophobia or lyssa, but a nervous or hysterical affection simulating its symptoms,—lyssophobia.

In investigating the action of drugs upon animals infected with this virus, it appeared to me that two methods of treatment might be followed, the one to endeavour to destroy the virus, almost certainly a micro-parasite, by the administration of a germicide; the other, to treat the symptoms developed with appropriate remedies, and by the use of tonics and stimulants to enable the animal to survive the attack, when, as in other cases, the virus would have exhausted itself and died out. The explicit statement of M. Pasteur (*vide infra*), that spontaneous recovery in dogs does sometimes occur, seemed to offer some prospect of success by this method.

I naturally commenced with bichloride of mercury, as being not only the most powerful germicide known, but also almost equally active as an antizymotic, in the combination of these two qualities standing quite alone; it has, too, lately been stated that Dr. Theodore Cash had found it a prophylactic against infection with anthrax inoculated subsequently to its use. I had thus some hopes of its efficacy in rabies.

I found that 6 to 7 tenths of a milligram was about the maximum dose that could be safely given to a medium-sized rabbit; consequently I inoculated one, 26/9/86, intracranially with active virus, and three hours afterwards injected subcutaneously 2 minims of ½ per cent. solution of bichloride of mercury; this was continued daily, with the interval of one Sunday. The animal was unaffected in any way till the 9th day, when the temperature rose to 41·2° C., paresis with the usual symptoms of infection was observed, and it died on the 12th day with characteristic appearances.

In the control animal, infected in similar manner, the incubation period was of exactly the same length, and it died on the same day as the one treated with sublimate, which had been therefore obviously inoperative in this case to destroy or in any wise modify the action of the virus. Had it only prolonged the incubation for a few hours it would have encouraged further trial, but with the result here obtained I saw no object in this.

Benzoic acid is recognised as a powerful germicide: Graham Brown ('Archiv Exper. Pathol.,' vol. 8, p. 144) found its soda-salt remarkably destructive to the virus of diphtheria. Rabbits will take considerable quantities of this—benzoate of soda—continued for several days, without any ill effects.

A rabbit inoculated intracranially with rabies, 15/9/86, one hour afterwards received by subcutaneous injection 1 c.c. of a saturated solution of the salt, which was repeated daily; on the seventh day the animal, much wasted, showed symptoms of infection, with paresis and rise of temperature; it died on the afternoon of the 9th day with well-marked characteristic appearances. In a control rabbit similarly inoculated the incubation period was one day longer than in the animal treated with the benzoate, and it died about twelve hours later. Here, too, the drug obviously had no beneficial action, and even seemed to tend to shorten the incubation period, and assist the activity of the virus.

I next tried iodine, as an active germicide, dissolving it in a solution of potassic iodide. I found subcutaneous injections of 2 cgrms. of iodine were borne well, which is a materially larger quantity relatively to their weight than the established dose for man. Accordingly a rabbit was inoculated with active lyssic virus, 25/10/86, and an hour afterwards 1 cgrm. of iodine in solution was injected subcutaneously; this was repeated on the three following days, when the quantity was increased to 2 cgrms. On the afternoon of the 7th day, however, paresis appeared, and the temperature rose to 40·7° C., and on the morning of the 10th day the animal was found dead, with post-mortem appearances that were quite characteristic. In a control animal inoculated at the same time the incubation period was similar, and it died about eighteen hours after the first.

Thus iodine appeared as inert as the substances previously tried in its action on the virus.

The next remedy that suggested itself was chloral hydrate. This is not only a powerful germicide but has been often recommended as having a specific action upon the symptoms in rabies, acting directly upon the brain and spinal cord. Rabbits will take enormous doses of this; 4 grammes even, in an average rabbit, frequently producing but partial narcosis, and after a few hours no disturbance whatever.

A rabbit infected in the usual manner, 30/10/86, one hour afterwards was injected with 1 grm. chloral hydrate; this quantity was repeated daily till, on the 7th day, the animal was found to be paralysed, but most unusually, the fore limbs were affected more strongly than the hind; the usual rise of temperature was absent or escaped observation; it was found dead on the following morning, the 9th day. A control rabbit similarly inoculated, after an incubation period of between eight or nine days, died on the 12th day; another with an incubation period of eight days died on the 11th.

This result, though not favourable to the protective action of chloral hydrate, yet seemed to point to a modifying action on the virus in some respects. I had also observed the results of previous experiments which seemed to lead to the same conclusion. On the 31st July, 1886, a strong gray rabbit that had been partially narcotised by the subcutaneous injection of about 3 grms. chloral hydrate, was inoculated intracranially with infective medulla; this animal remained quite unaffected till the 28th October, when it was found to be partially paralysed with a falling temperature; it died on the 30th October; the post-mortem appearances were well marked and unmistakable.

Again, in an experiment previously referred to, a large rabbit was narcotised by the injection of 3 grms. of the same salt, and then inoculated intracranially from the medulla of a rabid street dog. On the 5th day partial paraplegia was apparent, but no rise in temperature, which, however, may have occurred previously and fallen again; the animal continued to feed well, and towards the 10th day appeared to be recovering, which it gradually did, and remained well till the 22nd day, when it was found dead—any previous recurrence of symptoms not being observed. The post-mortem appearances were remarkably distinct and diagnostic; there could be no doubt that it died of paralytic rabies.

As these were the only anomalous cases with intracranial inoculations of intensified rabbit virus, as regards the incubation period, that I had had up to this time out of upwards of sixty cases, it appeared to me that the results described in two instances could not be due to mere chance, and must be owing to the action of the drug previously administered. I therefore continued experiments with it.

To another rabbit inoculated intracranially with active virus, 1 gramme of chloral hydrate in solution was injected daily for seven days; general paralysis was then observed, but again the rise of temperature, usually one of the first symptoms, was not noticed, being probably inhibited by the action of the drug; the animal died on the following, the 8th day.

Two other rabbits similarly inoculated for control showed incubation periods of rather longer duration—eight to nine days, living till the 12th and 15th days respectively; the drug in this case had no effect in prolonging the incubation period or in modifying the symptoms. Again, to another rabbit infected in the usual manner, a smaller quantity of chloral—half a gramme—was given by injection daily till the 9th day, when, as in former cases, incipient paresis appeared, but not as usual, commencing in the hind limbs, the fore limbs being first strongly affected, in marked contrast to the regular course of the symptoms; there was again no rise of temperature; the animal gradually wasted, general paresis became complete, and it died on the 11th day.

Two control rabbits similarly inoculated showed incubation periods respectively of between four and five and seven and eight days, dying on the 6th and 11th days.

In this case the action of the drug certainly modified the symptoms and possibly delayed their development and fatal termination; another rabbit, therefore, inoculated intracranially from a rabid dog was, from the second day after inoculation, treated daily with chloral hydrate in quantities of from half a gramme up to 3 grammes till the 7th day; on the 9th day the animal was weak and losing condition, but without any symptoms of specific infection, and there was no rise of temperature. It now received 1·5 grm. bisulphate of quinine without any obvious effect. On the following day, morning and evening, 0·2 mgrm. bisulphate of strychnia, and subsequently 0·3 mgrm. was given till the 12th day, when paresis commenced, and the animal was obviously sinking, but without showing the usual course of temperature; it died on the 17th day with very well-marked post-mortem appearances. The control animals similarly inoculated showed incubation periods of 17 and 20 days, dying both on the 21st day; so that here again the action of the drug was unfavourable, and I was forced to conclude that, whatever effect it may have when administered previously to inoculation, when given subsequently it has no beneficial action at all.

Terebine is highly extolled as an antiseptic and as a remedy in many virulent diseases. Mixed 1 part with 4 of olive oil, it may be given to rabbits by subcutaneous injection of even 1 c.c. without disturbance. Accordingly after infection I gave a rabbit daily, morning and evening, 0·5 c.c. of terebine in olive oil. On the 10th day, however, it was found paralysed, with a fall in temperature, and died during the 11th day.

A control animal showed no symptoms till the 12th day, and lived till the 15th.

I tried this drug again with another rabbit, using larger doses, but with a similarly unsatisfactory result.

I next tried curari. The action of this on healthy rabbits is somewhat uncertain, a quantity of the same solution that at one time is borne without disturbance, at another being rapidly fatal. I found that about 0·3 mgrm. of the sample I had and of the solution as I made it was the maximum quantity that could be safely employed. To a rabbit inoculated sub-durally with infective medulla I injected subcutaneously 0·2 mgrm. on the 5th day, and subsequently 0·3 mgrm. daily; the animal was unaffected till the morning of the 14th day, when it was found weak in the hind limbs, the bodily temperature having fallen to 35·4° C. It died on the same day shortly after injection of 0·3 mgrm. curari. A control animal showed an incubation period of only nine days, and was found dead on the morning of the 12th.

In this case the curari appeared to protract the incubation period and prolong the life of the animal; I therefore repeated the experiment with the drug, giving smaller quantities administered more frequently.

To a rabbit inoculated 4th December, 1886, 0·1 mgrm. curari was injected on the 4th day. On the 5th morning and evening, 0·15 mgrm., and subsequently 0·15 mgrm. till the 10th day, when the temperature had fallen to 38° C. and paresis was apparent, but confined to the fore limbs. Injection of the same quantity of curari—0·15 mgrm.—which hitherto had been without any appreciable effect on the animal, now greatly depressed it, within a few minutes of administration; the next day it was completely paralysed and died towards the middle of the 12th day.

In two control animals similarly inoculated the incubation period was in each 9 days; the one was then killed for another experiment, the other died on the 11th day. In this case the drug given more frequently, but in the same aggregate quantity daily, had, if any, but a very slight effect on the action of the virus in prolonging the incubation period or its fatal termination, and did not appear to warrant further experiments with it, the more especially as I found that even smaller quantities of curari than those I had given were dangerous, two rabbits of average size having been killed, the one by injections twice in the day of 0·13 mgrm., the other by a single injection of 0·100 mgrm.

I used the drug in 1 per cent. solution, freshly made by carefully triturating it with cold water only.

Salol, salicylate of phenol, is a drug recently introduced, which from its constitution should be a powerful germicide. Dissolved in olive oil, 1 part in 5, and injected subcutaneously, I found it was borne very well in moderate quantities by rabbits. I consequently treated a rabbit, inoculated with the virus of rabies, by giving it 0·2 grm. of salol twice daily during the incubation period, but as compared with a control animal I found no benefit resulting from its use. I tried it again in another case in much increased quantities, but with no better results.

I had thus tried divers agents, and the most powerful germicides with which I am acquainted, without the effects of infection being counteracted or modified, and could see no prospect of protection by their use. The other method proposed above to counteract or enable the animal to resist the result of infection, was by the administration of general tonics, or specific therapeutical agents.

I found that quinine in comparatively large doses (0·3 grm.) frequently repeated, had no appreciable tonic action, and in fact, was inert upon rabbits. Strychnia does seem so to act to some extent, in minute doses, which, however, must be continued for several days to produce any beneficial effect; its stimulating action upon the spinal cord, and its specific effect in spinal paralysis, is well established, and recommended it for the treatment of rabies in the rabbit, in which the stage of excitement is very slight and transient.

I found that cocaïne acts very markedly and quickly as a general tonic in the rabbit; an animal to which the hydrochlorate is given frequently, in quantities of from ½ grain to 1 grain or more, within a few days improves much in condition, with an increase of several per cent. of body weight, and an apparently increased appetite; even the smaller quantity, however, sometimes, but uncertainly, produced temporary excitement and general hyperæsthesia.

After preliminary trials I gave a rabbit, on the 4th day after infection, 5 minims of a 10 per cent. aqueous solution of cocaïne hydrochlorate, equal to about 0·04 grm. of the salt, and subsequently the same quantity morning and evening; between the 9th and the 10th days symptoms of infection appeared, and the animal was found dead on the 11th day, the length of the incubation period and the time of death being precisely the same as in two control animals inoculated at the same time.

To another rabbit similarly inoculated, I also gave on the 4th day about the same quantity (0·04 grm.) of this salt, repeating it subsequently twice daily till the 10th day, when in the control animals similarly infected the first symptoms had appeared; the cocaïne was then alternated with 0·2 mgrm. of strychnia bisulphate, but without effect, and the animal died at the same time as its companion. Another case in which I gave strychnia for a longer period was as follows: a rabbit inoculated intracranially from the medulla of a rabid dog, 9th November, 1887, received daily from the 7th day, ½ to 3 grammes chloral hydrate; on the 9th day 0·1 mgrm. quinine bisulphate, and from the following day, twice daily, 0·05 to 0·075 mgrm. strychnia bisulphate. The access of the first symptoms was not well marked either in this animal or in two others similarly inoculated, but the former died on the morning of the 18th day, and both the others on the 21st, so that here again there was no benefit from the action of the drug, but apparently the reverse.

Allyl alcohol has been suggested as a powerful germicide; I therefore tried its action upon rabbits, but I found it so rapidly and fatally toxical, even in the most minute quantities, that no benefit could be expected from its action.

Urethan (carbamate of ethyl) has been recommended for its action on the spinal cord; I therefore tried it, giving it subsequently to infection, but the result was equally negative.

Rabbits are singularly tolerant of atropine, even 1 gramme of the sulphate given subcutaneously often having no apparent action upon them. It could not therefore be expected to modify the symptoms. Moreover, Youatt* had tried the effect of belladonna extensively upon dogs infected with rabies, and though at first he had hopes of its efficacy, these were disappointed, and he ultimately found it useless.

I have also tried the action of arsenic upon rabbits. In the dog, given as arsenite of potash, it is a well-known and active tonic alterative. In man, too, and the horse it is used in some countries, with the result of increasing strength and endurance. In the rabbit, however, I could perceive no beneficial result from its administration, though the animal is very tolerant of it, and it takes large quantities proportionately to its weight without showing any symptoms of disturbance; I have not consequently tried its effect upon the virus of rabies.

In order to ascertain conclusively whether the bichloride of mercury, chloral, benzoate of soda or iodine had any toxical or inhibitory action upon the virus itself, though not modifying the symptoms it produces, other rabbits were inoculated intracranially from the medullas of the animals that had been subjected to their influence; in every instance they died infected, without any modification of the symptoms or the length of the incubation period, showing that these drugs had no action at all upon the virus.

Thus germicides, the most active tonics that I could find for the animal experimented upon, together with drugs acting specifically upon the spinal cord, were one and all inert materially to inhibit or modify the result of infection; but though none may be found that can do so in the rabbit, this, however, may not apply to other species very differently constituted, and it appears to me that of the many asserted cases of cure or recovery from this disease both in man and the dog, many of which rest apparently upon the best authority, some at least are authentic.

To take one such instance in man, the case of Offenberg which he treated by curari (reported in the 'Med. Times and Gazette,' 6th

* 'On Canine Madness,' by William Youatt, M.R.C.V.S., London, 1830.

October, 1877), where a country girl, 21 years of age, bitten by a dog suspected of rabies 28th July, 1874, admitted into the hospital at Wickrath in Rhenish Prussia, on the 80th day developed symptoms of hydrophobia, spasms in attempting to drink, followed by the usual course of symptoms. She was excited by light, with hyperæsthesia of the senses of smell and touch. Morphia and chloroform were without effect; she was then treated with frequent subcutaneous injections of curari, to the point of commencing general paralysis of the voluntary muscles; after being for two hours under the influence of the drug the symptoms of hydrophobia gradually disappeared and the patient ultimately recovered.

It is not probable that in this case the symptoms were merely simulative or hysterical (lyssophobic). The photophobia and hyperæsthesia of the sense of smell and touch do not favour that view; the patient, a peasant girl, was very unlikely to have heard of the occurrence of these symptoms, or to have been apprehensive of them.

This is one case out of several in which it does not seem to me that there is reason to doubt the fact of recovery, though it may well be that a method of treatment successful in one case would fail in another, or very possibly even aggravate the symptoms, owing to their great diversity.

With regard to dogs, the records of cure or recovery are very numerous. To take one instance;* rabies having broken out in a pack of hounds, Dr. James, relying on the action of mercury, treated two hounds which had both developed symptoms of infection, with turpeth mineral (the yellow subsulphate of mercury). The one recovered, the other died. It was also, he states, successfully employed in other cases, both in man and dogs.

Here it was improbable that the symptoms and nature of the outbreak could have been mistaken; misrepresentation, too, is precluded by the fact that in a pack of hounds all the circumstances affecting them would be perfectly well known.

The statements of M. Pasteur, too, which in a matter of fact may be implicitly relied upon, appear to me conclusive upon this point. He states distinctly ('Comptes Rendus,' vol. 95, p. 1187) that he has seen *some* cases of "spontaneous" recovery in dogs, after the first symptoms have appeared,† though never after the severe symptoms, and (*loc. cit.*, 25th February, 1884) that recovery is *frequent* in fowls.

From this it appears to me that this disease is not necessarily incurable in man and the dog, though the symptoms are so different

* 'Phil. Trans.,' vol. 39 (No. 441, 1736), p. 244.

† He adds that he has *also* seen cases of partial recovery and *subsequent relapse* after some months, followed by death.

in different cases that it may well be that treatment which in one is successful would fail in, or even aggravate another; and it seems to me very desirable that the effect of various therapeutical agents upon the dog should be investigated by those who have the opportunity and inducement to do so, though with this animal it obviously requires special methods, appliances, and precautions.

I cannot conclude this portion of the subject without expressing my strong opinion that for us in our insular position, remedial measures ought to be entirely unnecessary; to stamp out rabies and hydrophobia throughout England nothing more is required than an order by the Privy Council, rigorously enforced, for the muzzling of all dogs throughout the country for a sufficient period. Of the efficacy of this there can be no doubt.

In the Metropolitan district we see its effects in the disappearance from the streets of rabies, and of the cases of hydrophobia from the hospitals, lately so prevalent and calamitous; unfortunately, however, this can only be temporary, as, under existing conditions, the disease will, sooner or later, be again introduced from other parts where these regulations have not been enforced.*

X. *Nature of the Virus.*

Though nothing can be said to be positively known of the intimate nature of the virus of rabies, it has been considered by many observers† that it must be a micro-organism. Its evident powers of multiplication and reproduction, with the extreme length of its incubation period, alone go far to prove this. It is impossible to conceive that a soluble or chemical poison, or ferment, should remain latent and unaltered in the animal body for so long a period, then at once becoming active should multiply itself throughout the tissues, rendering them infective to other animals in the most minute quantities.

In the supposed discoveries of a specific microbe in the saliva of rabid animals, it has merely been one of the many saprophytes always present therein, but which was not familiar to the observer; and it is probable, from the uncertain result of inoculation with this secretion, that the virus is present in it in very small quantities, and consequently, though particulate, would be exceedingly difficult of observation with the microscope.

* P.S.—Already even—7/5/87—since the above was written this apprehension is realised; the police reports for April, just issued, showing a recrudescence of street rabies in the district.

† As by Hallier, 'Zeitschr. f. Parasitenkunde,' vol. 1, p. 301; and by Klebs, 'Aertzl. Correspondenzblatt,' No. 11, 1874; abstract in 'Archives Gén. de Méd.,' vol. 20, 1872, p. 352, &c.

In the asserted discoveries of a microbe in the tissues of the cerebro-spinal system, since the publication of M. Pasteur's statement that this is the seat of the virus, in some instances these have obviously resulted from mistaking the morphological elements of these tissues for micro-organisms. In the case of the statements of M. Fol,* that he has found a microbe in the ganglion cells, and within Schwann's sheath of the nerve fibres, of the encephalon and spinal cord, though these have received the qualified support of M. Pasteur, I have no doubt that the appearances which he describes as microbes are due to alterations in the cells and nerve fibres, produced by the strange modifications of the method of Weigert which he has adopted for preservation and staining. The appearances of stained granules which he describes, can always be produced by methods similar to those he has employed. I may add, that in very numerous experiments, by inoculating from infective medulla the material in which he asserted that he cultivated the microbe, viz., infusion of sheep's brain, I have never obtained the development of any form of vegetation whatever.

I have myself, as previously stated ('Lancet,' 1886, vol. 1, p. 1112), found a micrococcus in the cerebro-spinal tissues in some cases of rabies. It occurs chiefly in and around the central canal of the medulla spinalis and oblongata, and in the perivascular and pericellular lymph channels, but it is exceedingly difficult to stain, and I have not discovered any reagent by which this can be done with certainty, for I found in sections in which it was undoubtedly present —from their being portions contiguous to others in which it was demonstrated in vast numbers—that it was impossible to recognise it by any means whatever, with the best microscopical appliances, and though mounted in media of widely different refractive indices.

I have not been able to cultivate it constantly, but I did obtain some growths in agar-agar bouillon peptone, though never in any fluid medium, and from the second series of cultivations of these, with a minute portion of its scanty development, I inoculated one rabbit subcutaneously. The animal was unaffected for three months. It was then re-inoculated intracranially with a portion of medulla of intensified virulence; here also it remained unaffected for upwards of two months, when, being again inoculated, it died on the second or third day from accidental causes.

This, which up to that time was the only case I had had of the failure of infection after intracranial inoculation in upwards of sixty cases, could not have been merely accidental, and was presumably due to a protective or inhibitory action of the cultivation; but, as I have not been able to demonstrate the presence of or cultivate the microbe constantly, a final conclusion upon its functions must await further observations.

* 'Archives Sci. Phys. Nat.,' vol. 10, 1886, p. 327.

XI. *Conclusions.*

By these experiments it is shown:—

(1.) That the virus of rabies in the lower animals, and of hydrophobia in man, resides principally in the cerebro-spinal substance and in the peripheral nerves, as well as in the salivary glands, in accordance with the fundamental statement of M. Pasteur.

(2.) That inoculation of this substance upon the brain of an animal, by trephining, produces infective rabies in rabbits almost infallibly, and with a much shorter and less variable incubation period than after subcutaneous inoculation.

(3.) That in an infected animal, the tissues do not become virulent till towards the close of the incubation period.

(4.) That rabies, however produced, in both dogs and rabbits, is essentially a paralytic affection, the same disease in both animals, and that there is no constant distinction between the so-termed dumb and furious rabies in the dog.

(5.) That the activity of the virus of street rabies generally is increased, and becomes remarkably constant, by passing through a series of rabbits.

(6.) That the activity of the virus is shown by the duration of the incubation period, to which it is inversely proportionate, and that this circumstance may afford a means of determining the source of infection in case of death from rabies or hydrophobia.

(7.) That of numerous drugs of different classes tried on the rabbit, none have any constant effect upon the result of infection.

(8.) That by subcutaneous inoculations with modified virus, as practised by M. Pasteur, it is not practicable to confer immunity, even against subsequent infection, upon rabbits; and that with these animals the intensive or rapid method of inoculation is very liable itself to produce infection; that the constitutional refractoriness of the dog to infection with rabies by any method of inoculation, renders it extremely difficult to judge of the results of remedial or prophylactic measures with this animal, from a limited number of experiments; and that it is by the statistics of the treatment that the results in man must be judged.

Finally, I must state that my experiments were not undertaken with primary reference to M. Pasteur's statements, but that the fundamental importance of these so greatly modified and subverted previous views upon this disease, that it necessitated my investigating them, with the result of confirming the conclusions of their author in many essential points; and that it is to his notable discovery of the chief seat of the virus, with the constant and rapid effects, in the rabbit, of the methods of inoculation which he has introduced, that we are indebted for the means of investigating with ease and certainty the phenomena of this disease, which previously had been most difficult and inconclusive.

These experiments were performed at the Brown Institution, and I must express my hearty thanks to Professor Horsley, F.R.S., for the facilities and assistance he has so kindly afforded me, in this and other investigations.

A considerable portion of the cost of material for this investigation was defrayed by a grant from the Association for the Advancement of Medicine by Research.

EXPLANATION OF PLATE.

Fig. 1. Encephalon of rabid rabbit, intensely and unusually congested, the dura mater removed. The site of inoculation is perceptible at *x*, by slightly increased congestion.

Fig. 2. Tongue, larynx, and part of trachea, of the same rabbit, showing deep congestion.

Fig. 3. Stomach of a similar rabbit, showing the veins of the serous coats much distended, together with numerous and moderately large hæmorrhagic spots, distinctly marked in a typical manner, as described in text.

"A Further Minute Analysis, by Electric Stimulation, of the so-called Motor Region of the Cortex Cerebri in the Monkey (*Macacus sinicus*)." By Charles E. Beevor, M.D., M.R.C.P., and Victor Horsley, B.S., F.R.C.S., F.R.S. Received June 16, 1887.* (From the Laboratory of the Brown Institution.)

(Abstract.)

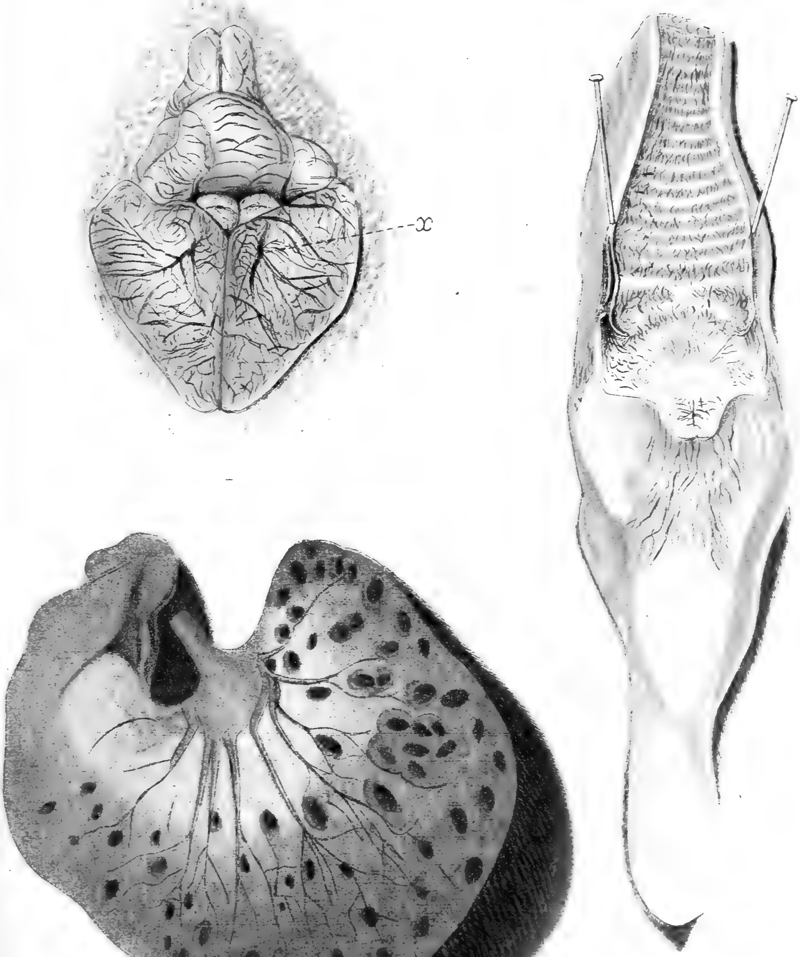
The present research, of which the following is a brief abstract, is in continuation of an investigation which we commenced two years ago, the first part of which is about to be published in the 'Philosophical Transactions.'

In our former paper we described the results of a minute analysis, obtained by electrical excitation, of that part of the cortex in which Professor Ferrier had previously shown that the movements of the upper limb were chiefly represented.

In the present paper the same mode of analysis has been employed for the investigation of the parts of the cortex grouped around the before-mentioned area.

Mode of Excitation.—The mode of excitation was the same, with a slight alteration, as that which we previously adopted.

* Received and read June 16th in abstract only. Full paper received August 12, 1887.


x

Mode of Subdivision of the Cortical Surface.—As before, we have again arbitrarily divided the cortical surface into minute areas 2 mm. square, and thus 73 centres were formed and subjected to excitation. Altogether 23 experiments have been made, the animals being invariably anæsthetised with ether.

Anatomy.—The region explored comprised the gyrus coursing in front of the whole length of the precentral sulcus; the posterior third of the middle frontal convolution; the posterior half of the superior frontal convolution; the upper end of the ascending frontal convolution, and the whole of the ascending parietal, except the lower half of its anterior border.

Topography of Representation.

The parts of the body which are represented in the region thus defined are as follows, viz.:—

(*a.*) The head and eyes.

(*b.*) The lower limb.

(*c.*) The upper limb.

(*a.*) *Head and Eyes.*—The representation of the important movement of turning the head and eyes to the opposite side is situated in a broad zone extending up along the whole length of the precentral sulcus and over the posterior half of the middle and superior frontal convolutions respectively as far as the margin of the hemisphere.

(*b.*) *Lower Limb.*—The movements of the lower limb are represented in the posterior fifth of the superior frontal, the upper third of the ascending frontal, and the upper third of the ascending parietal convolutions.

(*c.*) *Upper Limb.*—In our former paper the account of the representation of the upper limb was necessarily incomplete, owing to its fusion with that of neighbouring centres. This we have now accomplished, and the area for the movement of the upper limb may consequently be defined as being centralised in the middle of the ascending frontal convolution, from which point it reaches into the middle frontal. Upwards it extends slightly into the superior frontal convolution and backwards over the lower two-thirds of the ascending parietal convolution as far as the intra-parietal sulcus.

General Conclusions.

By exploring the above-mentioned areas with minimal stimulation (see previous paper) we have ascertained—

(1.) The *Primary Movement.*

(2.) The *March*, *i.e.*, the sequence of movements following the primary movement.

(3.) The *Character of the Movements.*

These facts have been ascertained for each of the 73 centres examined. It is obviously impossible here to indicate even the general conclusions thus arrived at, owing to the large number of separate observations which cannot be briefly collated. Reference must therefore be directed to the original paper.

The expenses of the research were defrayed by a grant from the British Medical Association.

"The Influence of Stress and Strain on the Physical Properties of Matter. Part I. Elasticity—*continued.* The Velocity of Sound in Metals and a Comparison of their Moduli of Longitudinal and Torsional Elasticities as determined by Statical and Kinetical Methods." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received April 29,—Read June 16, 1887.

[PLATE 2.]

We owe to Wertheim* a series of carefully executed experiments on the longitudinal elasticity of metals both by statical extension and by longitudinal and transverse vibrations. From these researches it would appear that the values of the moduli of longitudinal elasticity as determined for several metals by the first of these three methods are, as might be expected, less than those obtained by the other two. The differences, however, are very much greater than can be accounted for by the heating and cooling effects of contraction and elongation, and the author has already pointed out what he believes to have been in a great measure the cause of these discrepancies.† As a few observations made with two or three different metals had seemed to him to show the possibility of obtaining more concordant results, he was encouraged to extend his investigations to other metals, and moreover to institute a comparison between the values of torsional elasticity which could be obtained by statical and kinetical methods. It had originally been the author's intention to use the same specimens of the various metals as were employed in his previous experiments on moduli of elasticity and electrical conductivity,‡ but on applying to Messrs. Johnson, Matthey and Co. to have these specimens fused and redrawn, he was informed that what was desired would be almost if not quite impossible, inasmuch as several of the metals if fused in small quantities would be rendered too brittle for the

* 'Annales de Chimie,' vol. 12, 1844.

† 'Phil. Trans.,' vol. 174, 1883 (Part 1), p. 14.

‡ *Loc. cit.*

process of wire drawing. Accordingly by request, Messrs. Johnson and Matthey with their usual courtesy prepared specimens of platinum, silver, copper, aluminium, lead, platinum-silver and German silver in the same manner and of the same degree of purity as before. The wire-drawers had received special instructions to avoid kinks and to secure uniformity in the diameters of the wires throughout their lengths. Experiment I, which may be taken as representative of the degree of uniformity obtained in the diameter of the various wires, shows that in this last respect the instructions had been well carried out; nor could any kinks be detected in the wires. The results given in Experiment I were obtained by means of a gauge reading to $\frac{1}{100}$th of a millimetre; by estimation it was easy to measure to $\frac{1}{1000}$th of a millimetre.

Experiment I.

Distance in feet from one end of the wire at which the gauge was applied.	Gauge-reading in centimetres.
1·5	0·1088
3·0	0·1098
4·5	0·1087
6·0	0·1091
7·5	0·1092
9·0	0·1089
10·5	0·1091
12·0	0·1088
13·5	0·1093
15·0	0·1096
16·5	0·1089
18·0	0·1091
19·5	0·1091
21·0	0·1091
22·5	0·1092
24·0	0·1092
25·5	0·1088
27·0	0·1092
28·5	0·1093
30·0	0·1093

The mean value of the gauge-readings is 0·10918, and in no case does a gauge-reading differ from this mean by more than $\frac{1}{2}$ per cent. After the diameter had been determined for each of the wires in a similar manner by means of the gauge, they were made into coils of more than one foot diameter, and the diameter again determined from the apparent loss of mass in water at 4° C. and from the length. The values for the diameters obtained by the last method agreed very closely with those got by means of the gauge.

The Longitudinal Elasticity as determined by the Method of Statical Extension.

The mode of experimenting by statical extension has already been described,* and the precautions which were used then were used now; but in these fresh trials the author availed himself of a device whereby the departure from "Hooke's law," which had been formerly observed more or less with all the wires, can be done away with. This device the author owes to a perusal of the investigations of Professor G. Wiedemann on statical torsion.† Wiedemann has proved that though on first applying the loads used for twisting the wire the torsional strain increases in greater proportion than the stress, the frequent repetition of these loads gradually diminishes this want of proportionality. He has further shown that the process may be much facilitated by repeatedly putting the wire into torsional oscillations whilst under the influence of the torsional stress. In a similar manner the author now found that if the wire when under the influence of a load causing longitudinal stress were set oscillating longitudinally, by alternately pressing with the hand on the scale-pan and removing the pressure, the range for which "Hooke's law" held good was sensibly increased,‡ so that he was able to use larger loads than could otherwise have been used without passing beyond the boundaries of perfect elasticity. The following experiment will serve to show the degree of accuracy attainable.

* 'Phil. Trans.,' vol. 174, 1883 (Part I), pp. 3, 4.

† 'Phil. Mag.,' Jan. and Feb., 1880.

‡ It should be mentioned here that in all cases it is advisable to allow a rest after oscillating the wire in this way for the first time, and afterwards to oscillate it again just before testing.

Experiment II.—Platinum-silver.

Number of series of experiments.	Load on the wire in kilos., P.	Difference between consecutive loads, ΔP.	Scale-reading in half-millimetres, S.	Difference between consecutive scale-readings ΔS.	ΔS/ΔP.	Mean values of ΔS/ΔP in each series.
I	10	..	21·78	..	..	1·1958
	16	6	14·57	7·21	1·2015	
	12	4	19·35	4·78	1·1950	
	16	4	14·53	4·77	1·1925	
	14	2	16·93	2·35	1·1750	
	12	2	19·36	2·43	1·2150	
II	10	..	21·64	..	..	1·2017
	16	6	14·40	7·24	1·2067	
	10	6	21·63	7·23	1·2050	
	*12	..	19·19	..	..	
	14	2	16·77	2·42	1·2100	
	16	2	14·40	2·37	1·1850	
III	*10	..	19·63	..	..	1·1950
	14	4	14·86	4·77	1·1925	
	18	4	10·07	4·79	1·1975	
	10	8	19·63	9·56	1·1950	
	14	4	14·78	4·85	1·2125	
	18	4	10·06	4·72	1·1800	
	10	8	19·60	9·54	1·1925	
IV	10	..	19·61	..	..	1·1950
	14	4	14·79	4·82	1·2050	
	*14	..	11·49	..	..	
	18	4	6·72	4·77	1·1925	
	14	4	11·45	4·73	1·1825	
	*18	..	16·86	..	..	
	14	4	21·59	4·73	1·1825	
	10	4	26·44	4·85	1·2125	

* Signifies that the scale was shifted, either by slightly increasing or diminishing the permanent load as in the first two cases, or by unclamping the scale and vernier and entirely readjusting, but so that the same length of wire might be under examination as before.

The mean value of $\Delta S/\Delta P$ for the four series is 1·1969 and the probable error is 0·10 per cent. The values of $\Delta S/\Delta P$ obtained with the temporary stresses of 2 kilos., 4 kilos., 6 kilos., and 8 kilos. are, within the limits of errors of observation, equal, and moreover are independent of the permanent load on the wire.

The results recorded above were not got without the exercise of very great precaution both in adjusting the scale and vernier, and in preventing either the wire under examination or the comparison-wire from twisting after the adjustment had been completed. Should such twisting take place the vernier will not move with sufficient freedom up and down the scale.* Indeed in this experiment there are here and there slight traces of the vernier sticking in the scale, and in most of the other experiments it was considered advisable to unclamp both scale and vernier and completely readjust them, so that slightly different lengths of the wire might be under examination.† The next experiment furnishes an example of this mode of treatment.

Experiment III.—Piano-steel.

Temporary load, 8 kilos.; permanent load, 10 kilos.

Length of the wire under examination in centimetres, l.	Temporary alteration of length in centimetres, ΔS.	$\Delta S/l$.
753·8	0·5245	0·0006955
750·8	0·5195	0·0006920
754·0	0·5233	0·0006940

The mean of the numbers in the third column is 0·0006938, with a probable error of 0·09 per cent. It is evident therefore that with care considerable accuracy can be obtained. The elasticity of piano-steel within the limits of the loading here employed had been previously proved to be quite perfect, provided the precautions adopted in this and the other experiments were taken, so that it was considered unnecessary to try the effect of lesser loads, but with all the other metals at least two loads were used, and for these loads Hooke's law, "*ut extensio sic vis*," could by proper treatment of the wire be made to hold good within the limits of errors of observation.

* The author may mention here that he found it to be a great convenience to place four blocks of well-planed wood on the ground close to the four sides of the scale-pan, and to anchor the bar fastened to the comparison-wire by fine wires attached to the ends of the former and secured in a horizontal position.

† The different lengths were not measured until the series of temporary alterations of length produced by the various changes of load was completed, so as to avoid prejudice on the part of the observer.

The Longitudinal Elasticity as determined by the Method of Longitudinal Vibrations.

Preliminary Trials.

As it was found necessary to use the syren more or less throughout this part of the investigation, some experiments were made with a view of ascertaining how far this instrument could be relied on for determining the number of vibrations made in a given time. First, attention was directed to the registering apparatus, with the object of determining how far the putting of this in, or the taking of it out of action, would introduce error. Any error of the kind would be more manifest when the number of vibrations was determined for a short space of time than for a long one, and in the following experiment it will be seen that there is no appreciable error due to the inertia of the register.

Experiment IV.

The number of vibrations recorded in one minute by the syren in each of three trials, when the syren was kept in unison with a monochord, were:—

Number of trial.	Number of vibrations.
1	813 × 20
2	812 × 20
3	811 × 20
Mean.......	812 × 20

The number of vibrations recorded in five minutes was next ascertained by one trial to be 4064 × 20, which would give 812·8 × 20 per minute, or only 0·1 per cent. higher than before. The pitch of the monochord was then altered, and several fresh experiments similar to the above ended in showing that sometimes the number of vibrations per minute registered in the shorter-timed trials appeared to be greater, and sometimes less, than the number registered in the longer-timed trials, the difference being in all cases equally slight with the above.

Again, the syren was employed to determine the pitch of several of Koenig's forks; thus in a single trial, in each case of three minutes' duration, the instrument registered 256·7 and 513·8 vibrations per second respectively for two forks, which were marked with the numbers 256 and 512. Several other trials of similar accuracy, sometimes giving slightly greater, and sometimes slightly less vibration-frequencies than those marked on the forks, rendered it evident that it was possible to determine with great accuracy the vibration-number of a tuning-fork by means of the syren, when the notes of these two instruments are compared *directly* with one another.* The case, however, was different when a monochord was tuned to the note of a fork, and the former then compared with the syren. Thus the monochord was tuned to unison with a Koenig's fork marked 384, and afterwards the vibration-frequency of the former was determined by the syren. Two trials resulted in giving the number of vibrations per second as 387 and 386. In a similar manner, using a fork marked 512, six trials with the syren gave the vibration-frequency as follows :—515·8, 516·8, 514·3, 515·8, 516·8, 515·4, with a mean value of 515·8. The experiments with both forks therefore gave values for the vibration-frequencies which were about $\frac{3}{4}$ per cent. too high, and yet the monochord after these trials was in each case still in perfect unison with the fork with which it had been compared. As the assistant (Mr. Furse) seemed to think that the error arose from drawing the bow too strongly across the wire of the monochord, in the endeavour to make the sound of this instrument of sufficient intensity to be heard at the same time as that of the syren, a screen was placed so as partly to shield the sound of the latter from the manipulator of the monochord, and this plan proved to be successful, for now the syren recorded the same vibration-frequency for both monochord and tuning-fork, and several experiments of the same kind with forks of different pitch manifested that with the precaution mentioned above the syren could be made to determine the pitch of the monochord as accurately as the pitch of the tuning-fork.

Matters having been so far satisfactorily arranged, a considerable number of trials were made, for the purpose of deciding on the best mode of arranging the wire which it was desired to throw into longitudinal vibrations. In the first instance the wire to be examined was stretched horizontally, and clamped at one end to a block of iron secured to a window-sill. Towards its other extremity the wire passed over a fixed pulley, so that by placing weights on a scale-pan attached to this extremity any required degree of stress could be put upon the wire. Before going over the pulley the wire passed through the jaws of a strong vice, so that when stretched sufficiently it could be firmly clamped by means of the latter, and was protected from injury by placing pieces of hard wood between it and the jaws of the vice. Both the vice and pulley were firmly clamped to a very stout table. When fixed for examination the wire was thrown into longi-

* The author has again to thank Mr. Furse, the curator of the Museum of King George III, at King's College, for his assistance; as also Mr. H. A. Reatchlous, one of the students of the Physical Laboratory. Both these assistants are musicians, and the former is especially remarkable for his skill in manipulating the syren.

tudinal vibrations by rubbing it as lightly as possible with a resined glove; a monochord was tuned in unison with the wire, and the pitch of the former then determined by means of the syren. In this way copper, platinum, silver, and platinum-silver were examined, each with two different lengths, one length being about half that of the other. In all cases the shorter length gave a greater number of vibrations in proportion to the inverse of the length than the longer one. The average extent of the deviation is exhibited in Experiment V.

Experiment V.—Platinum Wire.

Length of the wire examined in centimetres, l.	Number of vibrations per second, n.	$l \times n$.
967·4	146·47	141,700
495·7	186·90	142,230

Here in the case of the shorter length the product $l \times n$ is about 0·4 per cent. higher than the same product for the longer length. As much greater accuracy than this was to be aimed at for the purpose in view, several days were spent in endeavouring to ascertain what flaws there might be in this mode of experimenting. In the first place the permanent loads placed on the scale-pan were gradually increased in amount, and as the wires were hard drawn, this could be done to a considerable extent without causing any sensible permanent elongation. Evidently, however, the source of error was not to be detected in this way, for though a slight change in the pitch of the note could be detected when the note was reduced below a certain comparatively small amount, yet after this amount of stress had been exceeded, no further addition of load seemed to produce any appreciable effect. Secondly, the clamp and vice were shifted and more firmly secured, but still with no good result.

After this it was decided to place the wire vertically, and as the room was very lofty a considerable length could be tested in this new position. The rest of the arrangement was the same as before, except that now the pulley was dispensed with and the wire was clamped at its upper extremity to a very massive iron plate, and hung freely down through the jaws of the vice before clamping with the latter. The following table gives the mean values of the products $l \times n$ for the different wires examined in vertical and horizontal positions, where l and n have the same signification as before :—

Table I.

Metal.	$l \times n$, position vertical.	$l \times n$, position horizontal.
Copper	192,650	195,567
Platinum	136,410	141,700
Silver	136,410	140,067
Platinum-silver	140,500	142,200
Aluminium	243,900	
Piano-steel	261,700	

The results shown in this table are far from being satisfactory, and the numbers in the third column differ from those in the second column by amounts which are in all cases considerably larger than the differences in the values of $l \times n$, as determined for different lengths of the same metal, either in the vertical or the horizontal position.

Final Method of Experimenting.

Though the notes obtained by rubbing the wires longitudinally were fairly clear and well defined when the wire was arranged according to either of the above methods, yet the divergence of the results given above was such as to induce the author to try a third method, which, when certain corrections have been applied for want of rigidity of the masses to which the ends of the wire are clamped, seems to be capable of considerable accuracy. In fig. 1, Plate 2, AB is a hollow box,* made of wooden planks half an inch thick. The length of the box is 600 cm., the breadth 10 cm., and the depth 10 cm. At one end of the box is a pulley C, round which the wire passes to the scale-pan S. D and E are two pairs of stout blocks of wood, each of which can be firmly clamped to any part of the box by a pair of very stout wooden screws.† The stout blocks of wood carry each an iron clamp, by which the wire can be secured, and the blocks of wood together with the screws are all well insulated from the box by means of thick layers of baize M, so that the vibrations of the wire cannot be imparted to the box.‡ The wire is first clamped to the wooden

* There is no advantage in having a *hollow* box; a solid piece of wood or metal of sufficient stoutness would answer the purpose equally well.

† Only one of each pair of screws is shown in the figure.

‡ This the author found to be a matter of some considerable importance, as the note was very much clearer and purer when the blocks were insulated from the box than when they were not. A rather curious case of synchronism occurred in one of the earlier experiments where the blocks were not insulated from the box. An iron wire had been arranged to give, as far as could be judged by the note, 512 vibrations per second. The note was, however, very far from clear; but by shortening or

block at D, and is then passed over the pulley at C, so as to be stretched to any required extent by placing weights on the scale-pan S. When the required stress is attained the wire is clamped at E, and the part to the right of E having been detached from the pulley and scale-pan, is drawn on one side and rested on some non-conductor of sound, such as baize or flannel. In several cases, before the last-mentioned adjustment had been completed, the blocks of wood at E were shifted backwards or forwards, until the note given out by the longitudinally rubbed wire was in unison with a Koenig's tuning-fork, but in others the pitch of the note was determined with the syren. If the clamps at D and E had been secured to perfectly rigid supports, the number of vibrations obtained when the wire was clipped in the centre would have been exactly double the number when the wire was free, except at both ends, but in consequence of lack of rigidity of the supports at D and E, the note given out in the former case had *less* than double the number of vibrations of the note in the latter. Now Lord Rayleigh has proved for transverse vibrations* that when, as in the present instance, the mass at each end is large compared with the force of the spring which urges the extremity attached to the mass towards the position of equilibrium, any slight yielding of the supports will cause a *rise* in pitch, and will produce the same effect as if the wire had been shortened in the ratio of $1 : 1-k/n^2$, where k is a constant, if we experiment with the same length of the same wire under the same conditions as regards the nature of the supports, and n is the tone of the wire. Lord Rayleigh's mathematical reasoning can be equally applied to *longitudinal* vibrations, and it is obvious that by obtaining the number of vibrations of the wire when free, except at both ends, and then when clipped in the centre, we may determine the amount by which the yielding of the supports heightens the pitch of the note. For the sake of greater accuracy the number of vibrations yielded when the wire was clipped one-third of its whole length from one end was in some cases also ascertained. The next experiments will sufficiently illustrate the mode of proceeding.

lengthening the wire slightly it was very much improved, though of course of a different pitch. The very marked want of clearness was presently found to arise from synchronism between the time taken by a pulse to pass from one block to the other *through the wood* and the time taken to pass from end to end of the wire and back again. When the blocks were insulated from the box the want of clearness vanished and the pitch of the note rose 6 or 7 per cent.

* 'Theory of Sound,' vol. 1, § 135.

Experiment VI.—Platinum-silver Wire.

Number of vibrations per second.*	Remarks.
259·7 × 1	Wire free except at both ends.
254·7 × 2	Wire clipped in the centre.
253·9 × 3	Wire clipped at a point one-third of the whole length from one end.

Let k be the equivalent shortening of the wire when it is free except at the two ends; then since the velocity of sound along the wire will be the same in all three cases we must have:—

$$\left(1-\frac{k}{1^2}\right) 259{\cdot}7 = \left(1-\frac{k}{2^2}\right) 254{\cdot}7; \quad . \quad . \quad . \quad . \quad (1)$$

and

$$\left(1-\frac{k}{1^2}\right) 259{\cdot}7 = \left(1-\frac{k}{3^2}\right) 253{\cdot}9; \quad . \quad . \quad . \quad . \quad (2)$$

From (1) we obtain $k = 0{\cdot}0255$,

and from (2) $k = 0{\cdot}0250$.

The mean of these two values of k is 0·02525, and since the length of the wire examined was 553·85 cm., the velocity of sound in centimetres per second obtained from the three sets of trials given in Experiment VI will be 280,440, 280,380, and 280,500 respectively, with a mean of 280,440 and a probable error of only 0·008 per cent.

Experiment VII.—Silver Wire.

Length, 553·85 cm.

Number of vibrations per second.	Remarks.
253·8 × 1	Wire free except at both ends.
253·1 × 2	Wire clipped in the centre.
253·0 × 3	Wire clipped at a point one-third of the whole length from one end.

Adopting the same mode of procedure as before we obtain for k the two values 0·00366 and 0·00354 with a mean of 0·00360. Thus the

* The numbers given under this heading are in this and the next experiments the mean values resulting from several closely accordant trials.

velocity of sound in centimetres per second as deduced from the numbers in the first column will be, within 0·001 per cent., in all three series of trials the same, namely, 280,100. Experiments VI and VII furnish results which are rather more consistent with each other and attended with a slightly less probable error, as judged by the departure of each individual value of the velocity of sound from the mean value, but still the agreement seemed to be very good in the case of the other metals. Thus with annealed iron wire of the same length as the silver wire, and which was tested not only for the fundamental note and the first and second octaves but also for the third octave, the following values of the velocity of sound were deduced:—

Velocity of sound in centimetres per second.	Note.
509,700	Fundamental.
510,000	First octave.
510,400	Second octave.
508,400	Third octave.
509,600 mean.	

Again a hard drawn copper wire of the same length gave the following results:—

Velocity of sound in centimetres per second.	Note.
395,600	Fundamental.
395,400	First octave.
396,400	Second octave.
395,800 mean.	

The velocity of sound for the other wires given in the next table was determined only from observation of the fundamental note and the first octave. All the metals, except tho piano-steel, the annealed iron and the German silver, were obtained from Messrs. Johnson, Matthey and Co., and were stated to be chemically pure.

Table II.

Metal.	Condition.	Density.	Velocity of sound in metres per second.
Piano-steel	Unannealed	7·7475	5198
Iron................	Annealed	7·6831	5096
Copper	Unannealed	8·8976	3958
German silver	,,	8·6320	3860
Platinum-silver	,,	12·1900	2804
Silver...............	,,	10·4668	2801
Platinum...........	,,	21·0500	2750

In the next table will be found a comparison between the moduli of longitudinal elasticity as obtained by the statical and kinetical methods for steel, copper, platinum, platinum-silver, and silver. All the results given in the table were obtained with as much care as those already quoted.

Table III.

Metal.	Young's modulus in grams per square centimetre as obtained by the kinetical method. e_k.	Ditto as obtained by the statical method. e_s.	Ditto supposing no heat to be gained or lost during the testing. e'_s.	$\frac{e_k - e'_s}{e_k}$.
Piano-steel	2133×10^6	2140×10^6	2144×10^6	−0·0051
Copper	1316	1323	1326	−0·0076
Platinum	1622	1623	1625	−0·0018
Platinum-silver	997	1001	1004	−0·0070
Silver..............	835·6	828·6	831·1	+0·0054

It will be seen from Table II that the values of Young's modulus, as determined by the statical and kinetical methods, agree with each other within less than 1 per cent., and that on the whole the values obtained by the *former* of the two methods are slightly *greater* than those obtained by the *latter* method.*

* The author is inclined to attribute this to the fact that with hard-drawn metals loading always produces a slight amount of temporary twisting. This (see Experiment II) tends to produce a slight degree of sticking between the scale and vernier used in the statical method. With well annealed wires this temporary twisting does not result from loading.

The velocity of sound as calculated from the values of e'_s and the density would in no case differ from those obtained directly by so much as ½ per cent.

The Velocity of Sound is Independent of the Temporary Load.

Experiment VIII.

A well annealed copper wire about 1 mm. in diameter was weighted with 18 kilos. and left with this load on for some time. It was then tested with various loads up to 18 kilos. with the following results.

Load on the wire before clamping.	Number of longitudinal vibrations in three minutes.*
6 kilos.	4048·5 × 20
10 "	4056·3
14 "	4054·9
18 "	4057·0
Mean of last three....	4056·1

It is evident from the above that except for the lightest load, the velocity of sound is entirely independent of the temporary load, and even with the load of 6 kilos. there is not a departure from the mean value obtained with the other three loads of more than 0·2 per cent.†

Similarly with a pianoforte steel wire of 0·08 cm. diameter, not the slightest difference in the pitch of the note could be discerned with loads which varied from 16 kilos. up to 30 kilos.

The temporary alteration of density which was produced in either of these two experiments would be too small to cause of itself a perceptible alteration of pitch. Thus in the case of the piano-steel wire, the temporary change of density resulting from the increase of the load from 16 kilos. to 30 kilos. would only cause a change in the pitch of the note of 0·03 per cent.

The Effect of Permanent Extension on the Velocity of Sound.

The author has shown‡ that when an iron wire has recently suffered permanent extension the longitudinal elasticity as determined by the

* The numbers in this column are the means of several observations with each load.

† The note with the load of 6 kilos. was not quite so clear as that obtained with the other three loads.

‡ 'Phil. Trans.,' vol. 174 (Part I), 1883.

statical method is decidedly less than when rest has been allowed after the permanent extension. The effect of rest in increasing the elasticity appeared to be less the less the temporary load used in testing the elasticity, and it seemed of some interest to ascertain in the first place the effect of permanent extension on the velocity of sound, and in the second whether rest would appreciably alter the velocity. The following experiment was therefore tried.

Experiment IX.

A well-annealed iron wire was stretched sufficiently by a temporary load to give a clear note when rubbed longitudinally. The pitch of the note was then taken on a monochord, and again after the wire had suffered more and more permanent extension, the same temporary load being used throughout, and the wire being shortened to its original length after each permanent extension.

Percentage permanent increase of length.	Length in centimetres of the wire of the monochord when the latter gave the same note as the longitudinally rubbed wire.
0·00	29·34
1·82	29·34
4·00	29·34
6·36	29·34
9·10	29·34
13·64	29·34
17·00	29·34

It will be seen from the above that the pitch of the note remained, as far as could be judged, absolutely unaltered by the permanent extension. After one hour's rest, however, it seemed to be appreciably sharper, the frequency as determined by the syren being 458·5 as against 458·1, the frequency before stretching and before rest, though after recent permanent extension. It would thus appear that rest after permanent extension does very slightly increase the velocity of sound in the case of iron.

The permanent increase of velocity of sound can, however, be almost, if not entirely, accounted for by the diminution of density. The latter amounted altogether to 0·17 per cent., and this diminution would cause an increase in the velocity of sound of 0·085 per cent., whereas the actual increase observed was only 0·087 per cent. We must therefore conclude that the *elasticity* was not appreciably affected permanently by the permanent extension, though rest did apparently very slightly increase the elasticity after recent permanent extension.

Several other experiments of a like nature were made with annealed iron, but all seemed to show that the elasticity as tested by the method of longitudinal vibrations was not appreciably altered permanently by permanent extension.

Similar experiments to the above were made with annealed copper wire, and with similar results, except as regards the effect of rest, which in this case produced no change.

A Comparison of Moduli of Torsional Elasticity as determined by the Statical and Kinetical Methods.

Statical Method.

The wire to be tested, about 28 feet in length, was fastened at its upper extremity to a clamp secured to a stout iron bracket. The lower extremity of the wire was clamped at C, fig. 2, Plate 2, the extremity of a brass rod $\frac{1}{2}$ inch in diameter, and $1\frac{1}{2}$ feet in length; the rod passed vertically through the centre of a horizontal brass plate P, 8 inches in diameter, so that half of the rod was above and the other half below the plate; the lower half of the rod terminated in a hook to which was suspended a scale-pan, S, weighing 2 kilos. The torsion was effected by placing weights* in two scale-pans, T, made of cardboard, and weighing 10 grams each; the scale-pans, T, were fastened each to one extremity of a light silk thread, which passed over a pulley, W, and was wrapped a few times round the rod; the two threads were wrapped round the rod in opposite directions, so that when equal weights were placed in the scale-pans, T, a torsional couple was produced. The plate P was divided at its circumference into degrees, and by using a fine steel pointer (fig. 3), placed above the rim of the plate, and nearly but not quite touching it, it was possible to estimate to one-tenth of a degree. The stands carrying the pullies, W, were capable of being moved either vertically or horizontally in any direction, and great care was taken to ensure that the parts of the silk threads between the rod and the pullies were parallel to each other. In order to accomplish this last the adjustments were made in the first instance as nearly correct as the eye could judge; torsion was now imparted in the manner described, and after the plate had been twisted through about 360 degrees, the position of the end of the pointer, as regards its distance from the rim of the circular plate, was noted. If this distance was not the same as before the application of the torsion, one or both of the pulley stands were shifted in a horizontal plane until there was no perceptible difference in the distance

* These weights were of thin strips of German silver and were each made very accurately equal to 10 grams.

of the end of the pointer from the rim of the plate before and after torsion.

When the horizontal adjustment was satisfactorily completed, the silk threads could be made perpendicular to the axis of the rod by shifting the pulley stands up or down in a vertical plane, until any given load in each pan produced a maximum twisting effect. The pullies, W, were large and light, and so delicately balanced that the loss from friction was very slight. This loss from friction, small as it was, could be eliminated in the following manner:—Suppose that the torsional stress has twisted the plate through a certain number of degrees, the plate is now twisted very carefully by hand a little further, and this stress then very gradually relaxed; let D_1 be the present position of the pointer. Again, let the original torsional stress be carefully relaxed a little, and then very gradually restored, the pointer will now take up a new position, D_2. The true position of the pointer, if there were no friction, would be $D_1 + D_2/2$. At least ten trials were made with each of the torsional stresses employed, and the mean of the different readings, which accorded very well with each other, was taken.

In order to apply the torsional stress with sufficient gentleness, the following plan was adopted:—Two smooth blocks of wood were placed with a face of each in contact with two opposite sides of the scale-pan, S, so that neither the pan nor the plate, P, would move when weights were put in the smaller pans, T. As soon as the latter were loaded, the blocks of wood were gradually and gently removed from the sides, so as to permit of the torsional stress producing its effect by slow degrees.*

Prof. G. Wiedemann has already shown in his experiments on torsion by the statical method, that the torsional elasticity is independent of the amount of longitudinal stress which may be acting on the wire at the same time as the torsional stress,† and the author has also proved that this is the case when the kinetical method is adopted.‡ However, it was considered advisable to make a few preliminary experiments, to ascertain whether temporary loading would affect the torsional rigidity. It is unnecessary to enter into the results of these preliminary trials further than to say that they fully verified Prof. G. Wiedemann's previous observations for all the metals which were examined.

Before commencing the actual testing the wire was frequently set

* This plan was found to answer the purpose very well. Professor G. Wiedemann has adopted a much more elaborate arrangement for effecting the same object, but the author cannot help thinking that with care such a device as that mentioned above is quite sufficient to prevent the stress from being applied too suddenly.

† 'Wiedemann's Annalen,' vol. 6, 1879; 'Phil. Mag.,' Jan. and Feb., 1880.

‡ 'Phil. Trans.,' vol. 177 (Part II), 1886.

in torsional oscillation with a load in the pan, S, slightly greater than any which it was intended to use, and after each set of oscillations a long rest was allowed. The object of this preliminary treatment* was to extend the limit of elasticity as much as possible.

Again, immediately before the testing the wire was set in torsional oscillation, but not through a greater arc than that through which it was intended eventually to twist the wire.† Finally, the loads which were intended to be used in the pans, T, were put in and taken out ten or a dozen times, and then the actual trials began, the load in the pan, S, having been some time previously reduced to the amount to be used in the trial. The following experiment will give a fair notion of the degree of accuracy which was obtained.

Experiment X.

A hard-drawn aluminium wire, about 800 cm. long, and 0·1 cm. in diameter. The load on the wire was merely that of the scale-pan, *i.e.*, 2 kilos.

Load in each pan producing torsion.	Position of index.	Degrees of torsion produced by the torsional stress.
0 grms.	230·70	—
20 "	51·70	179·00
0 "	233·93	182·23
20 "	50·95	182·98
0 "	231·20	180·25
20 "	51·70	179·50
0 "	233·90	182 20
20 "	52·80	181·10
0 "	233·05	180·25
20 "	51·73	181·32
0 "	233·05	181·32
	Mean........	181·02

The probable error of the mean value 181·02, given above, is 0·14 per cent.

A set of observations was next made with 10 grams instead of 20 in the pans, T, the mean number of degrees of torsion being in this case 90·43, with a probable error of 0·36 per cent. Within the limits of probable error 90·43 is the half of 181·02. In calculating

* The period of this treatment depends upon the nature of the metal; with iron it is advisable that it should extend over a couple of days.

† The object of this will be seen from the author's paper on "The Internal Friction of Metals," 'Phil. Trans.,' vol. 177 (Part II), 1886.

this result it was assumed that the number of degrees of torsion produced with 10 grams in each of the pans, T, was $\frac{90{\cdot}43 + \frac{1}{2}(181{\cdot}02)}{2}$, or 90·47.

The value of the modulus of torsional elasticity in grams per square centimetre can be found from the formula—

$$r_s = \frac{L \times D \times P \times 360}{n \times S^2},$$

where L is the length of the wire in centimetres, S the section in square centimetres, P the number of grams in each pan, n the number of degrees of torsion, r_s the modulus of torsional elasticity determined by the statical method, and D is the arm of the couple P × D in centimetres. The value of D was determined very carefully by a wire gauge reading to $\frac{1}{100}$th of a millimetre, due allowance being made for the thickness of the silk thread, and proved to be 0·9668 cm. The value of L varied in the different experiments from 650 to 800 cm., and within the limits of errors of observations the strain, as in the above experiment, was exactly proportional to the stress. The diameter of each of the wires was very nearly 1 mm., and in only one instance* was the value of n carried beyond 200°. As far as could be ascertained the torsional stress never exceeded the limit of elasticity, the recovery being in all instances apparently perfect.†

As soon as the determination of the modulus of torsional elasticity by the statical method had been satisfactorily concluded, the modulus was redetermined by the method of torsional vibrations. The time of vibration was in the case of each wire taken from the mean of a large number of observations, first with only the graduated plate attached to the wire, and again when the moment of inertia of the plate had been supplemented with a hollow ring of copper, turned true inside and outside, and of which the moment of inertia could be calculated with considerable accuracy. The error likely to arise in the determination of the modulus of torsional elasticity by the kinetical method would not in any case be greater than 0·1 per cent. In the next table will be found the results obtained by both methods :—

* That of platinum in which the value of n was 210.

† This does not necessarily imply perfect elasticity, as for this the recovery of the wire on the removal of the stress should be *instantaneous*. Whether this was so could not, of course, be ascertained.

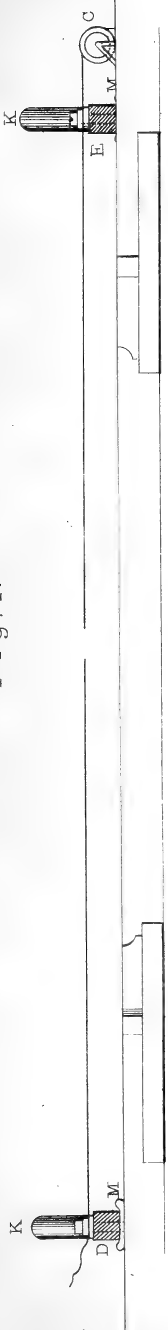


Fig. 1.

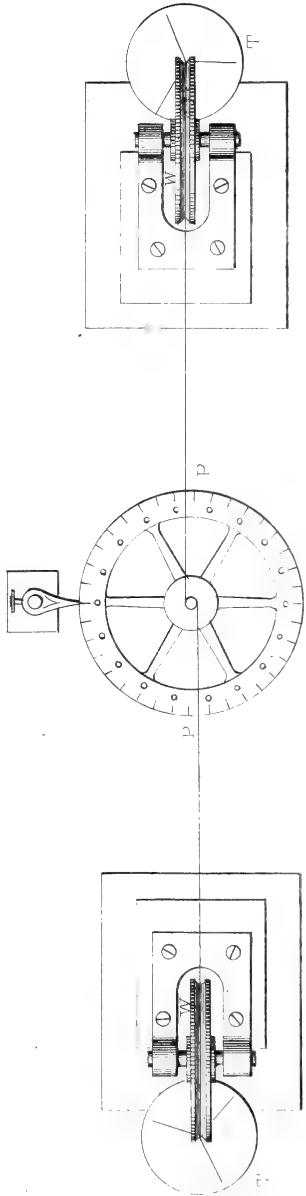
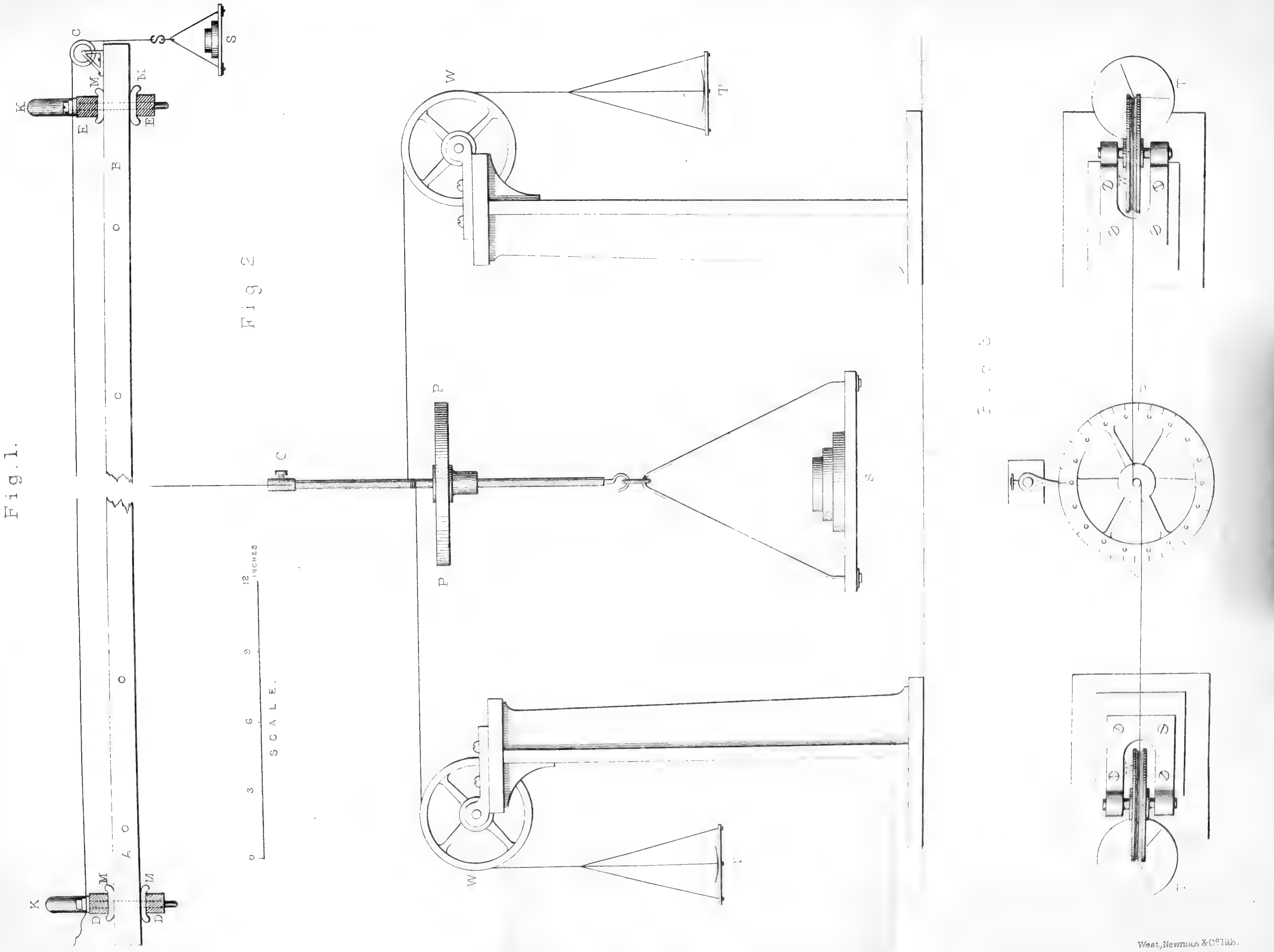


Fig. 3.

West, Newman & Co lith.



West, Newman & Co. lith.

Table IV.

Metal.	Condition.	Modulus of torsional elasticity in grams per square centimetre obtained by the statical method. r_s.	Ditto obtained by the kinetical method. r_k.	$\frac{r_s}{r_k}$.
Iron........	Annealed	751·5 × 10^6	766·5 × 10^6	1·020
Platinum ...	Unannealed	662·2	663·5	1·002
Silver.......	"	275·5	278·0	1·009
Aluminium..	"	267·7	266·9	0·997

The values of r_k given in Table IV were obtained from the formula—

$$r_k = \frac{2\text{LM}\pi^3}{981{\cdot}4t^2s^2},$$

where L is the length of the wire, t the time of vibration, s the section, M the moment of inertia, and 981·4 is the value of g at the place, the units being throughout C.G.S.

It will be seen that for the hard-drawn metals the values of r_s and r_k agree with each other within the limits of errors of observation, and that for these metals the mean value of r_s/r_k is 1·0043. It is impossible in this case to make an exact comparison of the values of r_s and r_k, when for the former allowance is made for the effects of loss and gain of heat, since the times of vibration in the kinetical method were too long* to avoid gain and loss of heat in using the method, but if the correction could be accurately applied, it would evidently on the whole bring stili greater accordance between the values of r_k and r_s.

For the annealed iron the value of r_s exceeds that of r_k by an amount which is greater than can be attributed either to heating and cooling effects or to errors of observation.

Summary.

1. The value of the modulus of longitudinal elasticity for hard-drawn metals, as determined by the statical method of loading, accords with the value obtained by the method of longitudinal vibrations, provided the deformations produced in using the former method are sufficiently small.

* The times of vibration varied from 6 to 9 seconds.

2. The velocity of sound in a wire is independent of the load on the wire.

3. The velocity of sound in a wire is not sensibly altered by permanent extensions of the wire, provided sufficient rest be allowed after the permanent extension has taken place.

4. The value of the modulus of torsional elasticity as determined by the statical method, accords with the value obtained by the method of torsional vibrations for most metals in the hard-drawn condition, provided the deformations produced are sufficiently small.

"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 J. H. GILBERT, LL.D., F.R.S. Preliminary Notice.* Received and read June 16, 1887.

For many years past the question of the sources of the nitrogen of our crops has been the subject 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 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 which thus indirectly serve 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 natural compensation must be found for the losses of combined nitrogen which the soil sustains by the removal of crops, and for those which result from the liberation of nitrogen from its combinations under various circumstances.

We propose 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

* This Preliminary Notice was originally intended to have served as the Abstract of a fuller paper, and is so referred to in the account of the meeting of June 16 (vol. 42, p. 483).

and conclusions of others which have recently been put forward, as above alluded to.

In our earlier papers we had concluded 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 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 comparable 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. In the experiments with leguminous crops the evidence was in favour of the supposition that, in some cases, the whole of the nitrogen had been taken up as nitric acid, whilst in others that source seemed to be inadequate.

It was further shown that, under otherwise parallel conditions, there was 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, at every depth of 9 inches down to a total depth of 108 inches, the Vicia soil contained much less nitric acid than the *Trifolium repens* soil; and it was concluded that much 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 experiments with *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. For example, at each of the twelve depths of the Medicago soil there remained very much less nitrogen as nitric acid than where very much less nitrogen had been removed in the *Trifolium repens* crops; there being on the average not one-twelfth as much in the lower ten depths of the Medicago soil as in the corresponding depths of the *Trifolium repens* soil. 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 is obvious that much nitrification takes place near the surface, but as the surface-soil became even somewhat richer in nitrogen, it was clear that the surface-soil has not been the primary source of the large amounts of nitrogen taken up by the plants. That source must in fact be 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, one leguminous crop, beans, had been grown for many years in succession, and finally yielded very small crops, containing less than 30 lbs. of nitrogen per acre. The land was then left fallow for several years; barley and clover were sown in 1883, and in that year, 1884, and 1885, about 300 lbs. of nitrogen per acre were removed, chiefly in the clover crops. This result was obtained where another leguminous crop had practically 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 results adduced could leave no doubt that nitric acid was an important source of the nitrogen of the Leguminosæ. Indeed, existing experimental evidence 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* experiments, or in that of 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 is assumed much nitrogen has 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 about 0·04 or 0·05 per cent. of nitrogen, and not more than 6 or 8 parts of carbon to 1 part of nitrogen, was susceptible of nitrification. It was also found that nitrification was more active in leguminous than in gramineous crop subsoils. Obviously, however, the conditions of nitrification in which samples are exposed in the laboratory, are very different from those of the subsoil *in situ*.

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, 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 *Medicago sativa* for a number of years in succession, 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, in the autumn of 1885 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 nitrogenous bodies 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.

Also in the autumn of 1885, when this difficulty first arose, it was decided, in the mean time, 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. The acids used were the malic, citric, tartaric, oxalic, acetic, and formic.

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 twenty-four hours, or more, than after only one 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 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 do not justify any very definite conclusions 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.

Provided this were clearly established to be the case, the question would still remain, whether the complex nitrogenous body is merely rendered soluble, and taken up as such, as is probably the case with the fungi, or whether, after being attacked, it is subjected to further change before entering the plant?

In the autumn of 1885, Dr. G. Loges published the results of experiments in which he acted upon soils by pretty strong hydrochloric acid, and determined the amount of nitrogen taken up ('Versuchs-Stationen,' vol. 32, p. 201). One of his soils contained 0·804, and the other 0·367 per cent. of nitrogen; whilst the surface soil of the lucerne plot at Rothamsted contained only about 0·122, 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 states that in experimenting with a great variety of soils he has always found the hydrochloric acid extract gave the phosphotungstic precipitate, from which it is concluded that the substance taken up is an amide or peptone body.

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 acted upon by hydrochloric acid of various strengths, for shorter or longer periods, and at different temperatures. They found the nitrogen in the extract existed partly as ammonia, but in much larger proportion as soluble amides, and that the amounts obtained for both increased with the strength of acid, the time of contact, and the temperature. 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. Such re-precipitation is quite in accordance with the results obtained in our own experiments, in which less nitrogen remained dissolved after twenty-four hours, than after only one hour's contact, when, with the longer period, the acidity of the extract became neutralised.

As in Loges' experiments, so in those of MM. Berthelot and André, the strength of acid used was in all cases much greater than in that of the Rothamsted experiments, and very much greater than is likely to occur in any root-sap. Further, the soil they operated upon was about four times as rich in nitrogen as the Rothamsted subsoils, whilst, with the strongest acid, and a temperature of 100° C., about one-fourth 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 amide bodies, and as indicating the changes to which they are subject when acted upon by acids. Supposing the acid root-sap so to act on the insoluble organic nitrogen of the soil, and especially of the subsoil, as already said, 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 as, so far as our experiments go, nitric acid seems to be a more prominent constituent of the root-sap than ammonia, the question arises whether the liberated ammonia is not oxidated into nitric acid before being taken up? 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.

Supposing any considerable amount of the amide rendered soluble may be taken up by the plant as such, it is obviously of interest to consider what is the evidence bearing on the question whether plants can take up such bodies and assimilate their nitrogen? The conditions of experiment and the results obtained by various experimenters, have therefore been considered. The substances which have been experimented upon are—urea, uric acid, hippuric acid, guanine, phosphate of ammonia, glycocoll, creatine, and tyrosin. In some cases the experiments have been made in soil, but in most by the water-culture method.

In the majority of cases there could be little doubt that the complex nitrogenous body contributed nitrogen to the plant, either directly or indirectly. In the case of the experiments with soil as a matrix, there was no direct evidence that the plant took up the complex organic body, as such; and the probability is that it suffered change before becoming available. In some of the water-culture experiments, especially when urea was used, that substance was found within the plant, and it was concluded that it contributed directly as a source of nitrogen to it. Hampe also concluded that glycocoll was as available as nitric acid as nitrogenous food to plants.

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 connexion 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 may be briefly considered. 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. In the case of the Cupuliferæ the occurrence seemed to be universal, and it was to a great extent limited to them, though it has been observed on willows, and on some conifers. The development was the greatest in the first few inches or richer layers of soil. Frank considered the action to be one of true symbiosis, and concluded that the chlorophyllous tree acquires its soil nutriment through the agency of the fungus.

Here, then, is a mode of accumulation by some green-leaved plants which allies them 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 the action is the 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.

Only very brief reference can be here made to the numerous experiments which have been conducted in recent years, the results of which are held to afford evidence that free nitrogen contributes to the yield in our crops—either through the agency of the plant itself, or of the soil under the influence of micro-organisms, or of other non-chlorophyllous forms.

Some years ago, Berthelot called in question the validity of the conclusions from the experiments of Boussingault, ourselves, and others, in which it was sought to determine whether plants assimilated the free nitrogen of the atmosphere, by growing them in enclosed vessels which excluded the possibility of electrical action within the plant or the soil. It is at any rate coincident with the pretty general acceptance of this objection, which obviously puts out of court more exact methods, and exposes the experimenter to many more possible sources of error, that there has been a great accession of experimental evidence adduced, which is held to show the participation of the free nitrogen of the atmosphere in the results of growth. Had the results so obtained by various experimenters been at all accordant one with another, the fact might have been considered proof that the objection was fully justified. They are, however, in a quantitative point of view, so conflicting, without any adequate explanation in the methods described, that it is impossible to accept the whole as they stand, and for the present it seems necessary to hold judgment on them in abeyance.

The various results alluded to will be discussed in some detail in our full paper, but we can only briefly refer here to some of the various modes of explanation which have been suggested.

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 action of micro-organisms within the soil.

Frank, experimenting with a soil very rich in nitrogen, found a loss of combined nitrogen; but, in the case of vegetation experiments, with a less rich soil, he generally found a gain. He concluded that two opposite actions are at work within the soil—one by which nitrogen is set free, and another by which it is brought into combination; the latter being favoured by the presence of living plants. He admits that there is no decisive evidence how this takes place; but he seems to assume that it is under the influence of micro-organisms.

Hellriegel, again, found that lupins did not grow well in an experimental soil, until he added to it the watery extract of a soil from a field where lupins were growing luxuriantly. After this, his experimental plants also grew well, developed the well-known nodules on their roots, and showed a gain of nitrogen. This, he suggested, was probably due to the action of the nodules within the soil, bringing the free nitrogen of the air into combination, and thus rendering it available to the growing lupins. The results of Tschirch and those of Brunchorst have, however, been held to be conclusive against such a view. According to their experiments, the nodules have no external communication with the soil, but receive their nutriment from the plant itself. On this point it is of interest to observe that, according to the recent experiments of Mr. Marshall Ward, on the death of the nodules the spores become distributed in the soil, and, if this be the case, the possibility of some action, whatever that may be, is not yet disproved.

Whatever may be the exact facts in the cases cited, it is at any rate clear that recent lines of explanation of the mode in which some of the higher plants derive their nitrogen involve the supposition of the intervention of lower organisms in some way. It must, however, be admitted on a review of the conflicting results at present at command, that they do not justify any confident conclusion that the compensations supposed do take place in any important degree, or that free nitrogen is to any important extent brought into combination under the influence of the lower organisms. In the meantime it seems not inappropriate to devote attention to some other aspects of the subject.

We would submit that a careful consideration of the history of agriculture, both ancient and modern, fails to afford evidence of compensation such as is now sought for. Indeed we would say, as we have done before, that—"The history of agriculture throughout the world, so far as it is known, clearly shows that a fertile soil is one which has accumulated within it the residue of ages of previous vegetation, and that it becomes infertile as this residue is exhausted."

In conclusion, we would call attention to the fact, that in the Rothamsted soil and subsoil, down to the depth at which the action of roots has been proved, there exists a store of about 20,000 lbs. per acre of already combined nitrogen. It is true that many soils will contain much less, but many much more. There is then obviously still a wide field for inquiry as to whether or not, or in what way, the very large store of already existing combined nitrogen may become available to growing vegetation. We have indicated some of the lines of investigation which we are ourselves following up; and we would submit that, whether or not the lower organisms may be proved to have the power of bringing free nitrogen into combination, it would at any rate be not inconsistent with well-established facts, were it found that the lower serve the higher by bringing into an available condition the large stores of combined nitrogen already existing, but in a comparatively inert state, in our soils and subsoils.

November 17, 1887.

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

An Address to the Queen upon the completion of the fiftieth year of her reign, which on June 27th, during the recess of the Society, had been graciously received by Her Majesty from the hands of the President, was read from the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Sir James Cockle, Dr. Huggins, Dr. Rae, Mr. Stainton, and Mr. Symons were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

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

The following Papers were read :—

I. "Researches on the Spectra of Meteorites. A Report to the Solar Physics Committee." Communicated to the Royal Society at the request of the Committee. By J. NORMAN LOCKYER, F.R.S.

Preliminary Note. Received October 4, 1887.

Some years ago I commenced a research on the spectra of carbon in connexion with certain lines I had detected in my early photographs of the solar spectrum. I have been going on with this work at intervals ever since, and certain conclusions to which it leads, emphasising the vast difference between the chemical constitution of the sun and of some stars, recently suggested the desirability of obtaining observations of the spectra of meteorites and of the metallic elements at as low a temperature as possible.

I have latterly, therefore, been engaged on the last-named inquiries. The work already done, read in conjunction with that on carbon, seems to afford evidence which amounts to demonstration on several important points.

I think, therefore, that it may be of use to state some of the conclusions at once, though the researches are still very far from complete, and though they must be given with great reserve, as the astronomical observations with which I have had to compare my laboratory work have been frequently made under conditions of very great difficulty. The evidence before me suggests the following conclusions :—

(1.) The luminous phenomena, not only of comets, as determined on other grounds by Schiaparelli, but of all bodies in the heavens shining by their own light, except stars like the Sun and Sirius, are produced by meteorites in various aggregations and at different temperatures.

(2.) The temperature of the meteorites in some cases is about that of the oxyhydrogen flame.

(3.) Among the chief sources of fluting absorption in many "stars" are manganese vapours at a low temperature.

(4.) The bright flutings of carbon in some "stars," taken in conjunction with their absorption phenomena, indicate that widely separated meteorites at a low temperature are involved.

(5.) Olivine and kindred minerals appear to be chief bright-line-producing agents in the "nebulæ."

(6.) New stars are produced by the clash of meteor swarms, the bright lines seen being low temperature lines of those elements in meteorites the spectra of which are most brilliant at a low stage of heat.

(7.) The spectrum of the hydrogen in the case of the nebulæ seems to be due to low electrical excitation, as happens with the spectrum of carbon in the case of comets. Sudden changes from one spectrum to another are seen in the glow of meteorites in vacuum tubes, when a current is passing.

Addendum. Received November 15, 1887.

In anticipation of the detailed account and maps which are now being prepared, I beg to append a brief statement showing the line of investigation adopted, and how the various intercomparisons of laboratory and observatory work which have suggested the above general views have been made.

Experiments upon which the foregoing Conclusions depend.

A. *Experiments upon Carbon.*

The main conclusions which may be stated here are that there are two systems of flutings which depend upon temperature only. At low temperatures all compounds of carbon give a set of simple flutings, the brightest of which are at wave-lengths 4510, 4830, 5185, and 5610. At higher temperatures there is a series of compound flutings, the brightest edges of which are at wave-lengths 4380, 4738, 5165, and 5640. In the case of compounds of carbon with hydrogen, there is an additional fluting at wave-length 4310, and this is the only criterion for the presence of hydrocarbons among the flutings shown on the map (see Map 3).

B. *Experiments upon the Luminous Phenomena of the various Metals volatilised in the Bunsen Burner and the Oxy-coal-gas Blowpipe Flame as compared with the Phenomena seen at higher Temperatures.*

The main conclusions are that certain lines, bands, and flutings are seen in the bunsen burner, that a larger number is seen in the flame, and that the total number seen in the burner and flame is small.

The order of visibility in the bunsen is, roughly—

Lines	Mg Na Li Tl Sr Ba Ca K Mn Bi
Bands	Ca Sr Ba
Flutings	Mg Mn

All the observations both of bunsen and oxyhydrogen flame may be condensed as follows:—

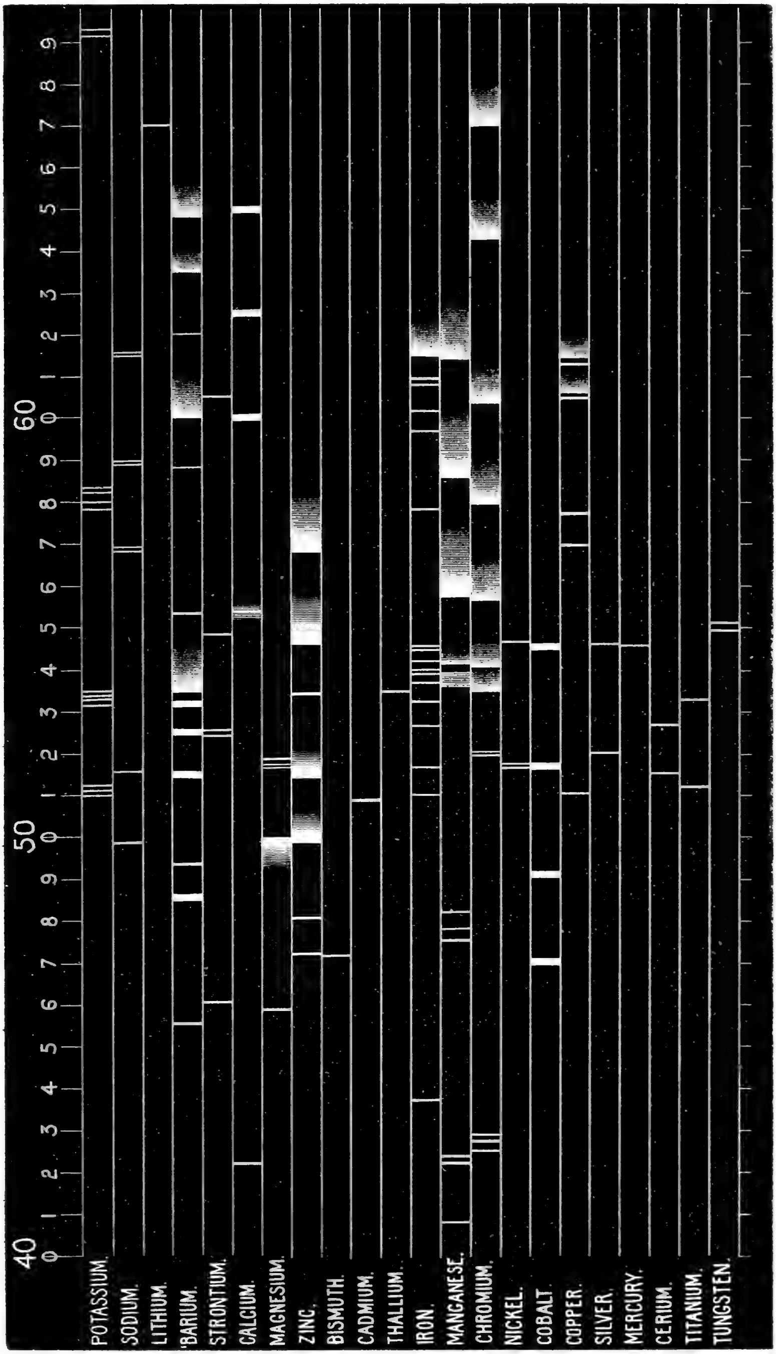
In metals of the alkalies	Na K Li
,, ,, alkaline earths	Ca Sr Ba
In magnesian metals	Mg Zn Cd
In iron metals........................	Fe Ni Co Mn Cr
In metals which yield acids	Bi Ti W
In copper metals	Cu Tl
In noble metals	Ag Hg
In earthy metals......................	Ce

The following table shows the positions of the principal lines, bands, and flutings seen in the spectrum of each of the metals examined, arranged roughly in the order of their intensities.

It should here be stated that as some of the researches have had to deal with feeble illumination small dispersion has been of necessity employed, and to make the observations along the several lines comparable a one-prism spectroscope has been so far used throughout. Hence the wave-lengths given are in all cases only approximate. With this proviso the lines observed have been as follows:—

Lines	In bunsen—						
	Mg	5183,	5172,	5167,	4586,	5201.	
	Na	5889,	5895.				
	Li	6705.					
	Tl	5349.					
	Sr	4607.					
	Ba	5534.					
	Ca	4226.					
	Mn	5395.					
	K	6950.					
	Bi	4722.					
	Seen on passing from the temperature of the bunsen to that of the oxy-coal-gas flame—						
	Fe	5268,	5327,	5371,	4383,	5790,	6024.
	Cu	5105,	5781,	5700.			
	Cr	5202,	5203,	5207,	5410.		
	Zn	4810,	4911.				
	Cd	5085.					
	Ni	5476.					
	Ti	5128,	5338.				
	W	5490,	5511.				
	Ag	5208,	5464.				
	Hg	5460.					
	Ce	5273,	5160.				
Bands	In bunsen—						
	Ca	5535,	6250,	6500,	6000.		
	Sr	6050.					
	Ba	5150,	5250,	5330,	4860.		
	Seen on passing from the temperature of the bunsen to that of the oxy-coal-gas flame—						
	Co	4710,	4920,	5170,	5460.		
Flutings	In bunsen—						
	Mg	5000.					
	Mn	5580,	5860,	6145,	5340.		
	Seen on passing from the temperature of the bunsen to that of the oxy-coal-gas flame—						
	Ba	6010,	6350,	6480.			
	Cr	5360,	5570,	5800,	6040.		
	Fe	6150.					
	Cu	6050,	6130.				
	Zn	5460,	5680,	4985,	5140,	5340.	

All the flutings, with the exception of magnesium, have their maxima towards the blue, and shade off towards the red end of the spectrum.



Map I.—Spectra of metals at the temperature of the oxy-coal-gas blowpipe.

C. *Experiments upon Mg at low Temperatures.*

I have again gone over the experiments already communicated to the Royal Society ('Roy. Soc. Proc.,' vol. 30, p. 27), and in addition have observed the spectrum of the metal burning in the centre of a large bunsen burner, in which case we get the line at 5201, and the fluting in the position of *b* without the fluting at 500. In the bunsen as ordinarily employed the fluting at 500 far eclipses the other parts of the spectrum in brilliancy, and at this temperature, as already observed by Messrs. Liveing and Dewar ('Roy. Soc. Proc.,' vol. 32, p. 202), the ultra-violet line visible is that at 373. Lecoq de Boisbaudran has observed the lines in the chloride at 4705 and 4483 ('Spectres Lumineux,' p. 85).

D. *Experiments upon the Glow of Na and Mg in Vacuum Tubes.*

A small piece of sodium, free from hydrocarbon, was placed in the lower limb of an end-on spectrum tube, and arrangements made for observing the spectrum of the gas evolved when the sodium was heated. Having first obtained as perfect a vacuum as possible, the sodium was gently heated, and the spectrum of the gas then gave nothing but the C and F lines of hydrogen. The pump being stopped and the sodium heated, a point was reached when C and F became very dim and were replaced by the structural spectrum of hydrogen.

In another experiment the sodium was replaced by a piece of magnesium along the end-on tube. The same process being gone through, similar phenomena were observed, but in the latter case there was a line at 500, in addition to the lines seen in the case of sodium.

The important point, then, is the existence of a line at 500 in the spectrum when magnesium is heated, and the absence of such a line in the gas evolved by sodium under the conditions stated.

E. *Experiments upon the Conditions under which the C and F Lines of Hydrogen disappear from the Spectrum.*

The association of the bright lines of hydrogen with nebulæ, many of the stars with bright lines, and the so-called new stars, points out at once that it is important to consider the various changes which hydrogen can undergo under various conditions of temperature and pressure. I pointed out many years ago that, when under certain conditions the spectrum of hydrogen is examined at the lowest possible temperature, the F line retains its brilliancy long after C disappears; and the fact that, after the chief lines of hydrogen have been made to disappear from the spectral tube, the spectrum which remains visible, and is sometimes very brightly visible, is also due to hydrogen, has always been a matter of thorough belief in my mind, although so many observers, down even to M. Cornu not so very long ago, have been inclined to attribute it to the existence of "impurities."

I began to map the so-called structural spectrum at the College of Chemistry in 1869, but other matters supervened which prevented the accomplishment of this work. This, however, is a matter of small importance, because quite recently Dr. Hasselberg has communicated to the St. Petersburg Academy an admirable memoir on the subject, accompanied by a map ('Mémoires de l'Académie Impériale,' Series vii, vol. 30, No. 7, Hasselberg). The brightest portions of the structure-spectrum are shown in Map 2.

The most convenient way of obtaining a supply of hydrogen for investigations of this kind is to use a little sodium which has never been in contact with hydrocarbon, or a piece of magnesium wire; to place them in the low end of a glass tube, one part of which can be used as an end-on tube, and then, after getting a vacuum so perfect that the spark will not pass, to slightly heat the metal. After a time the spectrum of hydrogen, sometimes accompanied by the low-temperature flutings of carbon, begins to be visible alike from the sodium and the magnesium.

If the vacuum has been very perfect to start with, at first the bright lines C and F will be visible without any trace of structure, and the hydrogen will be of a magnificent red colour. If now the action of the pump be stopped, and the sodium be still more heated, a point will be reached at which the conductibility of the gas is at its maximum, and then, the jar not being in circuit, the structure-spectrum of the gas will be seen absolutely alone, without any trace of either C or F. The gradual disappearance of the F line is very striking, and when the bright line is out of the field the lines due to the structure seem to be enhanced in brilliancy.

The brightest part of the spectrum is then that near D; in the blue-green we have a line at 464 more refrangible than F, and then a double line at 4930 and 4935; other less refrangible lines are seen. These are phenomena seen associated with sodium, but if we use the hydrogen produced from a piece of magnesium wire or from a crystal of olivine, under the same circumstances we find that so far as the lines of hydrogen go the phenomenon remains the same, but that there is then visible in the spectrum a line at 500, which has been recorded in the spectrum of magnesium under other conditions, not only by myself but by Dr. Copeland.*

* "To this table must be added 500·6 mmm. as the wave-length of the first line in the great band of magnesium as determined by M. Lecoq de Boisbaudran from the spark spectrum of the chloride of that metal, which evidently agrees with the flame spectrum, in this region at least. It is worthy of note that this line almost absolutely coincides with the brightest line in the spectra of planetary nebulæ." (Dr. Copeland, 'Copernicus,' vol. 2, p. 109.)

F. *Experiments upon the Spectra of Meteorites at low Temperatures.*

All the later observations recorded have been made on undoubted meteorites, fragments of which have been in the kindest manner placed at my disposal.

I. *In the Oxyhydrogen Flame.*

The observations gave in all only about ten or a dozen lines belonging to the metals magnesium, iron, sodium, lithium, and potassium, and two flutings, one of manganese, and one of iron.

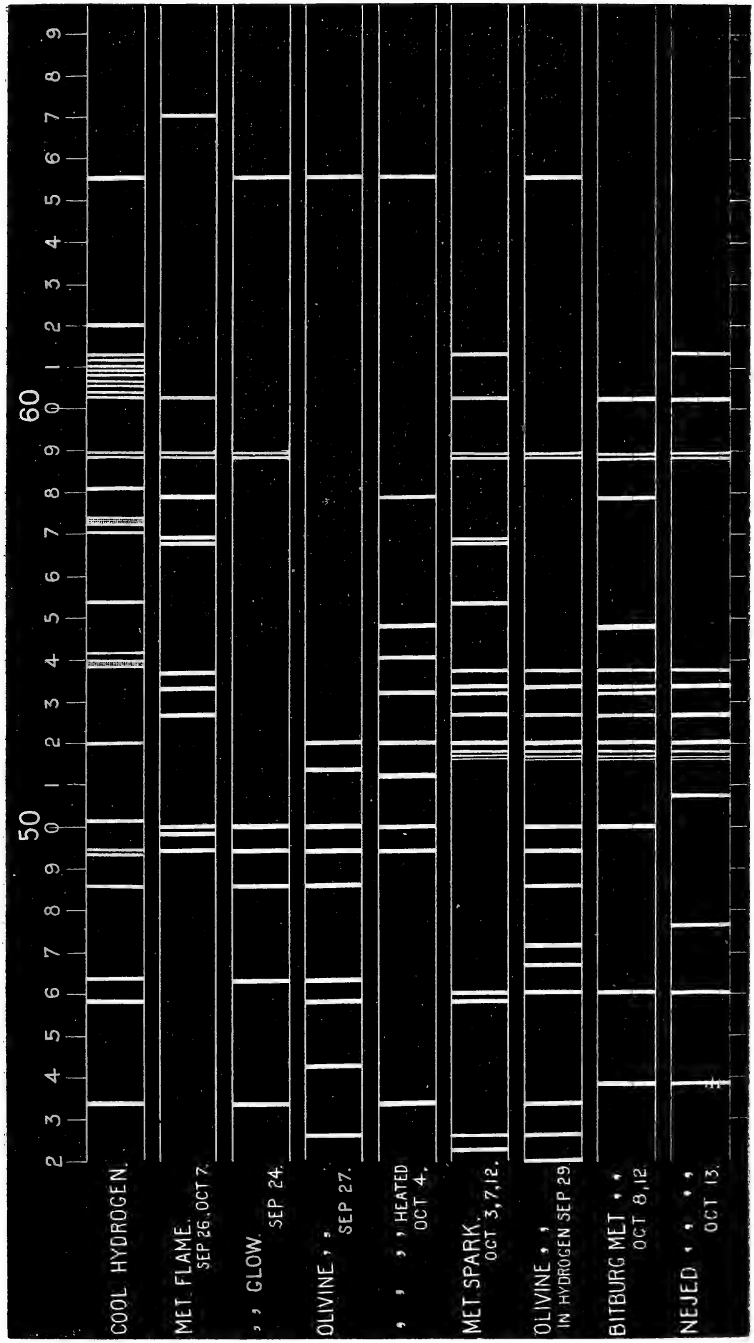
II. *With a Quantity Coil without Jar.*

The observations gave in all about twenty lines belonging to the metals magnesium, sodium, iron, strontium, barium, calcium, chromium, zinc, bismuth, and nickel, and four lines of unknown origin.

III. *When heated in a Vacuum Tube when a Current is passing along it.*

A small piece of iron meteorite was enclosed in the middle of a horizontal tube, so that the spark might be made to pass through the tube and over the meteorite. After complete exhaustion has been obtained, the first spectrum obtained when the tube, end on, is placed in front of the spectroscope, is a spectrum of hydrogen. The carbon flutings are only visible occasionally. If the meteorite then be very gently warmed by placing a bunsen burner at some distance below the tube, the glow over the meteorite is seen to change its colour, and the line at 500 is constantly, and another line at 495, apparently exactly in the position of the second line of the spectrum of the nebulæ, is occasionally, seen. This line is less refrangible than the structure line of hydrogen in this region, which occupies the same position as the barium line. This, however, if the heating is continued, especially in the case of stony meteorites, is soon succeeded by a much more brilliant green glow, in which magnesium *b* and many other lines appear, now accompanied by the carbon flutings. The observations made under all the above conditions are shown in Maps 2 and 2A.

In these observations if a line in the meteoric spectrum were coincident with a metallic line, with the dispersion employed, in the absence of the brightest line of that metal, the line was regarded as originating from some other substance. Thus a line was sometimes seen at 5480, apparently coincident, with the dispersion employed, with the green lines of Sr and Ni; sometimes the brightest line of Sr at 4607 was absent, and it was then fair to assume that the presence of 5480 was due to Ni, but in the presence of 4607 it might be due to Sr.


2 3 4 5 6 7 8 9 50 1 2 3 4 5 6 7 8 9 60 1 2 3 4 5 6 7 8 9
COOL HYDROGEN.
MET. FLAME. SEP 26, OCT 7.
,, GLOW. SEP 24.
OLIVINE ,, ,, SEP 27.
,, ,, ,, HEATED OCT 4.
MET. SPARK. OCT 3, 7, 12.
OLIVINE ,, ,, IN HYDROGEN SEP 29.
BITBURG MET ,, ,, OCT 8, 12.
NEJED ,, ,, ,, OCT 13.

MAP 2.

Map 2A.—Spectra of Olivine and Meteorites under various conditions.

Comparisons of the foregoing Observations among themselves, and with those made on various Orders of Celestial Bodies.

The discussions have taken, in the first instance, the form of comparisons of the different phenomena observed, and for this purpose all recorded observations of flutings and bright lines and dark lines in stars, comets, nebulæ, &c., have been carefully mapped in addition, all records having, when necessary, been brought to a common scale. Having these maps, I could then compare the totality of celestial observations with the laboratory work to which reference has already been made.

The following are among the comparisons already dealt with :—

I. The spectra of meteorites observed under the various conditions, chiefly considering magnesium, iron, and manganese, with the bright lines observed at low temperatures.

The main conclusions are :—

(1.) That only the lowest temperature lines of Mg, Na, Fe, Cr, Mn, Sr, Ca, Ba, K, Zn, Bi, and Ni are seen in the meteorites under the various conditions. They are not all seen in one meteorite or under one particular condition; the details of individual observations are fully recorded in Maps 2 and 2A.

(2.) That in the case of Mg the line most frequently seen is the remnant of the fluting at 500, while in a photograph the main ultraviolet line recorded is the one at 373, previously recorded under these conditions by Messrs. Liveing and Dewar. In the quantity spark other lines are seen, notably b_1, b_2, b_4, and 5201. The line at 500 was considerably brightened when the number of cells was reduced, thus showing it to be due to some molecule which can exist best at a low temperature.

(3.) That in the case of Mn the only line visible at the temperature of the Bunsen burner, 5395, is the only line seen in the meteorites.

(4.) That the lines of iron seen in the meteorites are those which are brightest when wire gauze is burned in the flame. The chief of these are 5268, 4383, 5790, and 6024; it is possible, however, that the two latter are due to some substance, not iron, common to the gauze and the meteorites.

II. The spectra of meteorites generally, with the bright lines and flutings seen in luminous meteors, comets, and some "stars."

α. Luminous Meteors.

With regard to the records of luminous meteors, it may be remarked that the observations, so far as they have gone, have given decided indications of magnesium, sodium, lithium, potassium, and of the carbon flutings seen in comets. The following quotations from

Konkoly and Professor Herschel are among the authorities which may be cited for the above statement.

"On August 12, 13, and 14 I observed a number of meteors with the spectroscope; amongst others, on the 12th, a yellow fireball with a fine train, which came directly from the Perseid radiant. In the head of this meteor the lines of lithium were clearly seen by the side of the sodium line. On August 13, at 10h. 46m. 10s., I observed in the north-east a magnificent fireball of emerald-green colour, as bright as Jupiter, with a very slow motion. The nucleus at the first moment only showed a very bright continuous spectrum with the sodium line; but a second after I perceived the magnesium line, and I think I am not mistaken in saying those of copper also. Besides that, the spectrum showed two very faint red lines."*

"A few of the green 'Leonid' streaks were noticed in November (1866) to be, to all appearances, monochromatic, or quite undispersed by vision through the refracting prisms; from which we may at least very probably infer (by later discoveries with the meteor-spectroscope) that the prominent green line of magnesium forms the principal constituent element of their greenish light."†

Again, later on in the same letter, Professor Herschel mentions Konkoly's observations of the bright *b* line of magnesium, in addition to the yellow sodium line in a meteor on July 26, 1873.

I again quote from Professor Herschel:—

"On the morning of October 13 in the same year, Herr von Konkoly again observed with Browning's meteor-spectroscope the long-enduring streak of a large fireball, which was visible to the north-east of O'Gyalla. It exhibited the yellow sodium line and the green line of magnesium very finely, besides other spectral lines in the red and green. Examining these latter lines closely with a star-spectroscope attached to an equatorial telescope, Herr von Konkoly succeeded in identifying them by direct comparison with the lines in an electric Geissler-tube of marsh-gas. They were visible in the star-spectroscope for eleven minutes, after which the sodium and magnesium lines still continued to be very brightly observable through the meteor-spectroscope."‡

The green line "*b*" of magnesium occurring as a bright line in luminous meteors indicates that their temperature when passing through our atmosphere is higher than that of the bunsen, and we may add of comets as generally observed, although some exhibit the *b* lines of magnesium and those of iron when at perihelion, as shown later on.

The two lines which Konkoly supposes are probably due to copper

* Konkoly, 'Observatory,' vol. 3, p. 157.

† Herschel, letter to 'Nature,' vol. 24, p. 507.

‡ *Ibid.*

will, I expect, be found to be iron lines when other observations are made of the spectra of meteors.

The main conclusions from this comparison are then: (1) that the temperature of luminous meteors is higher than that of the Bunsen flame; (2) that the meteorites which produce the phenomena we are now discussing are hotter than those in the experimental glow taken generally; and (3) that in both cases flutings of carbon may be seen.

β. *Comets.*

When the meteorites are *strongly* heated in a glow-tube, the whole tube when the electric current is passing gives us the spectrum of carbon.

When a meteor-swarm approaches the sun, the whole region of space occupied by the meteorites, estimated by Professor Newton in the case of Biela's comet to have been thirty miles apart, gives us the same spectrum, and further it is given by at all events part of the tail, which in the comet of 1680 was calculated to be 60,000,000 miles in length. The illumination therefore must be electrical, and possibly connected with the electric repulsion of the vapours away from the sun; hence it is not dependent wholly upon collisions.

Passing now from the flutings seen in cometary spectra, it is found that most of the lines which have been observed at perihelion are coincident with lines seen in experiments with meteorites, while the low temperature lines of Mg are absent. In the great comet of 1882, to which particular attention has been given on account of the complete record of its spectrum by Copeland,* the lines recorded were the D lines of sodium, the low temperature iron lines at 5268, 5327, 5371, 5790, and 6024, the line seen in the manganese spectrum at the temperature of the bunsen burner at 5395, and a line near *b* which might be due to magnesium, or to a remnant of the carbon fluting. In addition to these there was a line at 5475, probably due to nickel, the absence of the blue strontium line indicating that it is not likely to be the green line of strontium. There were also four other lines less refrangible than D, the origin of which has not yet been determined. As the comet got further from perihelion the lines gradually died out, those which remained longest being the iron line at 5268 and the line near *b*. The absence of D before the disappearance of all the lines is probably to be accounted for partly by the greater brightness of the continuous spectrum in that region.

In the comets of 1866-67, when seen away from the sun, the only line seen was the one at 500.†

* 'Copernicus,' vol. 2, p. 234.

† "In January, 1866, I communicated to the Royal Society the result of an examination of a small comet visible in the beginning of that year ('Roy. Soc. Proc.,' vol. 15, p. 5). I examined the spectrum of another small and faint comet in May,

It is fair to myself to say that I was not aware of these observations when I began to write this paper. The fact of the line at 500 remaining alone in Nova Cygni made it clear that if my views were correct, the same thing should happen with comets. It now turns out that the crucial observation which I intended to make was made twenty years ago.

In Comets *b*, 1881, and *c*, 1882, the only lines recorded were magnesium *b*; but, as before, the apparent absence of other lines might be due to continuous spectrum.

Of the five bands shown in Huggins's photograph of the spectrum of Comet Wells, taken with a wide slit, no less than three agree fairly in position with three lines seen in the spectra of meteorites. The wave-lengths of these are 4253, 4412, and 4769, and it is interesting to note that, so far, the origin of these lines is undetermined. The two remaining bands are at wave-lengths 4507 and 4634.

It is seen, then, that the spectra of comets—when their internal motions are relatively either slow or fast, and when therefore the number of collisions, and with it the heat of the stones in collision, will vary extremely—resemble the spectra of meteorites seen in glow tubes.

γ. "*Stars*" *with Flutings which have been observed in the Laboratory and in Luminous Meteors and Comets.*

The most prominent bright flutings of carbon are not only observed in luminous meteors and comets, but in stars of Class III*a*, and in some "Novas," notably Nova Orionis. So far, then, these bodies may in a certain measure be classed with luminous meteors and comets. But there is an important difference in the phenomena, for we have absorption as well as radiation. The discussion shows that the dark (or absorbing) flutings in these bodies are partly due to the absorption of light by the most prominent flutings of Mn and Zn, seen at low temperatures. This inquiry is being continued.

We have, then, in these bodies a spectrum integrating the *radiation* of carbon and the *absorption* of Mn and Zn vapour.

The law of parsimony compels us to ascribe the bright fluting of carbon in these stars to the same cause as that at work in comets, where we know it is produced by the vapours between the individual meteorites or repelled from them.

Hence we are led to conclude that the absorption phenomena are

1867. The spectra of these objects, as far as their feeble light permitted them to be observed, appeared to be very similar. In the case of each of these comets the spectrum of the minute nucleus appeared to consist of a bright line between *b* and F, about the position of the double line of the spectrum of nitrogen, while the nebulosity surrounding the nucleus and forming the coma gave a spectrum which was apparently continuous" (Huggins, 'Roy. Soc. Proc.,' vol. 16, p. 387).

produced by the incandescent vapour surrounding the individual meteorites which have been rendered intensely hot by collisions.

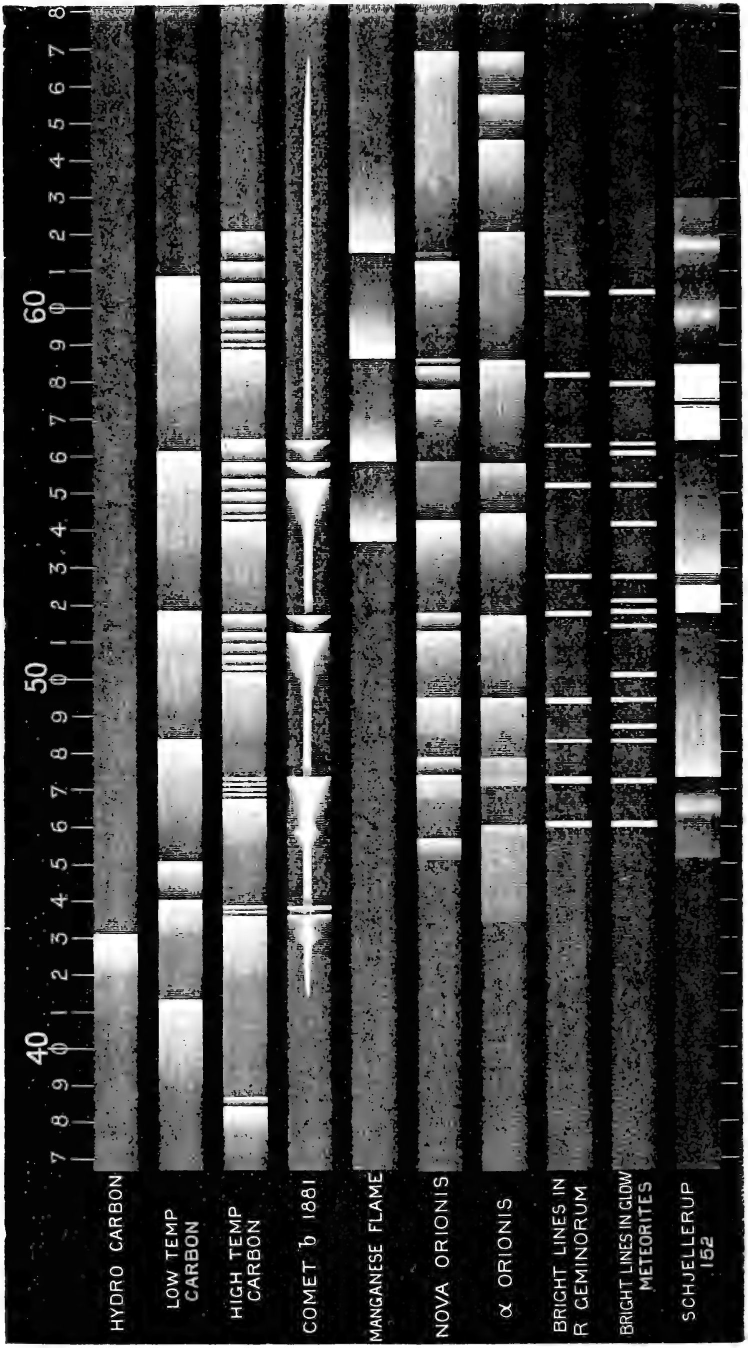
These stars, therefore, are not masses of vapour like our sun, but clouds of incandescent stones.

We have here probably the first stage of meteoritic condensation.

The Cases of Nova Orionis and R. Geminorum.

The stars with bright carbon flutings, the same as those seen in comets, are not limited to first-magnitude stars, such as α Orionis, but include at least one new star, Nova Orionis. Because the latter star lasted but a short time we might expect the phenomena presented to be different from those found in the first-magnitude star, which is a variable, like others with similar composite spectra. Practically there is a little difference, for in α Orionis, α Herculis, and others of that type, we find well-marked dark absorption flutings of manganese, as well as line-absorption of sodium and magnesium. The manganese absorptions agree with some of the Mn flutings seen in the Bessemer flame by Marshall Watts ('Phil. Mag.,' February, 1873). The absorptions are not so well developed in the Nova, for the reason, perhaps, that condensation due to gravity had not taken place to such a great extent, so that the heat of the stones themselves was not so great, and further because the local absorption around each meteorite would be cloaked by the bright radiation of the interspaces, which gives, as in comets, the maximum intensity to the bright fluting, wave-length 517. In R. Geminorum the demonstration of the same meteoric constitution, but without the strong absorption, is given by the fact that in that star so much of the light proceeds from the vapour produced by the meteorites, and from the carbon in the interspaces, that the carbon flutings and the bright lines of barium and strontium, and other substances present in meteorites, are visible at the same time, exactly as they are seen in the glow over a meteorite in an experimental tube, in which, as the pressure is reduced, the edges alone of the carbon flutings are visible, and put on the appearance of bright lines, almost exactly resembling the bright lines of the heated meteorites.

The spectra of these two stars I give on a map (Map 3) side by side with the bright flutings of carbon and the bright flutings of manganese with a view of showing that, both in the temporary Nova and the first magnitude star in the same constellation, many of the phenomena are the same and are therefore probably produced by the same cause. Some time after Dr. Copeland's original observations of this star were published, it was pointed out by Dunér, Vogel, and others, that some of the bright parts of the spectrum observed by him were really coincident with the bright parts of the spectrum of α Orionis; this, of course, is beyond question. But in addition to



Map 3.—Comparison of flutings seen in the spectra of "stars" and comets, with flutings of carbon, manganese, and zinc, and in the case of R. Geminorum lines with remnants of flutings and lines seen in a meteorite glow. (The Zn fluting is at λ 544 in α Orionis.)

these bright spaces Dr. Copeland gives some bright regions which, I think, have not been touched by the arguments of Vogel and Dunér above referred to. It will be observed that in the case of R. Geminorum, given on the same map as Nova and α Orionis, the bright lines correspond almost exactly with the bright spaces shown in the above-named stars and certain lines seen in meteorites—that is to say, a meteorite glow, when the carbon spectrum is bright, gives us all the lines recorded in the spectrum of the star, showing that some of the lines correspond with the brightest flutings of carbon.

There can be no question, I think, that in R Geminorum we have another stage, doubtless a prior stage, of the life-history not only of the Nova, but of α Orionis itself.

III. The spectra of meteorites glowing in tubes with the bright lines observed in celestial bodies—

(α) Comparison with the lines seen in nebulæ when C and F (bright) are either present or absent.

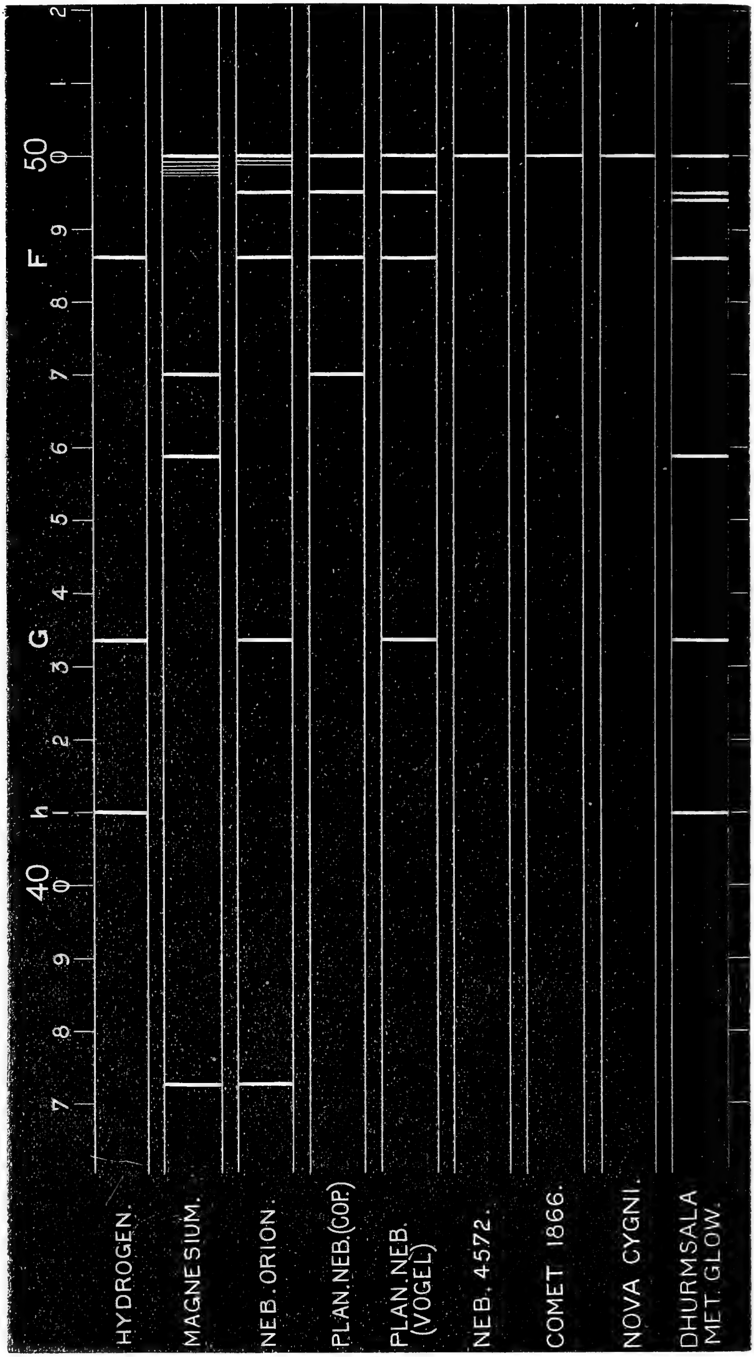
(β) Comparison with bright lines (not associated with flutings) seen in stars.

α. "*Nebulæ.*"

Only seven lines in all have been recorded up to the present in the spectra of nebulæ, three of which coincide with lines in the spectrum of hydrogen and three correspond to lines in magnesium. The magnesium lines represented are the ultra-violet low-temperature line at 373, the line at 470, and the remnant of the magnesium fluting at 500, the brightest part of the spectrum at the temperature of the bunsen burner. The hydrogen lines are *h*, F, and Hγ (434). Sometimes the 500 line is seen alone, but it is generally associated with F and a line at 495. The remaining lines do not all appear in one nebula, but are associated one by one with the other three lines. The lines at 500 and 495 and F have been seen in the glow of the Dhurmsala meteorite when heated, but the origin of 495 has not yet been determined.

The result of this comparison then is that the nebula spectrum is as closely associated with a meteorite glowing very gently in a very tenuous atmosphere given off by itself as is the spectrum of a comet near the sun with a meteorite glowing in a denser one also given off by itself when more highly heated.

Further, it has been seen that the nebula spectrum was exactly reproduced in the comets of 1866 and 1867, when away from the sun. As the collision of meteorites is accepted for the explanation of the phenomena in one case, it must, *faute de mieux*, be accepted for the other. The well-known constituents of meteorites, especially olivine,



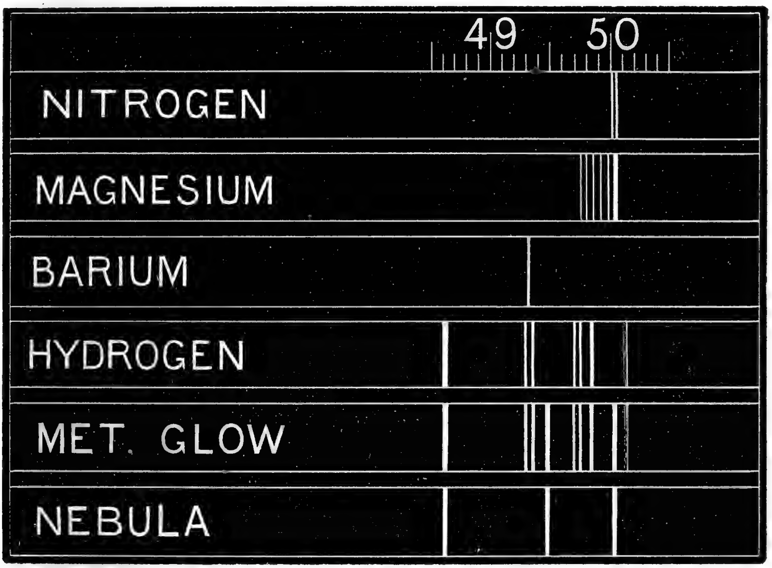
Map 4.—Spectra of nebulæ compared with the spectra of hydrogen, cool Mg, and meteorite glow.

fully explain all the spectroscopic phenomena presented by luminous meteors, comets, and nebulæ.

I published many years ago an experiment in which I had found that the gases evolved from meteorites under some conditions gave us the spectrum of hydrogen and under others the spectrum of carbon; but in the globes I then used I was not enabled to study the spectrum of the glow itself.

I should add that the line at 495 makes its appearance much more rarely than the one at 500, in meteorite glows.

Map 5 shows the positions of three of the nebula lines as compared with well-known lines.



Map 5.—Diagram showing the positions of the nebula lines as compared with lines of N, Mg, Ba, H, and meteorite glow.

β. "*Stars*" *with bright Lines.*

On reference to the map which I exhibit to the Society, though they and the discussion of them are yet incomplete, it will be seen that the principal lines which are seen bright in star spectra are, if we make due allowance for the discrepancies likely to occur in observations attended with great difficulties, lines which either have been observed in the vapours and gases given off by meteorites in vacuum-tubes, or which we might expect to see in a combined series of observations on meteorites having different chemical constituents. Among these lines are Hα, Hβ, Hγ, Hδ, 464, 540, 570, 580, 587; in one case (1st Cygnus) there are lines at 5065 and 5268, the latter due to iron. The difficulties attending this part of the inquiry are referred to subsequently, and it must be understood that in the absence of a detailed discussion, especially of the spectra of the "Novas," which I have not yet completed, the opinions I express in the next part of this preliminary notice with regard to bright-line stars must be regarded rather as suggestions than as final conclusions.

Discussion of the Maps showing the bright Lines visible in Stars and Nebulæ.

It results from the discussion of the bright lines seen, whether associated with the bright lines C and F of hydrogen or not, that, while on the one hand we have a class of bodies—the nebulæ—which give us the lines visible at the lowest temperature of chemical elements known to exist in meteorites, we have in the other class—the "stars" with bright lines—those lines visible at somewhat higher temperatures in meteorites. In the stars with bright lines the two most important lines, which have been separately mapped by Vogel,* occur at 540 and 582. The mean readings of all the observations give the positions of these lines as 540 and 580. In an experiment on the glow of a meteorite rich in manganese, the line of Mn at 5395, easily seen at the temperature of the bunsen, is distinctly seen in addition to the structure-spectrum of hydrogen. There is reasonable ground therefore for supposing that the line, this only one of the iron-group of metals visible at the temperature of the bunsen, may be the origin of one of the two lines seen alone in the spectrum of these "stars." It will be seen that in the map it has been easy to arrange all the bright lines hitherto seen in stars into one order, in which we begin with this line of manganese, and a line of iron seen at the temperature of the oxy-coal-gas flame, the wave-length of which is 579. As other lines indicating other substances are added to these fundamental ones, we pass from those stars in which C and F are not visible to those in which they make their appearance. Here, however, it is necessary to move with caution, because it may be that we are in presence of some of the lines visible in the structure-spectrum of hydrogen. The chief lines of hydrogen, as seen in the end-on tube when the conditions are such that C and F are not visible, have been already stated. Some of the lines observed in these stars, even the one at 540, have been found to be very nearly coincident with bright lines seen in the structure-spectrum, as well as with lines seen in the spectra of meteorites.

The suggestion, therefore, that some of the lines seen in bright-line stars are lines of cool hydrogen must be noted, although there are grounds for rejecting it, as will shortly appear. One objection is

* 'Publicationen des Astrophys. Observatoriums zu Potsdam,' vol. 4, No. 14.

that strong lines of the H structure at 607—610 and 574 have not been recorded in star spectra with those at 540 and 580.

In the nebulæ we deal chiefly with lines seen in the spectrum of magnesium at the lowest temperature; and these, as far as observations go, have not yet been associated with the lines at 540 and 580 to which reference has just been made, although they may or may not be associated with the bright lines C and F of hydrogen. In the nebulæ, however, no lines coincident with the lines of cool hydrogen have been observed. It will be seen, therefore, that we have here again strong grounds for rejecting the view that the lines seen in "stars" at 540 and 580 are due to cool H, for since hydrogen is common to both nebulæ and stars, there is no reason why structure-lines should occur in "stars" any more than in nebulæ.

Another ground for rejecting cool hydrogen as the origin of any of the lines in "stars" is that the structure-spectrum of hydrogen is only seen in confined glows, which is just the condition which cannot occur in space.

At the same time, the apparent coincidences of so many meteorite lines with structure-lines of hydrogen greatly increases the difficulties of laboratory work; in fact, the structure spectrum of hydrogen is to observations of meteorite glows in the laboratory what continuous spectrum is to observations of bright lines in stars.

If it be agreed that we are not dealing with cool hydrogen, then it will follow that the only difference between celestial bodies with bright lines in their spectra comes from no difference of origin or chemical constitution, but from a difference of temperature.

At one point in these researches I was under the impression that the differences in the systems of bright lines seen in the nebulæ and the bright-line stars might arise from a preponderance of irons or stones in the swarms. But I was led to abandon this idea, not only by the observation of the meteoritic glows, but by the consideration that even telescopically the "stars" in question are more condensed than the nebulæ.

The spectrum of the nebulæ, except in some cases, is associated with a certain amount of continuous spectrum, and meteorites glowing at a low temperature would be competent to give the continuous spectrum with its highest intensity in the yellow part of the spectrum; so that in this way we should understand that lines due to any gas or vapour in that part would be very much more likely to escape record than those in the part of the spectrum which the continuous spectrum hardly reaches. The general absence, however, of bright lines of metallic vapours, except 495 and 500, and of the bright lines of hydrogen, evidently justifies the conclusion that we are here in presence of those bodies in celestial space, connected with which the temperature and the electrical excitation are at the minimum, and it is very remarkable how the lines seen in a Geissler tube under the conditions stated, when either magnesium, or olivine, or other meteoric constituents are made to glow, should appear, one may almost say, indiscriminately among the orders of bodies in the heavens which up to the present time have been regarded as so utterly different in plan and structure as stars and nebulæ.

The records of purely continuous spectra in the case of many nebulæ, as for example the Great Nebula in Andromeda, is in all probability an indication of our inability to observe them properly. For a nebula to give a perfectly continuous spectrum, it is evident that the component meteorites must be incandescent, but still at a lower temperature than that required to give bright lines. Now the Mg line 500 is seen in some of the faintest nebulæ where there is little or no continuous spectrum, and it therefore seems likely that these are at a lower temperature than the nebulæ said to give perfectly continuous spectra. This being so, it is difficult to believe that other lines, which require a somewhat higher temperature for their existence than the line at 500, do not become visible at this increased temperature.

There can be little doubt that when our instrumental appliances and observing conditions become more perfect, it will be found that the so-called continuous spectra are really discontinuous. There is, indeed, an element of doubt as regards some of the existing observations; thus, the spectrum of the companion to the Great Nebula in Andromeda appears to end abruptly in the orange, and throughout its length is not uniform, but is evidently crossed by lines of absorption, or by bright lines.*

Again, the Great Nebula in Andromeda is generally regarded as having a continuous spectrum pure and simple, but an observer at Yale College (name not stated), has observed three bright lines in its spectrum ('Observatory,' vol. 8, p. 385). The lines are the F line of hydrogen, and two other lines at wave-lengths 5312·5 and 5594·0. The latter two lines are mentioned by the same observer as bright lines in γ Cassiopeiæ and β Lyræ, and are recorded by Sherman ('Astr. Nachr.' No. 2691) as bright lines in these stars and in Nova Andromedæ. No other observations with which I am acquainted give these two lines in γ Cassiopeiæ and β Lyræ, but Maunder ('Monthly Notices,' vol. 46, p. 20) gives them as two of the lines seen in Nova Andromedæ. It is possible, therefore, that the two lines in question, in the Yale College observations, had their origin in Nova Andromedæ; at all events there is no evidence to show that they are visible in the Great Nebula of Andromeda under normal conditions.

It is not impossible that the lines at 540 and 580 may be eventually traced in some of the brightest nebulæ, since these are apparently the lines next in order, as regards temperature, to the Mg line 500.

* Huggins, 'Phil. Trans.,' vol. 154, p. 441.

It is right that I should here point out that some observers of bright lines in these so-called stars have recorded a line in the yellow which they affirm to be in the position of D_3; while on the other hand, in my experiments on meteorites, whether in the glow or in the air, I have seen no line occupying this position.

I trust that some observer with greater optical means will think it worth his time to make a special inquiry on this point. The arguments against this line indicating the spectrum of the so-called helium are absolutely overwhelming. The helium line so far, has only been seen in the very hottest part of the sun which we can get at. It is there associated with *b*, and with lines of iron which require the largest coil and the largest jar to bring them out, whereas it is stated to have been observed in stars, where the absence of iron lines and of *b* shows that the temperature is very low. Further no trace of it was seen in Nova Cygni, and it has even been recorded in a spectrum in which C was absent, and once as the edge of a fluting.*

It is even possible that the line in question merely occupies the position of D_3 by reason of the displacement of D by motion of the "stars" in the line of sight. On this point no information is at hand regarding any reference spectrum employed. If, however, it should eventually be established that the line is really D_3, which probably represents a fine form of hydrogen, it can only be suggested that the degree of fineness which is brought about by temperature in the case of the sun, is brought about in the spaces between meteorites by extreme tenuity.

The Case of Nova Cygni.

The case of Nova Cygni is being discussed, and it appears likely that this "star" passed through all the stages of temperature represented by "stars" with bright lines, comets, and nebulæ. In the initial stage, the principal lines recorded were those of hydrogen, cool magnesium, and sodium. At a later date, in addition to these, lines apparently indicating hotter magnesium and carbon were observed. On the date of its highest temperature (December 8, 1876) the lines observed by Vogel indicate H, Na, Mg, C, Fe, Mn, and Ba, the "star" having then, it would appear from the discussion so far as it has yet gone, approached the condition of the great comet of 1882 at perihelion. The Fe, Ba, C, and Na gradually disappeared, then the hydrogen followed, and the last stage of all was that in which Mg (500) appeared alone, as in the comets of 1866–67 and in nebulæ. The complete discussion, however, must be reserved for a future communication. It is sufficient to say here that it is very

* ". . . . The spectrum is very bright: two strong bands are seen in the red, then the D line, followed by a bright line (D_3), as the edge of a band . . . . (Konkoly, "Neuer Stern bei χ^1 Orionis," 'Astr. Nachr.,' 2712).

probable that all the spectroscopic phenomena of Nova Cygni will admit of explanation on the supposition that it was produced by the collision of two swarms of meteorites. The outliers were first engaged, and at the maximum the denser parts of the swarm.

Difficulties connected with the Discussion.

An inspection of the maps, on which are shown all the observations already made upon bright lines recorded in the spectra of celestial bodies, will indicate at first sight an apparent variation of the positions of the lines greater than might have been expected. This, however, I think will vanish on the consideration of the whole question; and for my part certainly all the examinations which I have been able to make have led me to the conclusion that the various observations have been far better than it was almost possible to hope for when the great difficulties of the observations themselves are considered.

When it is remembered that, in order to get a determination of the position of a bright line, comparison-spectra and prisms are needed, and that, from mechanical considerations alone, the application of these aids to research is very frequently attended with difficulties and uncertainties; and further, when we consider that many of the observations have been necessarily made without these aids; the striking coincidences on the maps become of very much greater importance than the slight variations seen between the positions of the same line recorded by different observers in the same star.

It will be observed, too, that the information in some cases is fuller in the blue part of the spectrum. Here again a reference to what the maps are really intended to show is necessary. The maps do not show the complete spectrum observed, but only the bright lines recorded in it. The actual observations have really consisted in picking out these bright lines from the background of continuous spectrum, whether in stars, nebulæ, or comets; and, as the continuous spectrum will be generally brightest in the yellow and green, so in this part of the spectrum we must expect, first of all, to get the least information, and then, when the information is obtained, to get the greatest uncertainty, on account of the difficulty brought about by the greater luminosity of the background on which the line appears.

The discussion by Hasselberg and others of the various observations of comets which have been made from time to time indicates that under certain circumstances, where men of the highest skill and with the greatest care have determined the wave-lengths of the carbon bands, discrepancies exist too great to admit of their being attributed to errors inherent in this branch of observation.

If for a moment we consider alone the two bright flutings visible in the spectrum of carbon, one with its bright edge just more refrangible than b_4—this is the high-temperature spectrum—and the other—the low-temperature spectrum—with a fluting just less refrangible than b_1, it is at once suggested that sudden changes in comets may very likely be accompanied by a transition from one condition of carbon vapour to the other, so that on this account apparent discrepancies in the measurements of the same comet at different times may present real facts. Then again we have the motion of the swarm along its orbit, which in some cases we know is comparable to the velocity of light, so that variations of wave-length are produced as indicated in comet 1882. We also have the possibility that the velocity of the vapours in the jets, and that due to the electric repulsion—which, according to Zöllner's view, is the origin of comets' tails—may also produce changes of refrangibility.

Although as a rule the bright fluting seen in comets appears to be that due to high temperature, this is apparently not always the case. In the experiments on the glow of magnesium wire, the flutings of carbon have always been seen, and when the vacuum is approached the flutings have been those of the low-temperature spectrum. When the glow of the metal is seen under certain conditions, mixed with carbon vapour, b_1 and b_2 are seen as bright dots or short lines inside the carbon fluting, exactly as they were observed, probably, by Huggins in Brorsen's comet ('Roy. Soc. Proc.,' vol. 16, p. 386).

Authorities used in the Maps.

The map showing the bright lines in *Stars* is based upon the following authorities:—

3rd Cygnus, B.D. +36°, No. 3956, R.A. 20 h. 10 m. 6 s., Decl. +36° 18′.
Vogel.—'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 19.

2nd Cygnus, B.D. +35°, No. 4013, R.A. 20 h. 7 m. 26 s., Decl. +35° 50·8′.
Vogel.—'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 19.
Wolf and Rayet.—'Comptes Rendus,' vol. 65 (1867), p. 292. The wave-lengths were obtained from a curve based on the measurements given.

Argelander-Oeltzen 17681, R.A. 18 h. 1 m. 21 s., Decl. −21° 16·2′.
Vogel.—'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 15.
Pickering.—'Astronomische Nachrichten,' No. 2376.
Pickering.—'Observatory,' vol. 4, p. 82.

γ Argus, R.A. 8 h. 5 m. 56 s., Decl. −46° 59·5′.
Copeland.—'Copernicus,' vol. 3, p. 205.
Ellery.—'Observatory,' vol. 2, p. 418.

Stone 9168 (star in Scorpio), R.A. 16 h. 46 m. 15 s., Decl. −41° 37·6′.
Copeland.—'Copernicus,' vol. 3, p. 205.

1st Argus, R.A. 8 h. 51 m. 1 s., Decl. −47° 8′.
Copeland.—'Copernicus,' vol. 3, p. 206.

2nd Argus, R.A. 10 h. 36 m. 54 s., Decl. −58° 8′.
Copeland.—'Copernicus,' vol. 3, p. 206.

Gould 15305 (Argo), R.A. 11 h. 5 m. 19 s., Decl. −60° 21′.
Copeland.—'Copernicus,' vol. 3, p. 206.
Star in Centaurus, R.A. 13 h. 10 m. 37 s., Decl. −57° 31′.
Copeland.—'Copernicus,' vol. 3, p. 206.
Star in Cygnus, B.D. +37° No. 3821, R.A. 20 h. 7 m. 48 s., Decl. +38° 0·1′.
Copeland.—'Monthly Notices of the Royal Astronomical Society,' London, vol. 45, p. 90.
Lalande 13412, R.A. 6 h. 49 m. 15 s., Decl. −23° 46·8′.
Vogel.—'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 17.
Pickering.—'Astronomische Nachrichten,' No. 2376.
1st Cygnus, B.D. +35° No. 4001, R.A. 20 h. 5 m. 48 s., Decl. +35° 49·7′.
Vogel.—'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 17.
γ Cassiopeiæ, R.A. 0 h. 50 m. 4 s., Decl. +60° 7·2′.
Vogel.—'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 15.
Vogel.—'Beobachtungen zu Bothkamp,' Heft 2, p. 29.
Gothard.—'Astronomische Nachrichten,' No. 2581.
Konkoly.—Quoted by Gothard in 'Astronomische Nachrichten,' No. 2581. 'Observatory,' vol. 6, p. 332.
β Lyræ, R.A. 18 h. 45 m. 55 s., Decl. +33° 13·9′.
Vogel.—'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 15.
Vogel.—'Beobachtungen zu Bothkamp,' Heft 1, p. 33.
Gothard.—'Astronomische Nachrichten,' No. 2581.

The map showing the bright lines in *Nebulæ* is based upon the following authorities:—

Nebula in Orion.
Huggins.—'Roy. Soc. Proc.,' vol. 14, p. 39.
Planetary Nebula, R.A. 21 h. 22 m., Decl. +47° 22′.
Copeland.—'Copernicus,' vol. 1, p. 2.
Planetary Nebula.
Vogel.—'Monatsberichte der Akademie der Wissenchaften zu Berlin,' April, 1878, p. 303.
No. 4572, 2075 h., 16 H. 4, R.A. 20 h. 16 m. 7·9 s., N.P.D. 74° 20′ 19·3″.
Huggins.—'Philosophical Transactions,' vol. 154, p. 385.
Comet, 1886.
Huggins.—'Roy. Soc. Proc.,' vol. 15, p. 5.
Nova Cygni.
Lord Lindsay and Dr. Copeland.—'Copernicus,' vol. 2, p. 109.

The map showing the coincidence of flutings of carbon, manganese, and zinc, with bright lines and flutings in stars and comets, and in a meteorite glow, is based upon the following authorities:—

Hydrocarbon, Low temperature carbon, High temperature carbon } Work at Kensington.
Comet *b*, 1881.
Copeland.—'Copernicus,' vol. 2, p. 225.

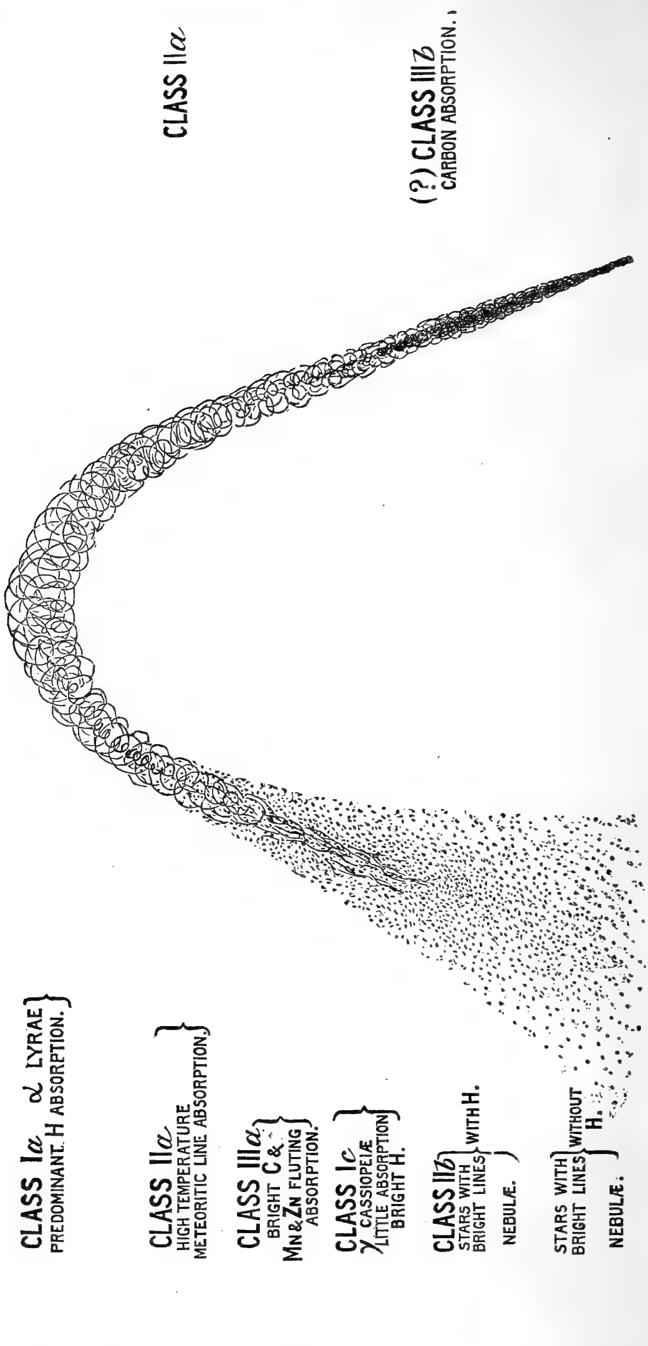
Manganese flame.
Lecoq de Boisbaudran.—' Spectres Lumineux.'
Work at Kensington.
Nova Orionis.
Copeland.—' Monthly Notices of the Royal Astronomical Society,' vol. 46 p. 109.
α Orionis.
Vogel.—' Beobachtungen zu Bothkamp,' Heft 1, p. 20.
R. Geminorum.
Vogel.—' Astronomische Nachrichten,' No. 2000.
Meteorite Glow.
Work at Kensington.
Schjellerup 152.
Vogel.—' Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 30.

On the Absorption Phenomena of Stars with bright Lines.

In addition to the map showing the bright lines visible in those stars the spectra of which contain them, I have prepared another map showing the absorptions which also occur. The two maps present a remarkable agreement—that is to say, there is the same progression in the absorption phenomena as there is in the bright line phenomena. In those stars in which bright lines are seen without the lines of hydrogen (in which stars the meteorite swarm is probably at a slightly higher temperature than that observed in the nebula when only the line at 500 is visible) we have no marked absorption-lines, but rather bands. When the hydrogen lines are added, as in *γ* Cassiopeiæ, then we get the absorption of sodium and *b* of magnesium, as we should expect. The individual meteorites therefore are much cooler in these stars than in the Novas, seeing that the absorption is so little developed. Speaking generally, therefore, we may say that there are two causes of minimum absorption phenomena in stars. In the first place, as in the bright-line stars, only a little vapour surrounds each meteorite, and that vapour consists of the substances visible at the lowest temperature; while, on the other hand, in stars like Sirius, in consequence of the absolute state of vapour, we only get practically the absorption of hydrogen, or at all events the absorption of hydrogen in great excess, due, I have very little doubt, in part, to the fact that most other substances have been dissociated by the intense heat resulting from the condensation of the meteorites.

Notes on the Provisional Temperature Curve.

In order to bring the various results referred to in this communication in a definite form before my own mind, I have prepared a diagram which I have called a temperature curve, so that on one side of it we may consider those stages in the various heavenly bodies in



Temperature Curve (provisional).

which in each case the temperature is increasing, while on the other arm of the curve we have that other condition in which we get first vaporous combination, and then ultimately the formation of a crust due to the gradual cooling of the mass. At the top of such a curve we shall of course have that condition in which the highest temperature must be assumed to exist. In a letter to M. Dumas in the year 1872, I suggested that possibly the simplification of the spectrum of a star might be associated with the highest temperature of the vapour, and that idea seems to have been accepted by other observers since that time. We shall have then stars of the first class at the top of the temperature curve. On the one arm of the curve representing increasing temperature we shall have at various heights those aggregations which give us indications of a gradually increasing temperature brought about by collisions, beginning with meteorites as widely separated as they can be to *keep up* any luminosity at all, and finally vaporous condensations due to gravity.

On the arm of the curve descending from stars of the first class to dark bodies like, say, the companion to Sirius, we must place those bodies where absorption of compound molecules is indicated. This we find in stars of Class III*b* of Vogel. But here a very interesting question arises. Between stars of the first class and that of III*b* we are bound to insert stars of Class II, already located naturally on the ascending arm.

The Case of equal Temperatures on either Side of the Curve.

Speaking roughly, it may be said that the construction of such a curve as this suggests that similar or nearly similar temperatures will be found on either side. This in the main, of course, is true; but it must be pointed out that, on the rising curve, the temperature will be that, as a rule, of individual meteorites and the vapours given out by them, while on the descending arm it will be the temperature of the consolidated mass, whether vaporous or becoming solid. But it is obvious that if we take two points near the top of the curve we shall have very nearly the same temperature of the atmosphere, by which I mean the temperature of the layers in either case which are most effective in producing the phenomena of absorption. To take a concrete case, stars of the second class are obviously, by the consent of all, of a lower temperature than stars of the first class: on which side, therefore, of the curve must they be placed? Or, to take a more concrete case still, our sun is a star of the second class: on which arm of the curve must we place the sun? Here we find ourselves in a position of some difficulty, but it would appear that future work may enable us really to divide stars of the second class into two series, and if we can do so there is very little doubt that one series will represent the phenomenon of decreasing temperature of the absorbing layers, while the other series will represent the phenomenon of increasing temperature.

What considerations are likely to help us in such an inquiry as this? The atmosphere of a star built up by meteorites should resemble in its constitution the totality of the chemical constitution of meteorites, and therefore it might be inferred that the spectroscopic phenomena presented by such an atmosphere would not be widely different from the spectroscopic phenomena presented by the vapours of many meteorites volatilised together.

To investigate this question I have obtained composite photographs of the spectra of several meteorites, with a solar spectrum for purposes of comparison. I find that, while, on the one hand, the composite photograph giving us the spectrum of the meteorites greatly resembles that of the sun, as it should do, there are some variations which suggest the line of separation to which I have before alluded. From Dr. Huggins's magnificent photographs of the stars we have learned that, as I had predicted years before the photographs were taken, the thickness of H and K varies very greatly in different stellar spectra. In those stars, presumably the hottest ones, in which we get the series of hydrogen lines almost alone as great absorbers, K is almost absent; it finally comes in, however, and after a certain stage has been reached it is the most important line in the spectrum. But there are stars in which the lines *h* and G of hydrogen are not very much more developed than they are in the case of our own sun, in which K is much thinner than in the solar spectrum; and associated with this condition of K there is the absorption of a hydrogen line more refrangible than K at wavelength 3800, which is not represented in the solar spectrum with anything like the intensity. The question arises, therefore, whether the enormous thickening of K observed in the sun and some other stars may not be limited to those stars which, like our sun, are reducing their temperature; for we certainly are justified in assuming that the temperature of the sun now is not so high as it was in an earlier stage of the development of the system. Such a difference as that, if it is subsequently established, can only come from the atmosphere, as an effect of cooling, becoming richer in those substances the lines of which get broader as the star cools down. We can easily imagine that during the process of cooling the relative quantities of the vapours should not always remain constant, although it is impossible in the present state of our knowledge to give any particular reason why such and such vapours should disappear from the spectrum in consequence of chemical combinations, while others should develop apparently in consequence of their retirement.

Hydrogen plus Carbon indicates mixed Swarms.

If we assume a brightening of the meteor-swarm due to collision as the cause of the so-called new stars, we have good grounds for supposing that in these bodies the phenomena should be mixed, for the reason that we should have in one part of the swarm a number of collisions probably of close meteorites, while among the outliers the collisions would be few. We shall in fact have in one part the conditions represented in Class III*a*, and in the other such a condition as we get in γ Cassiopeiæ. I have in another part of this paper discussed the flutings observed in Nova Orionis, and have shown that so far as they were concerned we have the radiation of carbon and the absorption of manganese; but there is evidence to show that with these fluted appearances bright lines were observed —D_3 and F, although no mention is made of C.*

We have here, there is little doubt, the *vera causa* of stellar long-period variability. 12 per cent. of stars of Class III*a* are variable, and 9 per cent. of Class III*b*. In the one case, meteor-swarms produce the increased brightness by colliding with those of the condensing one. In the other, they do so by their periastron passage round the dim condensed one. There is no variability, in the usual sense of the word, in stars like the sun and α Lyræ, and the reason is now obvious.

The Conditions of Collisions of Meteorites.

The Chemical Elements most frequently determined in Meteorites.

I think it well to give here as a reminder a short table showing the chief substances met with in meteorites. It will indicate the cause of the continued reference to the spectra of Mg, Fe, and Mn in what follows.

Siderites.

Nickel-iron, copper, manganese.
Troilite = FeS.
Graphite.
Schreibersite = iron and nickel phosphide.
Daubréeite = iron and chromium sulphide.

Siderolites.

Chondritic—

(α) Non-carbonaceous = Olivine = chrysolite = peridot = $(Mg,Fe)_2O_4Si$ = SiO_2 41·3, MgO 50·9. FeO 7·7.

* Konkoly, 'Astr. Nachr.,' 2712, D_3 and F; Riccò indicates D_3 in 'Astr. Nachr.,' 2707.

Enstatite $MgO_3Si = SiO_2$ 60, MgO 40.
Bronzite = Enstatite, in which some Mg is replaced by Fe.
Nickel-iron, manganese.
Troilite.
Chromite = iron protoxide 32, chromium sesquioxide 68, + Al and Mg.
Augite = pyroxene, SiO_2 55, CaO 23, MgO 16, MnO 0·5, FeO 4.
Silicate of calcium, sodium, and aluminium.

(β) Carbonaceous .... Carbon in combination with H and O.
Sulphates of Mg, Ca, Na, and K.

Non-chondritic—

Anorthite.
Enstatite.
Bronzite.
Olivine.
Augite.
Troilite.

I. *The Numbers of Meteorites in Space.*

It is well known that observations of falling-stars have been used to determine roughly the average number of meteorites which fall on the earth each twenty-four hours; and having this datum to determine the average distance apart between the meteorites in those parts of space which are traversed by the earth as a member of the solar system, Dr. Schmidt, of Athens, from observations made during seventeen years, found that the mean hourly number of luminous meteors visible on a clear moonless night by one observer was fourteen, taking the time of observation from midnight to 1 A.M.

It has been further experimentally shown that a large group of observers who might include the whole hemisphere in their observations would see about six times as many as are visible to one eye. Professor H. A. Newton and others have calculated that making all proper corrections the number which might be visible over the whole earth would be a little greater than 10,000 times as many as could be seen at one place. From this we gather that not less than twenty millions of luminous meteors fall upon our planet daily, each of which in a dark clear night would present us with the well-known phenomenon of a shooting star.

This number, however, by no means represents the total number of minute meteorites that enter our atmosphere, because many entirely invisible to the naked eye are often seen in telescopes. It has been suggested that the number of meteorites if these were included would be increased at least twenty-fold: this would give us 400 millions of meteorites falling on the earth's surface daily. If we consider, however, only those visible to the naked eye, and if we assume that the absolute velocity of the meteors in space is equal to that of comets moving in parabolic orbits, Professor H. A. Newton has shown that the average number of meteorites in the space that the earth traverses is in each volume equal to the earth about 30,000. This gives us a result in round numbers that the meteorites are distributed each 250 miles away from its neighbours.*

If, then, these observations may be accepted to be good for any part of space, we may, and indeed must, expect celestial phenomena which can be traced to meteorites in all parts of space.

Further, we have the experience of our own system that these meteors are apt to collect in groups.

A comet, it is now generally accepted, is a swarm of meteors in company. Such a swarm finally makes a continuous orbit by virtue of arrested velocities; impacts will break up large stones and will produce new vapours in some cases, which will condense into small meteoroids.

A meteorite in space under any of the conditions indicated by the comets, new stars, and such first-magnitude stars as α Orionis, will evidently be subject to collisions, but only to a greater number of collisions than those which must ordinarily occur if space is as full of meteorites as Professor Newton's calculations, from observations made on the earth, would naturally seem to indicate.

The Velocity of Luminous Meteors.

In spite of the difficulties which attend the observations necessary to determine the velocity of meteors entering our atmosphere, many observations have been made from which it may be gathered that the velocity is rarely under 10 miles a second or over 40 or 50. It is known that the velocities of some meteor-swarms are very different from those of others. Professor Newton, our highest authority on this subject, is prepared to consider that the average velocity may be taken to be 30 miles a second.

Result of Collisions.

If we take these velocities as representing what happens in other regions of space, and assume the specific heat of the meteorites to be 0·10, the increase in their temperature when their motions are arrested by impacts will be roughly as follows:—

* Article "Meteorites," Professor Newton, 'Encyclopædia Britannica,' 9th edition, vol. 16.

Velocity 1 mile per second	....	3,000° C.
" 10 " "	....	300,000
" 20 " "	....	1,200,000
" 30 " "	....	2,700,000
" 60 " "	....	10,800,000

It is clear, however, that we should under the conditions stated be more frequently dealing with grazes than collisions.

Comets due to Collisions of Meteorites.

The fact that comets are due to swarms of meteorites was first established by Schiaparelli in 1866, when he demonstrated that the orbit of the August meteors was identical with that of the bright comet of 1862.*

Nebulæ due to Collisions of Meteorites.

So far as I know the first suggestion that nebulæ were really in some manner associated with meteorites and not with masses of gas was made by Professor Tait in 1871.† I have used the suggestion in my lectures ever since, and it is now some years ago since I put it to an experimental test by showing that both the spectra of comets and nebulæ, so far as carbon and hydrogen were concerned, could be produced from a vessel containing the vapours produced by meteorites. More recently, M. Faye has stated in his works on the nebular hypothesis that the solar nebula may as probably have consisted of a cloud of stones as of a mass of gas. This view, however, has not been favoured by Dr. Huggins, who in his observations both on nebulæ and comets has inferred from the near coincidence of the line of 500 with the strong air line that we are probably in presence of nitrogen, or of a form of matter more elementary than nitrogen; the line at 373 being attributed by him also to some unknown form of hydrogen on account of its coincidence with one of the series of hydrogen lines in the ultra-violet observed in the spectra of stars of the first class.

"New Stars" due to Collisions of Meteorites.

The idea that the *Novas* which appear from time to time are due to collisions of meteorites was, I think, first advanced by myself in 1877, when I wrote in connection with Nova Cygni:—

* Letters to Father Secchi, printed in the 'Bollettino' of the Collegio Romano, and reproduced in 'Les Mondes,' vol. 13.

† "It seems to me that we have a series of indications of what (for want of a better phrase) may be called the *period of life* of a star or group; beginning with the glowing gases developed by impacts of agglomerating cold masses. (Planetary nebulæ and others irresolvable, such as those of Orion, Lyra, &c., where the spectrum consists of a very few bright lines only.)" (Professor Tait, 'Edinburgh, Roy. Soc. Proc.,' 1871.)

"The very rapid reduction of light in the case of the new star in Cygnus was so striking that I at once wrote to Mr. Hind to ask if any change of place was observable, because it seemed obvious that, if the body which thus put on so suddenly the chromospheric spectrum were single, it might only weigh a few tons, or even hundredweights, and, being so small, might be very near us. Mr. Hind's telescope was dismounted, and I have not yet got any information as to the change of position; and as I am now writing in the Highlands, away from all books, I have no opportunity of comparing the position now given by Lord Lindsay in R.A. 21h. 36m. 52s., Decl. + 42° 16′ 53″, with those given on its first appearance by Winnecke and others.

"We seem driven, then, from the idea that these phenomena are produced by the incandescence of large masses of matter, because if they were so produced, the running down of brilliancy would be exceeding slow.

"Let us consider the case, then, on the supposition of small masses of matter. Where are we to find them? The answer is easy:—in those small meteoric masses which, an ever-increasing mass of evidence tends to show, occupy all the realms of space."*

The Effects of Collisions.

The question of what must happen to the meteorites themselves in consequence of this system of collisions is worth going into thoroughly. A very cursory examination seems to indicate that much light is thrown on the condition of meteorites as we know them, and their division into iron and stony.

As 30 miles per second is a very frequent value obtained for the velocity of meteorites when they enter our atmosphere, it is possible to compare temperatures brought about by collisions with those produced by passage through our atmosphere. Two masses of meteoric iron meeting each other in space would probably, if moving with a certain velocity, be formed into a pasty conjoined mass, and this process might go on until an iron of large dimensions was formed, and the various meteorites thus welded together would present in time a very fragmentary appearance. While irons were thus increasing in size, collisions with smaller meteorites would be attended with very local increases of temperature, perhaps sufficient to volatilise the surface or allow it to be indented, and in this manner the well-known "thumb-marks" receive explanation.

The masses of iron, when in a state of fusion, whatever their size, would be able to include stony meteorites in their vicinity. In the case of stones it is easy to see that the result would be very different. Their collisions would have, most probably, the effect of reducing large pre-existing masses to smaller ones, and the collision of a large

* 'Nature,' vol. 16, p. 413.

stone with a large iron would probably effect the driving of the stone into fragments, while the iron would be liquefied so as to inclose some of the fragments in its mass.

These operations of Nature might go on either in free space, or in the head of a comet, or in meteor-swarms. They probably cause the appearance of the so-called new stars, and in these various circumstances the rate of subsequent cooling would of course be very different, so that the results would be very different indeed.

Large masses on collision probably destroy each other, produce fragments and vapour, which again condense. The heterogeneous structure is thus to a certain extent explained. On collision the part of the substance of the meteorite given up will depend on the temperature, and thus a mass of metallic iron mixed with silicates at low temperature will get rid of the iron at once, which must then perforce condense in a separate swarm; therefore under low temperature conditions, say at aphelion, irons alone will be formed and the stones will become spongy. The stones will absorb the carbon and hydrogen vapours.

General Conclusions.

The general conclusions to which the foregoing investigations lead may thus be stated:—

I. All self-luminous bodies in the celestial spaces are composed of meteorites, or masses of meteoritic vapour produced by heat brought about by condensation of meteor-swarms due to gravity.

II. The spectra of all such bodies depend upon the heat of the meteorites, produced by collisions, and the average space between the meteorites in the swarm, or in the case of consolidated swarms, upon the time which has elapsed since complete vaporisation.

III. The temperature of the vapours produced by collisions in nebulæ, stars without C and F but with other bright lines, and in comets away from perihelion, is about that of the bunsen burner.

IV. The temperature of the vapours produced by collision in α Orionis and similar stars is about that of the Bessemer flame.

V. The line of increase of temperatures of the swarms of meteorites and subsequent cooling of the mass of vapour produced, and the accompanying phenomena, may be provisionally stated as follows:—

Sequences of Spacing and Temperatures (Provisional).

From Cold to Hot = Sparse to Dense Swarms.

	Spectrum of interspace.		Spectrum of vapour of meteorite.		Spectrum of meteorite.
	H.	C.	Radiation.	Absorption.	Radiation.
Nebulæ (without F)	Nil	Nil	Mg (500) ± 495		
Comets 1866 and 1867	Nil	Nil	Mg (500)	Nil	
Nova Cygni after collision	Nil	Nil	Mg (500)		Dimly discontinuous.
Stars with bright lines (without F)	Nil	Nil	Fe, Mn	Broad band 475	
Nebulæ (with F)	H	Nil	Mg (500) ± 495	Nil	
Stars with bright lines (with F)	H	Nil	Fe, Mn	D, *b*, and other lines and bands	Continuous.
Comets under mean conditions of collision	Nil	C	Mg(*b*)	Nil	
Comets at perihelion	Nil	C	Meteorite lines	(?)	Continuous.
Stars, Class III*a*	Nil	C	......	Meteorite flutings and lines	Vividly continuous.
Mixed swarms—					
R. Geminorum	H	C	Meteorite lines	Meteorite lines.	
Nova Orionis at maximum	H	C	......	Meteorite flutings and lines.	
Condensation.					
Stars, Classes I and II			Continuous	High temperature lines of substances present in meteorites	The radiation from individual meteorites now gives place to radiation from the interior vaporous and subsequently consolidated mass of the condensed swarm.
Subsequent Cooling.					
Stars { Class II, some stars, including the Sun			Continuous	K in excess	
Stars { Class III*b*				Flutings of carbon	

VI. The brilliancy of these aggregations, at each increasing temperature, depends on the number of meteorites in the swarm, *i.e.*, the difference depends upon the quantity, and not the intensity, of the light.

VII. The existing distinction between stars, comets, and nebulæ rests on no physical basis.

VIII. The main factor in the various spectra produced is the ratio of the interspaces between the meteorites to their incandescent surface.

IX. When the interspace is very great, the tenuity of the gases given off by collisions will be so great that no luminous spectrum will be produced ("nebulæ" and "stars" without F bright). When the interspace is less, the tenuity of the gas will be reduced, and the vapours occupying the interspaces will give us bright lines or flutings ("nebulæ" and "stars" with F bright). When the interspace is relatively small, and the temperature of the individual meteorites therefore higher, the preponderance of the bright lines or flutings in the spectrum of the interspaces will diminish, and the incandescent vapour surrounding each meteorite will indicate its presence by absorbing the continuous spectrum-giving light of the meteorites themselves.

X. 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. H. Darwin*—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."

XI. New stars, whether seen in connexion with nebulæ or not, are produced by the clash of meteor-swarms, the bright lines seen being low-temperature lines of elements the spectra of which are most brilliant at a low stage of heat.

XII. Most of the variable stars which have been observed belong to those classes of bodies which I now suggest are uncondensed meteor-swarms, or condensed stars in which a central more or less solid condensed mass exists. In some of those having regular periods the variation would seem to be partly due to swarms of meteorites moving around a bright or dark body, the maximum light occurring at periastron.

XIII. The spectrum of hydrogen seen in the case of the nebulæ seems to be due to low electrical excitation, as happens with the

* 'Nature,' vol. 31, p. 25.

spectrum of carbon in the case of comets. Sudden changes from one spectrum to the other are seen in the glow of meteorites in vacuum tubes when a current is passing, and the change from H to C can always be brought about by increased heating of the meteorite.

XIV. Meteorites are formed by the condensation of vapours thrown off by collisions. The small particles increase by fusion brought about again by collisions, and this increase may go on until the meteorites may be large enough to be smashed by collisions, when the heat of impact is not sufficient to produce volatilisation of the whole mass.

XV. Beginning with meteorites of average composition, the extreme forms, iron and stony, would in time be produced as a result of collisions.

XVI. In recorded time there has been no such thing as a world on fire, or the collision of masses of matter as large as the earth, to say nothing of masses as large as the sun; but the known distribution of meteorites throughout space indicates that such collisions form an integral part of the economy of nature. The number of bodies, however, subject to such collisions is extremely small, and must, it would appear, form but a small percentage of the celestial bodies, seeing that they must be dark and cold.

XVII. *Special Solar Applications.*

α. The solar spectrum can be very fairly reproduced (in some parts of the spectrum almost line for line) by taking a composite photograph of the arc spectrum of several stony meteorites, chosen at random, between iron meteoric poles.

β. The carbon which originally formed part of the swarm the condensation of which produced the solar system, has been dissociated by the high temperature brought about by that condensation.

γ. The indications of carbon which I discovered in 1874 ('Roy. Proc. Soc.,' vol. 37, p. 308) will go on increasing in intensity slowly, until a stage is reached when, owing to the reduction of temperature of the most effective absorbing layer, the chief absorption will be that of carbon—a stage in which we now find the stars of Class III*b* of Vogel's classification.

δ. At the present time it seems probable that among the chief changes going on in the solar spectrum are the widening of K and the thinning of the hydrogen lines.

I have finally to express my great obligations to Messrs. Fowler, Taylor, and Richards, who have helped me in various ways in the researches embodied in this paper. Mr. Fowler, the assistant to the Solar Physics Committee, has made most of the observations on meteorites, and low-temperature spectra generally, which have been

recorded on the maps, and he has carried out this work with a care, skill, and patience beyond all praise. The observations have in nearly every case been checked also by myself. Mr. Taylor, the Demonstrator of Astronomy, has been chiefly responsible for looking up the literature and mapping the results, in which he has been aided by Mr. Richards.

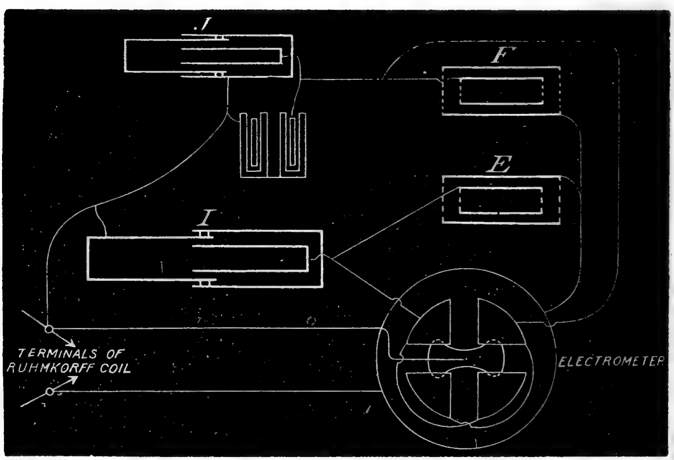
II. "Specific Inductive Capacity." By J. Hopkinson, M.A., D.Sc., F.R.S. Received October 14, 1887.

The experiments which are the subject of the present communication were originally undertaken with a view to ascertain whether or not various methods of determination would give the same values to the specific inductive capacities of dielectrics. The programme was subsequently narrowed, as there appeared to be no evidence of serious discrepancy by existing methods.

In most cases the method of experiment has been a modification of the method proposed by Professor Maxwell, and employed by Mr. Gordon. The only vice in Mr. Gordon's employment of that method was that plates of dielectrics of dimensions comparable with their thickness were regarded as of infinite area, and thus an error of unexpectedly great magnitude was introduced.

For determining the capacity of liquids, the apparatus consisted of a combination of four air condensers, with a fifth for containing the liquid arranged as in a Wheatstone's bridge, fig. 1. Two, E, F, were

Fig. 1.

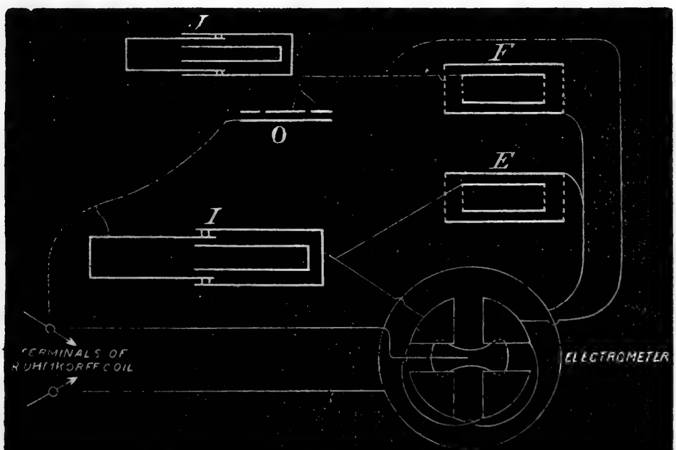


of determinate and approximately equal capacity; the other two, J, I, were adjustable slides, the capacity of either condenser being varied by the sliding part. The outer coatings of the condensers E, F, were connected to the case of the quadrant electrometer, and to one pole of the induction coil; the outer coatings of the other pair, J, I, were connected to the needle of the electrometer and to the other pole of the induction coil. The inner coatings of the condensers J, F, were connected to one quadrant, and I, E, to the other quadrant of the electrometer. The slide of one or both condensers J, I, was adjusted till upon exciting the induction coil no deflection was observed on the electrometer. A dummy was provided with the fluid condenser, as in my former experiments, to represent the necessary supports and connexions outside of the liquid. Let now x be the reading of the sliding condenser when no condenser for fluid is introduced, and a balance is obtained. Let y be its reading when the condenser is introduced fitted with its dummy, z when the full condenser is charged with air. Let z_1 be the reading when the condenser charged with fluid is introduced, then will K, the specific inductive capacity of the liquid, be equal to $(y-z_1)/(y-z)$.

Three fluid condensers were employed, one was the same as in my former experiments.* Another was a smaller one of the same type arranged simply to contain a smaller quantity of fluid. The third was of a different type designed to prove that by no chance did anything depend on the type of condenser; this done it was laid aside as more complicated in use.

To determine the capacity of a solid, the guard-ring condenser of

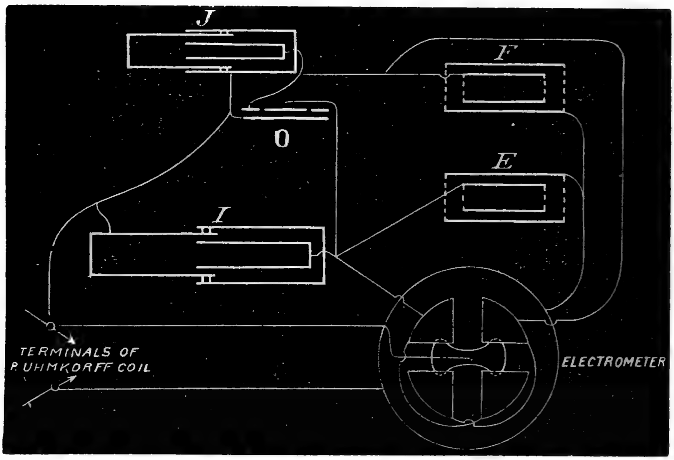
Fig. 2.



* 'Phil. Trans.,' 1881, Part II.

my previous experiments* was used. Advantage was taken of the fact that at the time when there is a balance the potentials of the interiors of all the condensers are the same. Let the ring O of the guard-ring condenser be in all cases connected to J, let the inner plate of the guard-ring be connected to J as in fig. 2, and let a balance be obtained. Let the inner plate be now transferred to I as in fig. 3, and again let a balance be obtained; the difference of the

Fig. 3.



two readings on the slide represents on a certain arbitrary scale the capacity of the guard-ring condenser at its then distance.

In some cases it was necessary to adjust both condensers to obtain a balance, then the value of a movement of the scale of one condenser in terms of the other was known from previous experiment. In some cases it was found most convenient to introduce a condenser of capacity known in divisions of the scale of the sliding condenser coupled as forming part of the condenser J. The old method of adding the opposite charges of two condensers then connecting to the electrometer and adjusting until the electrometer remained undisturbed was occasionally used as a check; it was found to give substantially the same results as the method here described when the substance insulated sufficiently well to give any results at all.

Colza Oil.—This oil had been found not to insulate sufficiently well for a test by the method of my former paper. Most samples, however, were sufficiently insulating for the present method. Seven samples were tested with the following mean results:—

* 'Phil. Trans.,' 1878, Part I.

No. 1. This oil was kindly procured direct from Italy for these experiments by Mr. J. C. Field, and was tested as supplied to me—

$$K = 3{\cdot}10.$$

No. 2 was purchased from Mr. Sugg, and tested as supplied—

$$K = 3{\cdot}14.$$

No. 3 was purchased from Messrs. Griffin, and was dried over anhydrous copper sulphate—

$$K = 3{\cdot}23.$$

No. 4 was refined rape oil purchased from Messrs. Pinchin and Johnson, and tested as supplied—

$$K = 3{\cdot}08.$$

No. 5 was the same oil as No. 4, but dried over anhydrous copper sulphate—

$$K = 3{\cdot}07.$$

No. 6 was unrefined rape purchased from Messrs. Pinchin and Johnson and tested as supplied, the insulation being bad, but still not so bad as to prevent testing—

$$K = 3{\cdot}12.$$

No. 7. The same oil dried over sulphate of copper—

$$K = 3{\cdot}09.$$

Omitting No. 3, which I cannot indeed say of my own knowledge was pure colza oil at all, we may, I think, conclude that the specific inductive capacity of colza oil lies between 3·07 and 3·14.

Professor Quincke gives 2·385 for the method of attraction between the plates of a condenser, 3·296 for the method of lateral compression of a bubble of gas. Palaz* gives 3·027.

Olive Oil.—The sample was supplied me by Mr. J. C. Field—

$$K = 3{\cdot}15.$$

The result I obtained by another method in 1880 was 3·16.

Two other oils were supplied to me by Mr. J. C. Field.

Arachide.—K = 3·17.

Sesame.—K = 3·17.

A commercial sample of *raw linseed oil* gave K = 3·37.

Two samples of *castor oil* were tried; one newly purchased gave

* 'La Lumière Électrique,' vol. 21, 1886, p. 97.

K = 4·82; the other had been in the laboratory a long time, and was dried over copper sulphate—

$$K = 4·84.$$

The result of my earlier experiments for castor oil was 4·78; the result obtained subsequently by Cohn and Arons* is 4·43. Palaz gives 4·610.

Ether.—This substance as purchased, reputed chemically pure, does not insulate sufficiently well for experiment. I placed a sample purchased from Hopkin and Williams as pure, over quicklime, and then tested it. At first it insulated fairly well, and gave K = 4·75. In the course of a very few minutes K = 4·93, the insulation having declined so that observation was doubtful. After the lapse of a few minutes more observations became impossible. Professor Quincke in his first paper gives 4·623 and 4·660, and 4·394 in his second paper.

Bisulphide of Carbon.—The sample was purchased from Hopkin and Williams, and tested as it was received—

$$K = 2·67.$$

Professor Quincke finds 2·669 and 2·743 in his first paper, and 2·623 in his second. Palaz gives 2·609.

Amylene.—Purchased from Burgoyne and Company—

$$K = 2·05.$$

The refractive (μ) index for line D is 1·3800,

$$\mu^2 = 1·9044.$$

Of the benzol series four were tested: *benzol, toluol, xylol,* obtained from Hopkin and Williams, *cymol* from Burgoyne and Company.

In the following table the first column gives my own results, the second those of Palaz, the third my own determinations of the refractive index for line D at a temperature of 17·5° C., and the fourth the square of the refractive index:—

			μ.	μ^2.
Benzol	2·38	2·338	1·5038	2·2614
Toluol	2·42	2·365	1·4990	2·2470
Xylol	2·39	—	1·4913	2·2238
Cymol	2·25	—	1·4918	2·2254

For benzol Silow found 2·25, and Quincke finds 2·374.

The method employed by Palaz is very similar to that employed by myself in these experiments; but, so far as I can ascertain from his paper, he fails to take account of the induction between the case of

* 'Wiedemann's Annalen,' vol. 28, p. 474.

his fluid condenser and his connecting wire; he also supports the inner coating of his fluid condenser on ebonite; and, so far as I can discover, fails to take account of the fact that this also would have the effect of diminishing to a small extent the apparent specific inductive capacity of the fluid. Possibly this may explain why his results are in all cases lower than mine. Determinations have also been made by Negreano ('Comptes Rendus,' vol. 104, 1887, p. 423) by a method the same as that employed by myself.

Three substances have been tried with the guard-ring condenser—double extra dense flint-glass, paraffin wax, and rock salt. The first two were not determined with any very great care, as they were only intended to test the convenience of the method. For double extra dense flint-glass a value 9·5 was found; the value I found by my old method was 9·896. For paraffin wax 2·31 was obtained—my previous value being 2·29. In the case of rock salt the sample was very rough, and too small; the result was a specific inductive capacity of about 18, a higher value than has yet been observed for any substance. It must, however, be received with great reserve, as the sample was very unfavourable, and I am not quite sure that conduction in the sample had not something to do with the result. In the experiments with the guard-ring condenser the disturbing effect of the connecting wire was not eliminated. My thanks are due to my pupil, Mr. Wordingham, for his valued help in carrying out the experiments.

Presents, November 17, 1887.

Transactions

Baltimore:—Johns Hopkins University. Circulars. Vol. VI. Nos. 58–9. 4to. *Baltimore* 1887; Studies from the Biological Laboratory. Vol. IV. Nos. 1–2. 8vo. *Baltimore* 1887; Studies in Historical and Political Science. Fifth Series. Nos. 7–9. 8vo. *Baltimore* 1887; Register. 1886–87. 8vo. *Baltimore* 1887.
The University.

Medical and Chirurgical Faculty of the State of Maryland. Transactions. Session 89. 8vo. *Baltimore* 1887.
The Faculty.

Peabody Institute. Annual Report, 1887. 8vo. *Baltimore.*

Batavia:—Bataviaasch Genootschap van Kunsten en Wetenschappen. Notulen. Deel XXV. Aflev. 1–2. 8vo. *Batavia* 1887; Tijdschrift voor Indische Taal-, Land- en Volkenkunde. Deel XXXI–XXXII (Aflev. 1). 8vo. *Batavia* 1887; Dagh-Register gehouden int Casteel Batavia. 1640–1641. Van J. A. Van der Chijs. Large 8vo. *Batavia* 1887.
The Society.

Transactions (*continued*).

Berlin:—Königl. Akademie der Wissenschaften. Abhandlungen. 1886. 4to. *Berlin* 1887. Sitzungsberichte. Nos. 19–34. 8vo. *Berlin* 1887. The Academy.

Brussels:—Société Royale Malacologique de Belgique. Annales. Tome XXI. 8vo. *Bruxelles* [1887]; Procès-verbaux des Séances. Année 1887. 8vo. *Bruxelles.* The Society.

Cardiff:—Naturalists' Society. Report and Transactions. Vol. XVIII. 8vo. *Cardiff* 1887. The Society.

Cordova:—Academia Nacional de Ciencias. Boletin. Tomo IX. Entregas 1–4. 8vo. *Buenos Aires* 1886. The Academy.

Delft:—École Polytechnique. Annales. Tome III. Livr. 2. 4to. *Leide* 1887. The School.

Dublin:—Royal Dublin Society. Proceedings. Vol. II. Parts 5–6. Vol. V. Parts 3–6. 8vo. *Dublin* 1880, 1886–87; Transactions. Vol. I. Nos. 10–12. Vol. II. No. 1. Vol. III. Nos. 11–13. 4to. *Dublin* 1880, 1886–87. The Society.

Edinburgh:—Geological Society. Transactions. Vol. V. Part 3. 8vo. *Edinburgh* 1887; Catalogue of the Library. 8vo. *Edinburgh* 1887. The Society.

Emden:—Naturforschende Gesellschaft. Jahresberichte 71. 1885–86. 8vo. *Emden* 1887. The Society.

Frankfurt-am-Main:—Senckenbergische Naturforschende Gesellschaft. Bericht. 1887. 8vo. *Frankfurt.* The Society.

Görlitz:—Naturforschende Gesellschaft. Abhandlungen. Band XIX. 8vo. *Görlitz* 1887. The Society.

Liverpool:—Geological Society. Proceedings. Vol. V. Part 3. 8vo. *Liverpool* 1887. The Society.

London:—Anthropological Institute. Journal. Vol. XVI. No. 4. Vol. XVII. Nos. 1–2. 8vo. *London* 1887. The Institute.

Clinical Society. Transactions. Vol. XX. 8vo. *London* 1887. The Society.

East India Association:—Journal. Vol. XIX. Nos. 5–7. 8vo. *London* 1887. The Association.

Entomological Society. Transactions. 1887. Parts 2–3. 8vo. *London.* The Society.

Geological Society. Quarterly Journal. Vol. XLIII. No. 171. 8vo. *London* 1887. The Society.

Marine Biological Association of the United Kingdom. Journal. No. 1. 8vo. *London* 1887. The Association.

Mathematical Society. Proceedings. Nos. 287–300. 8vo. *London* [1887]. The Society.

Mineralogical Society. Mineralogical Magazine and Journal of the Mineralogical Society. Vol. VII. No. 34. *London* 1887. The Society.

Transactions (*continued*).

Royal Microscopical Society. Journal. 1887. Parts 3–5. 8vo. *London.* The Society.

Royal Statistical Society. Journal. Vol. L. Parts 2–3. 8vo. *London* 1887. The Society.

Zoological Society. Proceedings. 1887. Parts 1–3. 8vo. *London.* The Society.

Montreal:—Royal Society of Canada. Proceedings and Transactions. Vol. IV. 4to. *Montreal* 1887. The Society.

Paris:—Société de Géographie. Bulletin. Sér. 7. Tome VIII. Trim. 1–2. 8vo. *Paris* 1887. The Society.

Toulouse:—Académie des Sciences. Mémoires. Sér. 8. Tome VIII. 8vo. *Toulouse* 1886. The Academy.

Upsala:—Universitet. Årsskrift. 1886. 8vo. *Upsala* 1886. The University.

Observations and Reports.

Batavia:—Magnetical and Meteorological Observatory. Regenwaarnemingen in Nederlandsch-Indië. Jaargang VII. 8vo. *Batavia* 1886. The Observatory.

Berlin:—Sternwarte. Berliner Astronomisches Jahrbuch für 1889. 8vo. *Berlin* 1887. The Observatory.

Bombay:—Observatory. Magnetical and Meteorological Observations. 1885. 4to. *Bombay* 1887. The Observatory.

Boston:—Blue Hill Meteorological Observatory. Results of Observations. 1886. 4to. *Boston* 1887. The Observatory.

Cadiz:—Observatorio Real de Marina de la Ciudad de San Fernando. Almanaque Náutico. 1888–89. 8vo. *Madrid* 1886–87. The Observatory.

Cordova:—Observatorio Nacional Argentino. Resultados. Vol. VI. 4to. *Buenos Aires* 1887. The Observatory.

Geneva:—Observatoire. Résumé Météorologique pour Genève et le Grand St. Bernard. Année 1886. 8vo. *Genève* 1887. The Observatory.

London:—Army Medical Department. Report. 1885. 8vo. *London* 1887. The Department.

Madison:—Washburn Observatory. Publications. Vol. V. 8vo. *Madison* 1887. The Observatory.

Madrid:—Comisión del Mapa Geológico de España. Boletin. Tomo XII. Cuaderno 2. 8vo. *Madrid* 1885. The Commission.

Oxford:—Radcliffe Observatory. Observations. 1884. Vol. XLII. 8vo. *Oxford* 1887. The Observatory.

Journals.

American Journal of Philology. Vol. VIII. No. 2. 8vo. *Baltimore* 1887. The Editor.

Annales des Mines. Sér. 8. Tome X. Livr. 6. Tome XI. Livr. 1–2. 8vo. *Paris* 1886–87. L'École des Mines.

Anthony's Photographic Bulletin. Vol. XVIII. Nos. 10–15. 8vo. *New York* 1887. The Editor.

Archives Néerlandaises des Sciences Exactes et Naturelles. Tome XXI. Livr. 5. Tome XXII. Livr. 1. 8vo. *Harlem* 1887. Société Hollandaise des Sciences.

Asclepiad (The) Vol. IV. No. 15. 8vo. *London* 1887. Dr. Richardson, F.R.S.

Astronomische Nachrichten. Band CXVII. 4to. *Keil* 1887. The Editor.

Canadian Record of Science. Vol. II. Nos. 7–8. 8vo. *Montreal* 1887. Natural History Society, Montreal.

Naturalist (The) 1887. Nos. 142–148. 8vo. *London.* The Editors.

Naturalists' Monthly (The) Vol. I. No. 1–3. 4to. *London* 1887. The Editor.

Scientific News. Vol. I. Nos. 5–9. 4to. *London* 1887. The Editor.

November 24, 1887.

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

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council nominated for election was read as follows:—

President.—Professor George Gabriel Stokes, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.—{ Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.—Sir William Bowman, Bart., M.D.; Henry Bowman Brady, F.L.S., F.G.S.; Professor Arthur Cayley, D.C.L., LL.D.; W. T. Thiselton Dyer, M.A.; Professor David Ferrier, M.A., M.D.; Edward Frankland, D.C.L.; Arthur Gamgee, M.D.; Professor Joseph Henry Gilbert, M.A.; Professor John W. Judd, P.G.S.; Professor Herbert McLeod, F.I.C.; William Pole, Mus. Doc.; William Henry Preece, M.I.C.E.; Admiral Sir George Henry Richards, K.C.B.; Professor Arthur William Rücker, M.A.; the Earl of Rosse, D.C.L., LL.D.; Sir Bernhard Samuelson, Bart., M.I.C.E.

The Rev. Octavius Pickard-Cambridge was admitted into the Society.

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

The following Papers were read:—

I. "On the Classification of the Fossil Animals commonly named Dinosauria." By H. G. SEELEY, F.R.S., Professor of Geography in King's College, London. Received August 31, 1887.

Three classifications of the Dinosauria have been proposed, which differ from each other in the principles on which their authors proposed to make the divisions.

First in time is Professor Cope's classification ('Philadelphia, Acad.

Nat. Sci. Proc.,' November 13th, 1866, and December 31st, 1867; 'Amer. Phil. Soc. Trans.,' vol. 14, Part I). He relied upon the characters of the tarsus and the ilium; and on their varied condition divided Dinosaurs into three orders named Orthopoda, Goniopoda, and Symphopoda. In the *Orthopoda*, the generic types associated are Scelidosaurus, Hylæosaurus, Iguanodon, and Hadrosaurus. And in this group the relations of the tibia and fibula are compared to those of modern Lizards, the proximal tarsals being distinct from each other and from the tibia. The ilium has a narrowed anterior prolongation.

The *Goniopoda* is so named from the abrupt flexure of the tarsus in the middle, which prevented the foot being extended in a line with the leg, so that the animals are plantigrade. The astragalus is distinct from the tibia, but embraces its distal end. The anterior portion of the ilium is dilated and plate-like. The genera in this group comprise Megalosaurus, Lælaps, Cœlosaurus, &c.

The *Symphopoda* comprises animals having the first series of tarsal bones confluent with each other and with the tibia. The anterior part of the ilium is dilated and plate-like. The type genera are Ornithotarsus and Compsognathus.

Professor Huxley rejected Professor Cope's groups because he considered that the relations of the tarsal bones to the tibia and fibula, which were supposed to characterise the Goniopoda, are also found in the Orthopoda. I am not concerned to inquire how far this criticism invalidates Cope's nomenclature, which does not rest wholly upon tarsal characters for definition; but it may be remarked that Professor Marsh subsequently obtained specimens which proved that there are many Dinosaurs in which the astragalus does not embrace the tibia. In place of Cope's three orders Professor Huxley offered a classification founded upon characters of the teeth, mandible, ilium, femur, and the absence or presence of dermal armour. He divided the order Dinosauria into three groups or families, named *Megalosauridæ*, *Scelidosauridæ*, and *Iguanodontidæ*. And it was further proposed to unite these families with Compsognathus into an order, Ornithoscelida ('Geol. Soc. Quart. Journ.,' vol. 26, February, 1870). The characters used for its definition are different from those relied upon by Cope. The Megalosauridæ is co-extensive with the Goniopoda. The Orthopoda is subdivided, chiefly on details of tooth character and the presence of dermal armour in the Scelidosauridæ, and its supposed absence in the Iguanodontidæ; but the grounds for the division became less evident when Mr. Hulke found dermal armour well developed in his *Iguanodon Seelyi* ('Geol. Soc. Quart. Journ.,' vol. 38, p. 144, May, 1882).

Subsequently Professor Marsh, in a series of memoirs dating from 1878 to 1884, proposed to divide the Dinosauria into four orders and three sub-orders. The characters used in the classification are drawn from all parts of the skeleton. The chief orders are the *Sauropoda*, comprising the allies of Cetiosaurus; the *Stegosauria*, which includes the allies of Scelidosaurus; the *Ornithopoda*, formed for the allies of Iguanodon; and the *Theropoda*, which includes genera related to Megalosaurus. The sub-orders grouped under the Theropoda are named from their typical genera *Cœluria* and *Compsognatha*. The chief difference of Marsh's system from that of Huxley is that he separated the allies of Cetiosaurus from the Iguanodontidæ to form the type of a primary division of the group, as I had suggested ('Geol. Soc. Quart. Journ.,' vol. 30, 1874, p. 690), and named it Sauropoda. Otherwise the Theropoda is identical with the Megalosauridæ; the Ornithopoda is the Iguanodontidæ similarly re-named; while the Stegosauria is the Scelidosauridæ of Huxley, enlarged like the other groups by Professor Marsh's admirable discoveries, and re-named.

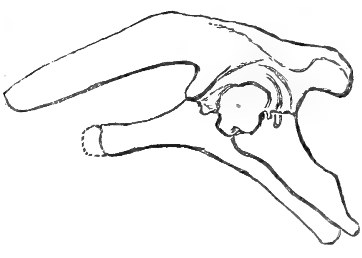
The characters on which these animals should be classified are, I submit, those which pervade the several parts of the skeleton, and exhibit some diversity among the associated animal types. The pelvis is perhaps more typical of these animals than any other part of the skeleton, and should be a prime element in classification. The presence or absence of the pneumatic condition of the vertebræ is an important structural difference. Differences in the construction of the base of the skull are indicative of affinities. The presence or absence of armour is less important, since it may show all grades of development from the perfect shield of Polacanthus to small granules in the skin; and the condition of the tarsus seems to me likely to be influenced by the habits of life of the animals. Yet the more general of these characters are morphologically preferable to slight differences in dental character, or digitigrade or plantigrade progression, or number of digits, or relative size of limbs. Many of the characters hitherto regarded as ordinal seem to me rather of a nature to distinguish families.

The ilium at first sight has the aspect of a distinctive character of the whole group, and has been regarded as Avian, because it extends both in front of the acetabulum and behind it. This character is common to birds; but it is also shared by the Ornithosauria, and to some extent by the Anomodontia. Hence this condition of the ilium does not necessarily imply that the Dinosauria is a homogeneous group. Professor Cope pointed out two distinct types of ilium which he regarded as ordinal. First, there is the ilium which is prolonged forward as a more or less narrow process which is typically seen in Iguanodon and less typically in Scelidosaurus. Secondly, there is the ilium which has its anterior process developed into a vertical plate. The bone varies a little in shape in every genus, but I see no reason to doubt that these two types of iliac bones are available for purposes of classification.

The pubes also present two types. First there are genera in which the bones are directed anteriorly and meet by a median symphysis, and have no posterior extension except for the proximal symphysis with the ischium. This type is represented by Cetiosaurus, Ornithopsis, Megalosaurus, and many genera figured by Professor Marsh. The second form of pubis has one limb which is directed backward parallel to the ischium, and another limb directed forward. It is typically seen in Omosaurus and in Iguanodon. There are many variations in stoutness and details of form of the bones, but so far as I am aware these two plans comprise all the Dinosaurian genera.

So far as can be ascertained by comparison of figures and specimens,

Stegosauria.

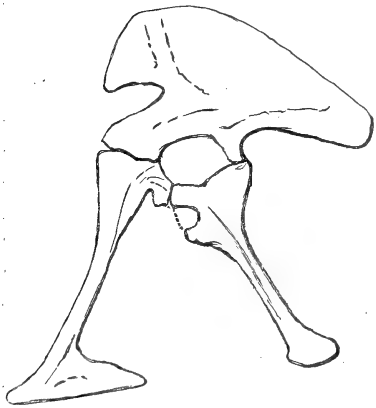


Stegosaurus.

Ornithopoda.

Camptonotus.

Theropoda.



Allosaurus.

Sauropoda.

Morosaurus.

there is no important difference of plan in the pelvis between the animals which have been referred to the order Stegosauria and those referred to the order Ornithopoda; and similarly, the plan of construction of the pelvis is essentially the same in the animals on which have been founded the orders Sauropoda and Theropoda. But there is as marked a difference between these two pelvic types as can be found in any part of the animal kingdom. These resemblances and differences are shown in the figures, which are copied from type genera of Professor Marsh's four orders.

The evidence concerning the penetration of air cells into the vertebræ has been less fully brought forward. But in the known genera which have been referred to the Stegosauria, the vertebræ are solid, and the like condition obtains in all the genera of Ornithopoda. The genera in Professor Marsh's list which are thus united are Stegosaurus, Diracodon, Omosaurus, Scelidosaurus, Acanthopholis, Cratæomus, Hylæosaurus, and Polacanthus, with Camptonotus, Laosaurus, Nanosaurus, Hypsilophodon, Iguanodon, Vectisaurus, Hadrosaurus, Agathaumus and Cionodon.

On the other hand, the precaudal vertebræ of Sauropoda are more or less hollow. This hollowness may amount to perfect excavation which leaves only an external investing film with a longitudinal median septum, or it may include a multitude of internal cells, or it may be limited to a pair of shallow impressed pits on the sides of the centrum. One of the characters by which Professor Marsh defines the Theropoda is: "vertebræ more or less cavernous." The animals included in the group appear to differ greatly in this condition. I have no evidence of presacral vertebræ of Megalosaurus being chambered, and the chambered condition of the caudal vertebræ rests upon a few specimens such as the types of Poikilopleuron. Professor Cope mentions that the tissue of the sacral vertebræ of Lælaps is so coarse as to resemble a mass of borings of the Teredo, but still the demonstration of the pneumatic condition has not been made. Nor is the evidence clearer with regard to Zanclodon. Professor Marsh figures deep pits in the sides of the dorsal vertebræ of Creosaurus. In Ceratosaurus, Marsh observes that all the presacral vertebræ are very hollow, and this is also true of the anterior caudals. The same condition is described in the cervical vertebræ of Labrosaurus, though the external foramina are small, while the Cœluria, if included in the order, would show a vertebral condition more perfectly pneumatic than in any of the Sauropoda. Hence, as the chambered condition of vertebræ is developed in most of the types of the group, it is possible that its absence in genera in which it is unrecorded may be due to the small size of the foramina having failed to indicate its existence, or to the air-cells having been so slightly developed that they did not penetrate the bones, as is the case with penguins among birds. But the development of the pneumatic condition is sufficiently general among Sauropoda and Theropoda, to show that these groups are united together by a character which separates them from Stegosauria and Ornithopoda. It is not possible to form an opinion as to the inference which should be drawn from this character concerning the vital organisation of the animals in which it is found. For, many of the armoured genera have the neural arch much extended vertically, in the dorsal region, showing that the lungs were greatly developed. But since the difference in height between the carapaces of flat-shelled Emydian Chelonians and Tortoises, is chiefly due to differences in the volume of the lungs, it is quite possible that considerable variations in osteological character may occur in the vertebræ, without much difference in the vital organ which produces the change. On the other hand it must be remembered that among existing animals, the pneumatic skeleton is only found in birds.

Of late years the Dinosaurian skull has become well known. Mr. J. W. Hulke, F.R.S., described the brain-case of Iguanodon in 1871 ('Geol. Soc. Quart. Journ.,' vol. 27, p. 199), and in 1874 I described the base of a cranium ('Geol. Soc. Quart. Journ.,' vol. 30, p. 690) which was named *Craterosaurus Pottonensis.* In the former the brain-case is closed in front, and the basi-sphenoid has a comparatively slight downward development, while in the latter the base of the skull is much more like that of Hatteria than it is like Iguanodon. These types include so far as the evidence goes all the forms of skull hitherto discovered. On the plan of Iguanodon are shaped the skulls of Hypsilophodon and apparently Diclonius, while the skulls of Diplodocus and Ceratosaurus have much in common with Craterosaurus in having the deep pituitary depression, the anterior part of the brain-case open, &c. The evidence concerning the skull is very imperfectly known, but, so far as it goes, points in the same direction as the other characters in indicating that there are probably only two types in the group. Any classification must necessarily be provisional until the skulls and skeletons which exist are adequately described. The considerations adduced appear, however, to show that the Dinosauria has no existence as a natural group of animals, but includes two distinct types of animal structure with technical characters in common, which show their descent from a common ancestry rather than their close affinity. These two orders of animals may be conveniently named the Ornithischia* and the Saurischia, and defined by the following characters.

Ornithischia.

In this order the ventral border of the pubic bone is divided, so that one limb is directed backward parallel to the ischium as among birds,

* "Ischia" is used by Aristotle for the pelvis.

and the other limb is directed forward. Neither of these limbs of the pubis appears to form a median symphysis. The ilium is prolonged in front of the acetabulum as a more or less slender process or bar. The vertebræ are solid, and the skeleton is not pneumatic. The basi-cranial structure is distinctive, differing from that of Crocodiles and Lizards. The body and limbs are frequently covered with scutes which may form a complete shield or be reduced so as to be unrecognisable. The digits vary from three to five.

Saurischia.

In this order the pubis is directed forward from its symphysis with the ischium, and no posterior limb of the bone is developed. Both pubis and ischium appear to meet by a median symphysis, so that the arrangement and relations of the bones are Lacertilian. The anterior prolongation of the ilium has a vertical expansion. The vertebræ are more or less pneumatic or cavernous; and in the dorsal region the neural arch is commonly elevated. The basi-cranial structure is sublacertilian. No armour has been found. The digits vary in number from three to five.

I see no ground for associating these two orders in one group, unless that group includes Birds, Crocodiles, Anomodonts, and Ornithosaurs; for differences of pelvic structure have been as persistently inherited as any condition of the vertebrate skeleton.

The classification may be summarised in the following table :—

Cope, 1866.	Huxley, 1870.	Seeley, 1874.	Marsh, 1878–84.	Cope, 1883.	Seeley, 1887.
Orders.	**Families.**	**Order.**	**Orders.**	**Orders.**	**Orders.**
Orthopoda ...	Scelidosauridæ	...	Stegosauria ...	Orthopoda ...	Ornithischia.
	Iguanodontidæ	...	Ornithopoda ...		
		Cetiosauria ...	Sauropoda ...	Opisthocœla* ...	Saurischia.
Goniopoda ..	Megalosauridæ	...	Theropoda ...	Goniopoda ...	
Symphopoda	Compsognatha	...		Hallopoda.	

* Sir Richard Owen grouped Cetiosaurus and Streptospondylus in an extinct sub-order of Crocodilia named Opisthocœla in 1859; while Megalosaurus and Iguanodon were united to form the Dinosauria in 1841. This is the earliest and most definite reference of these animals to separate ordinal groups.

II. "Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. Part III. On Parts of the Skeleton of a Mammal from Triassic Rocks of Klipfontein, Fraserberg, South Africa (*Theriodesmus phylarchus*, Seeley), illustrating the Reptilian Inheritance in the Mammalian Hand." By H. G. SEELEY, F.R.S., Professor of Geography in King's College, London. Received October 24, 1887.

(Abstract.)

The author describes a slab showing impressions of the fore-limb and some other bones of the skeleton, which indicate a plantigrade animal as large as a wolverine. Its general affinities are with flesh-eating types. The humerus approximates to that of Thylacinus. The ulna and radius at their proximal ends are like those of Lemuroids and Carnivores, but the forms of the distal articulations are different. The carpus appears to include three central bones. Part of one of the digits appears to have been lost and renewed. The animal is regarded as a primitive type which cannot be placed in any ordinal group which has been defined.

III. "Further Contributions to the Metallurgy of Bismuth." By EDWARD MATTHEY, F.S.A., F.C.S., Assoc. Roy. Sch. Mines. Communicated by Professor G. G. STOKES, P.R.S. Received October 20, 1887.

§ 3. *Bismuth: its Separation from Copper.*—In the paper upon this interesting metal, which I had the honour of bringing under the notice of the Royal Society in February last, I referred to the difficulties with which the treatment of bismuth is surrounded when associated with other metals—by any very rapid or comprehensive process.

During the conduct of my operations in the reduction of bismuth from its ores, and its subsequent refining, I have frequently found this metal to contain a small proportion of copper, an element most detrimental even in small traces, and hitherto I believe, only eliminated by a wet process, costly in practice and tedious in operation. It is necessary by such method to dissolve up the whole of the alloy and precipitate the bismuth in the usual manner—a bulky operation, and one requiring a considerable amount of time. It became therefore advisable, in order to treat cupreous bismuth rapidly and upon a commercial scale, to effect this separation, if possible, by means of a dry process.

In this I have succeeded.

Having observed, in conducting experiments with bismuth and its sulphides, that sulphide of bismuth becomes very easily impregnated with copper, I made the simple experiment of fusing the cupreous bismuth with bismuth sulphide, and found it possible by this means to remove every trace of copper, the sulphur readily combining with the metallic copper.

In this absorption a proportion of bismuth is reduced equivalent to the amount of copper taken up in the operation.

The residual bismuth and copper sulphides thus produced amount to but a small proportion in comparison with the quantity of alloy treated, and the bismuth is readily recovered by subsequent reduction and refusion.

Large quantities of alloy can be treated at one operation, and the bismuth so freed from copper is available for commercial purposes. I have found it better, when bismuth is associated with other metals, such as arsenic, antimony, lead, tellurium, &c., as well as with copper, to separate all these metals (see former papers) before attempting to remove the copper by the foregoing method.

The operation has been conducted successfully upon many thousands of pounds of similar alloy, and the following figures will show the results obtained in one case, as an example:—

Weight of cupreous bismuth treated = 314 lbs. containing 0·10 per cent. of copper, equal to approximately 0·3 lb.

	lbs.
From the operation described I obtained of bismuth free from copper	282
Of bismuth subsequently reduced and refined from the skimmings	29·9
And bismuth left in residues for further treatment with larger quantities (by determination).............	2
	313·9

	lb.
Copper from the skimmings	0·2
Copper left in residues..........................	0·1
	0·3 lb.

Thus the whole of the copper and of the bismuth, within a small fraction, is accounted for, the latter being obtained as commercially pure bismuth *and wholly free from copper.*

As the above operation shows, the first separation frees 90 per cent. of the bismuth *at once* from the copper associated with it.

It may be as well to state that I have effected complete separation with bismuth containing proportions of copper varying from one-tenth of 1 per cent. to 1 per cent. by the above process.

IV. "On the Motion of a Sphere in a Viscous Liquid." By A. B. Basset, M.A. Communicated by Lord Rayleigh, D.C.L., Sec. R.S. Received November 10, 1887.

(Abstract.)

The determination of the small oscillations and steady motion of a sphere which is immersed in a viscous liquid, and which is moving in a straight line, was first effected by Professor Stokes in his well-known memoir "On the Effect of the Internal Friction of Fluids on the Motion of Pendulums;"* and in the appendix he also determines the steady motion of a sphere which is rotating about a fixed diameter. The same subject has also been subsequently considered by Helmholtz and other German writers; but, so far as I have been able to discover, very little appears to have been effected with respect to the solution of problems in which a solid body is set in motion in a viscous liquid in any given manner, and then left to itself.

In the present paper I have endeavoured to determine the motion of a sphere which is projected vertically upwards or downwards with given velocity, and allowed to ascend or descend under the action of gravity (or any constant force), and which is surrounded by a viscous liquid of unlimited extent, which is initially at rest excepting so far as it is disturbed by the initial motion of the sphere.

In solving this problem, mathematical difficulties have compelled me to neglect the squares and products of velocities, and quantities depending thereon, which involves the assumption that the velocity of the sphere is always small throughout the motion; and I have also assumed that no slipping takes place at the surface of the sphere. The problem is thus reduced to obtaining a suitable solution of the differential equation

$$\mathrm{D}\left(\mathrm{D}-\frac{1}{\mu}\frac{d}{dt}\right)\psi,$$

where

$$\mathrm{D} = \frac{d^2}{dr^2}+\frac{\sin\theta}{r}\frac{d}{d\theta}\left(\operatorname{cosec}\theta\frac{d}{d\theta}\right).$$

ψ is Stokes's current function, and μ is the kinematic coefficient of viscosity. The required solution is obtained in the form of a definite integral by a method similar to that employed by Fourier in solving analogous problems in the conduction of heat; the resistance experienced by the sphere is then calculated, and the equation of motion written down and integrated by successive approximation on the supposition that μ is a small quantity. The values of the acceleration and velocity of the sphere to a third approximation are found to be

* 'Cambridge Phil. Soc. Trans.,' vol. 9.

$$\dot{v} = f\epsilon^{-\lambda t} - V\lambda\epsilon^{-\lambda t} - fka\sqrt{\frac{\mu}{\pi}}\{(\tfrac{1}{2}-\lambda t)\phi(t) + \sqrt{t}\}$$
$$+ fk^2a^2\mu t\epsilon^{-\lambda t}(1-\tfrac{1}{2}\lambda t),$$

$$v = \frac{f}{\lambda}(1-\epsilon^{\lambda t}) + V\epsilon^{-\lambda t} - fka\sqrt{\frac{\mu}{\pi}}\left\{\left(t+\frac{1}{2\lambda}\right)\phi(t) - \frac{\sqrt{t}}{\lambda}\right\}$$
$$+ \tfrac{1}{2}fk^2a^2\mu t^2\epsilon^{-\lambda t},$$

where

$$f = \frac{(\sigma-\rho)g}{\sigma+\frac{1}{2}\rho}, \quad k = \frac{9\rho}{a^2(2\sigma+\rho)}, \quad \lambda = k\mu, \quad \phi(t) = \int_0^t \epsilon^{-\lambda\tau}(t-\tau)^{-\frac{1}{2}}d\tau,$$

ρ being the density of the liquid, σ that of the sphere, and a its radius.

It thus appears that after a very long time has elapsed, the acceleration will vanish and the motion will become steady. The terminal velocity of the sphere is $f\lambda^{-1}$, which is seen to agree with Professor Stokes's result.

If the sphere were projected with velocity V, and compelled by means of frictionless constraint to move in a horizontal straight line, the values of the acceleration and velocity would be obtained from the preceding formulæ by expunging the terms $f\epsilon^{-\lambda t}$, $f\lambda^{-1}(1-\epsilon^{-\lambda t})$, in the expressions for $\dot{v}$ and v respectively, and then changing f into $-V\lambda$.

The preceding results can only be regarded as a somewhat rough representation of the actual motion, for (i) the square of the velocity has been neglected; (ii) no account has been taken of the possibility of hollow spaces being formed in the liquid; (iii) if the velocity of the sphere became large, the amount of heat developed would be sufficient to vaporise the liquid in the immediate neighbourhood of the sphere, and the circumstances of the problem would be materially changed.

In the latter part of the paper I have considered the problem of a sphere, surrounded by a viscous liquid, which is set in rotation with given angular velocity, Ω, about a fixed diameter, and similar results are obtained. To a first approximation the angular velocity is equal to $\Omega\epsilon^{-\lambda t}$, where λ is a positive constant, which shows that the motion ultimately dies away.

V. "On the Direct Application of First Principles in the Theory of Partial Differential Equations." By J. LARMOR, M.A., Fellow of St. John's College, Cambridge. Communicated by Lord RAYLEIGH, D.C.L., Sec. R.S. Received November 8, 1887.

(Abstract.)

If an equation involving total differentials of any number of variables can be expressed in the form—

$$\delta u + \sigma \, \delta v = 0,$$

where u, v are any functions of the variables, then the only *single* integral algebraic relations that are consistent with it are included under the form—

$$u = \phi(v).$$

When the form of σ is assigned, the functional symbol ϕ is to be chosen, if possible, so as to agree with that form; and if this is not possible, then the equation has no integral expressible as a *single* relation. This statement holds because the equation expresses a particular case of the proposition that if $\delta u = 0$, then $\delta v = 0$, and conversely, *i.e.*, that u remains constant (does not vary) when v is constant, and only then, whatever be the particular values assigned to the variables: but this is simply the definition of the algebraic idea of functionality.

If, however, σ involve differentials, the alternative $\delta u = 0$ when $\sigma = 0$ may lead to integrals of a new type.

In the same way, an equation of the form—

$$\delta u + \sigma_1 \, \delta v + \sigma_2 \, \delta w = 0,$$

must have all its *single* integrals included under the form—

$$u = \phi(v, w),$$

where the form of ϕ is to be chosen so as to agree with the expressions for σ_1, σ_2, when these are assigned.

When no *single* integral exists, equations of this type may be satisfied by two simultaneous integral relations, one of which may be arbitrarily assumed, as originally pointed out by Monge. This kind of exception, however, need not trouble us when partial differential coefficients are concerned; for these implicitly assume the existence of a single relation connecting the dependent variable with the independent ones.

Traces of this idea are to be found throughout the writings of Boole

—and of Monge long previously. In this paper it is applied, first to the non-analytical exposition of the differential criteria of algebraic functionality given by Jacobi, and then to the discussion in a similar manner of the theory of partial differential equations of the first and second order, particularly those named after Lagrange, Monge, and Ampère.

VI. "On the Power of Contractility exhibited by the Protoplasm of certain Plant Cells." (Preliminary Communication.) By WALTER GARDINER, M.A., Fellow of Clare College, Cambridge, Demonstrator of Botany in the University. Communicated by Prof. M. FOSTER, Sec. R.S. Received November 21, 1887.

In a former communication ('Roy. Soc. Proc.,' No. 240, 1886), some account was given of the principal changes which take place in the gland cells and stalk cells of *Drosera dichotoma* during secretion. The present paper deals with certain experiments and observations which were undertaken in order to attempt to ascertain by what mechanism the bending of the tentacles is made possible in *Drosera*, and what changes occur in the tentacle cells.

During actual movement no obvious histological changes can be detected in the cells of the bending portion, but when the tentacle has become well inflected, it becomes apparent that the cells of the convex side become more, and those of the concave less turgid than before. Some time after stimulation, and when the period of aggregation has set in, it can be observed that the cells of the convex side are less aggregated than those of the concave. Having ascertained that of the dye solutions, eosin, and of salts, the salts of ammonia, are readily sucked up into the tissue, it was further noticed that in stimulated tentacles the cells of the convex side readily allow the solutions to penetrate, while those of the concave are only penetrated with great difficulty. Thus in the case of a stimulated tentacle treated with eosin, the convex cells are stained long before the concave, and with ammonic carbonate the tannin of the convex cells may be precipitated while the concave cells remain normal, or the convex cells may even be killed while the concave cells remain alive. Thus after stimulation certain changes have occurred in the concave cells of the bending portion, and one result of this change is an increased impenetrability of the primordial utricle. In my former paper I have shown that the tentacle cells of *Drosera* are very sensitive to contact, for if the gland cells be slightly crushed, all movement of the stalk cells ceases for a time, and the spindle-shaped rhabdoid contracts and tends to become spherical. Bearing in mind also the very pronounced inflection which is occasioned by the stimulus of contact or food, by electrical stimulus or, as Darwin has shown, by the stimulus of temperature, one is led to ask whether these phenomena are not associated with true contractility, and whether the increased impenetrability of the protoplasm of the concave cells is not occasioned by a definite contraction of the primordial utricle and a consequent decrease in the size of the molecular pores.

Experiments were then made upon the pulvinus of *Mimosa pudica.* Small pieces of stem (bearing leaves) were cut under a watery solution of eosin, and the pulvini were maintained in a state of stimulation. When the eosin had sufficiently penetrated, transverse and longitudinal sections of the pulvinus were made and examined. It was then seen that the dye had readily penetrated into and stained the protoplasm of the outer cells of the convex side of the pulvinus, while on the concave side no staining whatever, of that tract of cells situated towards the more external portion, which especially play an active part in movement, had taken place. The more indifferent cells immediately surrounding the vascular bundle also show some contrast in coloration, for in the upper half this tissue remains unstained, while in the lower half some staining occurs. Thus by the process of staining the seat of the especially irritable tissue was clearly brought into view. The author now commenced electrical experiments with the pulvini. Two small pins (which were found not to injure the tissue to any appreciable extent) were inserted into the irritable tissue —one at each end, and fine wires from these pins communicated to the various electrical apparatus as required. When suitably stimulated with either a constant current, an induction shock, or a tetanising shock, the leaf fell immediately contact was made. With the single induction shock the breaking shock was found to be a stronger stimulus than the making. A small piece of stem with the pulvinus attached—the lamina and a portion of the petiole of the leaf having been previously removed—was attached to a lever which wrote upon a revolving drum. On throwing in the electrical stimulus the pulvinus contracted and a curve was obtained. The pulvinus was then turned upside down and, after recovery, was again stimulated and a second curve obtained. In both instances the pulvinus raised a weight greater than that of the leaf and leaf stalk. These experiments for the most part only confirmed those of Cohn and Kabsch, except that they were carried out in further detail; but one new and important observation was made, viz., that under the influence of a feeble tetanising current the period of recovery of the pulvinus could be materially shortened, and the leaf could be induced to assume the position before stimulation in less time than it would have taken under ordinary circumstances. The wonderful delicacy with which the irritable cells of the pulvinus at once reply to stimulation, the fact that in their reaction to the stimulus of electricity they obey the same laws as animal muscle, and, like certain muscles, may also be relaxed by a feeble tetanising current, go far to suggest that in dealing with the movements of the pulvinus of *Mimosa* we have essentially to do with the phenomenon of contractility.

Although the foregoing results may be said to favour the idea that in irritable organs, movements are brought about by a definite contraction of the protoplasm of the cells of the irritable side, yet the author felt that the matter could only be set at rest by still further strengthening the evidence, and if it were possible, by the actual observation of a cell contracting under the influence of electrical or other stimulation. He therefore turned his attention to the simple filamentous Algæ, and among them to an organism which he believed would be peculiarly sensitive to stimulation, viz., *Mesocarpus pleurocarpus.* The filaments consisting of rows of cells were first experimented upon, electrically. A single induction shock of moderate strength was found to cause a splitting apart of the previously united transverse walls of the contiguous cells along the middle lamellæ. In each cell, the two end walls now project inwards towards the centre of the cell in a concave manner, so that between each pair of cells of the filament there arise a series of double convex lenticular spaces. The rupture does not extend to the free surface.

With a stronger shock so much contraction is produced that the cells actually fly apart and a complete rupture is effected. The end walls of each cell are now observed to be slightly convex instead of concave. This is a result of the contraction of the freed edges of the external walls, which in consequence of the rupture no longer maintain their cylindrical form. Each cell now resembles a cylinder with its two ends somewhat convex, and its sides very slightly contracted in the immediate neighbourhood of their lines of union with the ends. As in *Mimosa* the breaking is a stronger stimulus than the making shock. Similar contraction is obtained with the tetanising shock and with the constant current.

Sudden illumination, sudden rise of temperature (45—50° C.), and the stimulus of certain poisons, bring about the contraction and breaking apart in the most marked manner. Of the poisons, camphor, quinine, strychnine, physostigmine and strong alcohol were found to be exceedingly powerful, with very dilute alcohol no obvious change occurred. The strongest plasmolysing reagents did not bring about the rupture of the cells, but only the partial separation of the end wall, and if the cells are killed by boiling water, by iodine, or by very dilute chromic acid (0·25 per cent.), similar results follow. With 1 per cent. osmic acid or 1 per cent. chromic acid the cells may be killed and fixed with little or no contraction.

The results with plasmolysis entirely agree with those previously obtained in the case of *Drosera* (*loc. cit.*): the protoplasm seeming to be partially paralysed, the whole of its energy apparently expended in endeavouring to protect itself from the abnormally rapid withdrawal of water. The passive shrinking produced by strong dehydrating reagents is essentially different from the active contraction arising from normal stimulation, and one may well inquire whether the effects produced by plasmolysis at all tally with those vital processes which actually take place under ordinary circumstances in plant cells.

The results obtained with *Mesocarpus* demonstrate that we have here a plant cell which reacts in a most powerful manner to the stimulus of temperature, of light, of electricity, and of poisons, and that this reaction, which may be watched under the microscope, is attended by a diminution in size. In the opinion of the author such a series of reactions can only point to one property of the protoplasm, viz., that of contractility, and taking into consideration the whole of the observations, there appears to be no doubt that the protoplasm of plant cells, like that of animal cells, is capable of active contraction. The author believes that in all irritable organs the movements are brought about in consequence of a definite contraction of the protoplasm of the irritable cells, and that during such contraction some of the cell sap escapes to the exterior. At the same time the elastic cell wall contracts *pari passu* with the protoplasm. The author has already drawn attention to the intimate connexion between the protoplasm and the wall (' Phil. Trans.,' 1883, Part 3), and has shown that even after pronounced plasmolysis, the ectoplasm of the primordial utricle is always connected to the cell membrane by very numerous and delicate strands of protoplasm. The protoplasm may be withdrawn from the wall by a very strong electric shock, but the normal effect of a moderate stimulus is to cause the protoplasm to contract, and in certain cases pull upon its wall, while in very turgid cells where the cell wall is in a state of great tension, the wall for the most part simply contracts upon the protoplasm. The escape of liquid from the interior of the cell is regarded as being due to filtration under pressure. The author is unable to uphold Pfeffer's theory that the sudden abolition of turgidity is dependent upon the destruction of a certain quantity of an osmotically active substance. In his opinion there is in every cell a sufficient quantity of osmotically active substance to ensure turgidity, but the increase or decrease of turgidity essentially depends on the contraction or relaxation of the primordial utricle. His experiments all tend to show that it is the ectoplasm which mainly determines the state of turgidity of the cells. Thus in the tentacle cells of *Drosera* the endoplasm may actually be withdrawn from the ectoplasm by the lengthy action of strong solutions of magnesium sulphate, and although it is almost entirely collected around the nucleus at the centre of the cell, the latter still remains turgid.

The author is also of opinion that de Vries' view, that the turgidity of the cell is mainly dependent on the presence of certain osmotically active substances in the sap, of an acid nature, requires some further qualification, for his own results agree rather with those of Schwartz, since he finds that turgid cells may possess either an acid or an alkaline sap. Thus, in *Drosera* itself, the cells of the tentacles have an acid, and those of the petals of the flower an alkaline reaction.

Finally, the author believes that the property of contractility, which he claims to have established for the irritable cells of *Drosera* and *Mimosa*, and for the less specialised cells of *Mesocarpus*, is a property which is possessed in a greater or less degree by all the actively living cells which constitute the tissues of plants. The important bearing of these results on all phenomena of movement and growth is sufficiently obvious. The author hopes to deal with the matter in fuller detail in a future paper.

Presents, November 24, 1887.

Transactions.

Berlin:—Physikalische Gesellschaft. Verhandlungen. 1886. 8vo. *Berlin* 1887. The Society.

Bern:—Naturforschende Gesellschaft. Mittheilungen. 1886. 8vo. *Bern* 1887. The Society.

Birmingham:—Mason Science College. Calendar. 1887–88. 8vo. *Birmingham* 1887. The College.

Bombay:—Royal Asiatic Society (Bombay Branch). Journal. Vol. XVIII. No. 45. 8vo. *Bombay* 1887. The Society.

Boston:—American Academy of Arts and Sciences. Proceedings. Vol. XIV. Parts 1–2. 8vo. *Boston* 1887; Memoirs. Vol. XV. Part 4. No. 5. 4to. *Cambridge* 1886. The Academy.

Breslau:—Schlesische Gesellschaft für vaterländische Cultur. Jahresbericht LXIV., with Ergänzungsheft. 8vo. *Breslau* 1887. The Society.

Brighton:—Brighton and Sussex Natural History and Philosophical Society. Annual Report. 1887. 8vo. *Brighton.* The Society.

Brussels:—Académie Royale de Médecine de Belgique. Mémoires Couronnés et autres Mémoires. Tome VIII. Fasc. 2–4. 8vo. *Bruxelles* 1887. The Academy.

Académie Royale des Sciences, des Lettres et des Beaux Arts de Belgique. Mémoires Couronnés et autres Mémoires. Tomes XXXVII–XXXIX. 8vo. *Bruxelles* 1886; Mémoires. Tome XLVI. 4to. *Bruxelles* 1886; Mémoires Couronnés et Mémoires

Transactions (*continued*).

des Savants Étrangers. Tomes XLVII–XLVIII. 4to. *Bruxelles* 1886; Notices Biographiques et Bibliographiques concernant les Membres, les Correspondants et les Associés. 1886. 12mo. *Bruxelles* 1887; Biographie Nationale. Tome VIII. Fasc. 3. Tome IX. Fasc. 1–2. 8vo. *Bruxelles* 1885–87; Catalogue des Livres de la Bibliothèque. 8vo. *Bruxelles* 1881–87.
The Academy.

Calcutta:—Asiatic Society of Bengal. Journal. Vol. LVI. Part 1. No. 1. 8vo. *Calcutta* 1887; Proceedings. 1887. Nos. 2–5. 8vo. *Calcutta* 1887. The Society.

London:—Institution of Civil Engineers. Minutes of Proceedings. Vols. LXXXIX–XC. 8vo. *London* 1887; Brief Subject-Index. Vols. LIX–XC. 8vo. *London* [1887]; Charter, List of Members, &c. 8vo. *London* 1887. The Institution.

Institution of Mechanical Engineers. Proceedings. 1887. No. 2. 8vo. *London.* The Institution.

Institution of Naval Architects. Transactions. Vol. XXVIII. 4to. *London* 1887. The Institution.

Quekett Microscopical Club. Journal. Ser. 2. Vol. III. No. 19. 8vo. *London* 1887. The Club.

Society of Antiquaries. Proceedings. Vol. XI. No. 3. 8vo. *London* 1887. The Society.

Society of Biblical Archæology. Transactions. Vol. VIII. Part 3. Vol. IX. Part 1. 8vo. *London* 1885, 1887.
The Society.

Victoria Institute. Journal of the Transactions. Vol. XXI. No. 82. 8vo. *London* 1887. The Institute.

Lyons:—Société d'Anthropologie. Bulletin. Tome V. 8vo. *Lyon* 1887. The Society.

Magdeburg:—Naturwissenschaftlicher Verein. Jahresbericht und Abhandlungen. 1886. 8vo. *Magdeburg* 1887. The Verein.

Manchester:—Geological Society. Transactions. Vol. XIX. Parts 8–10. 8vo. *Manchester* 1887. The Society.

Melbourne:—Royal Society of Victoria. Transactions and Proceedings. Vols. XXII–XXIII. 8vo. *Melbourne* 1886–87.
The Society.

Munich:—Königl. Bayer. Akademie der Wissenschaften. Abhandlungen (Math.-Phys. Classe). Band XV. Abth. 3. Band XVI. Abth. 1. 4to. *München* 1886–87; Sitzungsberichte (Math.-Phys. Classe). 1886. Hefte 1–3. 1887. Heft 1. 8vo. *München;* Inhaltsverzeichniss. Jahrg. 1871–1885. 8vo. *München* 1886; Sitzungsberichte (Philos.-Philol. und Hist. Classe). 1886. Hefte 1–4. 1887. Heft 1–2. 8vo. *München;* Inhaltsverzeichniss. Jahrg. 1871–1885. 8vo. *München* 1886; Gedächtnissrede auf

Transactions (*continued*).

Joseph von Fraunhofer. 4to. *München* 1887; Gedächtnissrede auf Leopold von Ranke. 4to. *München* 1887; Gedächtnissrede auf Carl Theodor von Siebold. 4to. *München* 1886.

The Academy.

Anderson (R. J.) Volume of Papers (chiefly excerpts) on Anatomy and Physiology. 8vo. *Dublin*, &c., 1878–1886.

The Author.

Baker (J. G.), F.R.S. Handbook of the Fern Allies. 8vo. *London* 1887. The Author.

Bristowe (J. S.), F.R.S. A Treatise on the Theory and Practice of Medicine. 6th edition. 8vo. *London* 1887. The Author.

Browne (W. H.) Firework Accidents: their Cause and Prevention. Sm. 8vo. *Hull* 1884. The Author.

Buist (J. B.) Vaccinia and Variola, a Study of their Life-history. Sm. 8vo. *London* 1887. The Author.

Egleston (T.) The Metallurgy of Silver, Gold and Mercury in the United States. Vol. I. Silver. 8vo. *London* 1887.

The Editor of "Engineering."

Henry (J.) Scientific Writings. 2 vols. Large 8vo. *Washington* 1886. The Smithsonian Institution.

Jones (T. R.), F.R.S. Notes on some Silurian Ostracoda from Gothland. 8vo. *Stockholm* 1887; Note on *Nummulites elegans*, Sowerby, and other English Nummulites. 8vo. [*London*] 1887; Notes on the Palæozoic Bivalved Entomostraca.—On some Silurian Genera and Species. 8vo. [*London*] 1887.

The Author.

Lewis (H. Carvill) The Alleged Physical Phenomena of Spiritualism. 8vo. [*London*] 1887. The Author.

Murray (R. A. F.) Victoria. Geology and Physical Geography. 8vo. *Melbourne* 1887. The Department of Mines.

Pitt-Rivers (Lieut.-Gen.), F.R.S. Excavations in Cranborne Chase, near Rushmore. Vol. I. 4to. 1887. The Author.

Russell (H. C.), F.R.S. Notes upon the History of Floods in the River Darling. 8vo. *Sydney* 1887; Notes upon Floods in Lake George. 8vo. *Sydney* 1887. The Author.

Scott (R. H.), F.R.S. Weather Charts and Storm Warnings. Sm. 8vo. *London* 1887. The Author.

Smith (F.) A Manual of Veterinary Hygiene. Sm. 8vo. *London* 1887. The Author.

Smith (J. B.) A Treatise upon Cable or Rope Traction as applied to the Working of Street and other Railways. 4to. *London* 1887.

The Editor of "Engineering."

Stokes (G. G.), Pres. R.S. On the Beneficial Effects of Light. Burnett Lectures. Third Course. Sm. 8vo. *London* 1887.
The Author.

Symons (G. J.), F.R.S. British Rainfall, 1886. 8vo. *London* 1887.
The Author.

Tidy (C. M.) Handbook of Modern Chemistry. Second edition. 8vo. *London* 1887.
The Author.

November 30, 1887.

ANNIVERSARY MEETING.

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

The Report of the Auditors of the Treasurer's Accounts on the part of the Council was presented, by which it appears that the total receipts during the past year, including balances carried from the preceding year, amount to £7,691 13*s.* 6½*d.* on the General Account, and £14,673 18*s.* 3*d.* on account of Trust Funds, and that the total expenditure in the same period, including purchase of stock, amounts to £7,441 7*s.* 2*d.* on the General Account, and £12,937 7*s.* 4*d.* on account of Trust Funds, leaving a balance on the General Account of £232 0*s.* 5*d.* at the Bankers', and £18 5*s.* 11½*d.* in the hands of the Treasurer, and on account of Trust Funds a balance at the Bankers' of £1,736 10*s.* 11*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists :—

Fellows deceased since the last Anniversary (Nov. 30, 1886).

On the Home List.

Baxendell, Joseph, F.R.A.S.
Beresford-Hope, Right Hon. Alexander James Beresford, LL.D., F.S.A.
Denham, Sir Henry Mangles, Admiral.
Elliot, Sir Walter, K.C.S.I.
Fox, Wilson, M.D.
Gaskin, Rev. Thomas, M.A.
Haast, Sir John Francis Julius von, K.C.M.G.
Hunt, Robert.
Hymers, Rev. John, D.D.
Iddesleigh, Stafford Henry Northcote, Earl of, G.C.B.
Phillips, John Arthur, F.G.S.
Quain, Richard, F.R.C.S.
Smythe, William James, General, R.A.
Whatman, James, M.A.
Whitworth, Sir Joseph, Bart., LL.D.

On the Foreign List.

Kirchhoff, Gustav Robert.

Change of Name and Title.

Sclater-Booth, The Right Hon. George, *to* Lord Basing of Basing-Byflete.

Fellows elected since the last Anniversary.

Buchanan, John Young, M.A.
Cash, John Theodore, M.D.
Douglass, Sir James Nicholas, M.I.C.E.
Ewing, Prof. James Alfred, B.Sc.
Forbes, Professor George, M.A.
Gowers, William Richard, M.D.
Halsbury, Hardinge Stanley Giffard, Lord, M.A.
Kennedy, Professor Alexander B. W., M.I.C.E.
King, George, M.B.
Kirk, Sir John, M.D.
Lodge, Professor Oliver Joseph, D.Sc.
Milne, Professor John, F.G.S.
Pickard-Cambridge, Rev. Octavius, M.A.
Snelus, George James, F.C.S.
Walsingham, Thomas, Lord.
Whitaker, William, B.A.

The President then addressed the Society as follows:—

During the past year death has removed from us fifteen of our Fellows and one Foreign Member. It is remarkable that no less than six of these had reached the age which the Psalmist takes for the extreme duration of human life, while the average of the whole number exceeds seventy-five years. Within two months after our last anniversary, Sir Joseph Whitworth died at the age of eighty-four. Starting from an humble beginning, he attained through his talent and steady application a commanding position among constructors of machinery and heavy ordnance, and the truth of surface and accuracy of dimensions of what came from his workshop are probably unrivalled. Sir Walter Elliot, who was still older, combined a high official position in India with the pursuit of natural history, and was the author of several papers in scientific serials. John Hymers and Thomas Gaskin were mathematicians well known to Cambridge men of some standing, and were both elected Fellows of our Society nearly half a century ago. The former was the author of various mathematical text-books, which for a long time were those chiefly used in their respective subjects by Cambridge students for mathematical honours. The latter, once a colleague of my own in a mathematical honour examination, was famed for his skill in the solution of problems, though he has not left much behind him in the way of mathematical writings, beyond a book containing the solution of a variety of problems. In Robert Hunt we have lost an aged Fellow whose name is well known in connexion with the study of the action of light in producing chemical changes, and on vegetation. In Joseph Baxendell we had a man who during a long life was a diligent observer of astronomical and meteorological phenomena. John Arthur Phillips, a geologist who attended more particularly to the chemical origin of mineralogical and geological phenomena, was the author of several papers, some of which appear in our own Proceedings. It is not long since Sir Julius von Haast was among us, apparently in full vigour, having come to England in connexion with the Colonial Exhibition, and now this distinguished geologist and naturalist is no more. The Earl of Iddesleigh was suddenly carried off in the midst of the duties belonging to an important office in the State, while Beresford-Hope has succumbed to an illness of some duration. These two joined us under the statute which enables the Council to recommend to the Society for election, in addition to the fifteen who are selected in the ordinary way, and nearly always on account of their scientific claims, persons who are members of Her Majesty's Most Honourable Privy Council, and whose ability is thus attested, though they are not usually men of science. From the list of foreign Members, one name has disappeared which has become a household word among the physicists of all civilized nations. The name of Kirchhoff will ever be remembered as that of the introducer, conjointly with Bunsen, of spectral analysis into the regular work of the chemical laboratory, a step which has been so fertile in results. To him too we owe the reference of the dark lines of the solar spectrum to the absorption of portions of light coming from deeper portions of the sun by the vapours of substances which in the condition of incandescent vapour themselves emit bright lines in corresponding positions, and to him therefore we are indebted for the detection of chemical elements in the sun and stars, though partial anticipations of these discoveries had been made by others. The fertility of these researches, and the attention which they consequently excited, should not make us forget the many important investigations in mathematical physics of which Kirchhoff was the author.

The present year is memorable as the Jubilee of the reign of Her Most Gracious Majesty our beloved Sovereign, and the Patron of our Society. An address of congratulation on this auspicious event was prepared by the Council, and was graciously received by Her Majesty in Windsor Castle at the hands of your President, who was accompanied on that occasion by the senior Secretary.

It happens that this same year is also the Jubilee of the Electric Telegraph, if we date from the first construction of a telegraph on an actually working scale, as distinguished from preparatory experiments made only in the laboratory. The Jubilee was duly celebrated by the Society of Telegraph Engineers. The name of our former Fellow Wheatstone will go down to posterity as having occupied a foremost place in this great practical application of Oersted's fertile discovery.

I will just briefly allude to another outcome of scientific research. The last half-century was well advanced when our Fellow Dr. Perkin, by utilising a colour reaction which had been employed by chemists as a test for aniline, laid the foundation of the industry of the coal-tar colours, which has now attained such great proportions, and the investigation of the chemical theory of which has occupied the attention of so many eminent chemists from our own Fellow Dr. Hofmann onwards.

There is yet another Jubilee connected with this same year in which our Society is if possible still more closely connected: it is now just 200 years since the publication of the first edition of that immortal work, the "Principia" of Newton. Some of the important results embodied in the "Principia" had previously been communicated to the Royal Society.

But restricting our view to the last half-century alone, we can hardly help casting a glance at the progress of science, and of the practical applications of science, within that period. In electricity, I have already referred to the electric telegraph, now passed into the management of a department of the State, and inwoven in our daily life, with its wires stretching all round the earth like the nerves in the body, and placing us in immediate connexion with distant countries. Much more recent than the invention of the electric telegraph is that, in some respects, still more wonderful apparatus for communication at a distance afforded by the telephone. The application of electricity to lighting purposes, of which we have availed ourselves for the lighting of the apartments of our own Society, is an industrial outcome of Faraday's discovery of magneto-electric induction which could not have been thought of when the account of that discovery first appeared in our Transactions. It is true that what I have just been mentioning with respect to electricity consists of industrial applications rather than the discovery of new scientific principles; but these industrial applications react upon abstract science beneficially in more ways than one. The possibility of useful applications induces theorists to engage in investigations which they might not otherwise have thought of, the result of which is oftentimes to lead them to a clearer apprehension of fundamental principles, and to induce them to undertake exact quantitative determinations of fundamental constants. Moreover, the grand scale on which apparatus for actual commercial use has to be constructed, renders it possible for scientific men, through the courtesy of those who direct the construction, to make interesting experiments on a scale the cost of which would be quite prohibitory if it were a matter of science pure and simple. Take for example the experiments made by Faraday on 100 miles of submerged covered wire at the Works of the Electric Telegraph Company.

When we think of the progress of science, both abstract and applied, during the last half-century, we can hardly help speculating as to the possible increase of scientific knowledge half a century hence. Perhaps we might be tempted to think that the mine must have been so far worked that no great quantity of precious ore can still be left, except what lies too deep for human power to extract. Yet surely the progress of knowledge in the past warns us against any hasty conclusion of the kind. How often have accessions to our knowledge been made which were quite unforeseen and quite unexpected, and how can we say what great discovery may not be made at any moment, and what a flood of light may not result from it?

In what direction such discoveries may be made, it would be rash indeed to attempt to predict. Yet one cannot help thinking of one or two cases in which we seem almost in touch of what if we could reach it would probably give us an insight into the processes of nature of which we have little idea at present. Take for example the theory of electricity as contrasted with the theory of light. In the latter we have the laws of reflection and refraction, which have long been known, the remarkable phenomenon of interference, the curious appearances which we designate by phenomena of diffraction. But all these fall in the most simple and natural way into their places when we have arrived at the answer to the question, What is light? which is furnished by the statement,—Light consists in the undulations of an elastic medium. But we are not at present able to give a similar answer to the question, What is electricity? The appropriate idea has yet to be found. We know a great deal about its laws, and its connexion with magnetism and chemical action; we are able to measure accurately physical constants relating to it; we make it subservient to the wants of daily life; and yet we are unable to answer the question, What is it? Could we only give a definite answer to this question, it seems likely that the production of electricity by friction, electrostatic attractions and repulsions, the laws of electrodynamics, those of thermodynamics, the nature of magnetism, and magneto-electric phenomena would prove to be all simple deductions from the one fundamental idea. Nay more: so closely is electricity related to chemical action, that could we only clearly apprehend the nature of electricity, it seems not unlikely that an unexpected flood of light might be shed on chemical combination.

Let me refer to one other instance in which a large accession to our present knowledge seems not altogether hopeless. We know that when an electric discharge is passed through a given gas, or between electrodes formed of a given substance, an analysis of the spark reveals a usually complicated spectrum of bright lines, characteristic of the chemical substances present. The arrangement of the lines in most cases seems capricious, while in other instances we have repetitions of lines, or else rhythmical flutings, indicative of law, though one of no simple character. There can be no reasonable doubt that the periodic times indicated by the bright lines seen in the spectrum are those belonging to the component vibrations of the chemical molecules themselves; and the appearance is just such as would be produced by a tolerably complex dynamical system vibrating under the action of internal forces of restitution. Now such a system may really be composed of two or more simpler systems, held together less firmly than the parts of one of the simpler systems; and the complex vibrations of the whole may be made up of those of the several simpler systems, modified, however, by their mutual connexion, together it may be with others due to the mutual connexion of the simpler systems regarded each as a whole. It is conceivable that relations of chemical composition may thus be pointed out even between substances which we deem elementary, and which from their great stability we may, perhaps, never be able actually to decompose.

But I must apologise for having taken up your time with speculations as to the future; I will turn now to some mention of the action of your Council during the past year, and of the progress made by committees appointed by the Council.

In response to an invitation received from the Academy of Sciences of Paris, that the Society should be represented at the International Conference of Astronomers, which it was proposed should assemble in Paris, in the spring, for the purpose of deliberating about concerted action for obtaining a complete map of the starry heavens by means of photography, your Council requested the Astronomer Royal to represent the Society on that occasion. The conference met, as it was proposed, last spring, and I believe that the English astronomers at least think that a good foundation has been laid for concerted action in that great undertaking.

As the Fellows are already aware from a circular which has been issued, the Council have decided to make a change in the mode of publication of the 'Philosophical Transactions.' The average yearly volume is a good deal more bulky now than it was at the beginning of the century, and its size is such as not unfrequently to make it desirable to bind one volume in two. The sciences, moreover, which are represented in the 'Philosophical Transactions,' divide themselves very naturally into two groups: mathematics, physics, and chemistry forming one, and the biological sciences the other. The Council have decided to issue the 'Transactions' from henceforth in two series, corresponding to these two divisions, and a yearly volume will appear in each series. It is hoped that this arrangement will be conducive to an earlier publication, as the numeration of the pages in the two series can go on independently. The individual papers will also be issued separately, so that Fellows who prefer receiving them in this way can have them as soon as they are printed. Moreover, the issue of the 'Transactions' in two series will enable Institutions that are concerned with one only of the two groups of subjects, and that are not on our list for free presentation, to purchase for their libraries the series devoted to that group, instead of going to the expense of procuring the whole 'Transactions.'

I am happy to be able to announce that the publication of the "Challenger" report is now nearly finished. Twenty-eight volumes, some in two parts, have now been published, and these are all in the Society's library.

The Krakatoa Committee have now all but completed their labours. A vast amount of information on the phenomena related to that most remarkable volcanic explosion has been collected and digested, different branches of the inquiry having been taken up by different members of the Committee. An estimate has been made of the cost of publication of the report, and the Council has decided that it should be published as a separate work, and has voted the sum required for publication. The printing of the volume is now far advanced, and in a very few weeks it will in all probability be in the hands of the public.

The reports of the observers of the total solar eclipse of August last year are now coming in. From inquiries I have made I am in hopes that they will all be in by the end of the year. It is obviously convenient that they should all be dealt with together, rather than appear in a scattered form for the sake of a slightly earlier publication of those which happen to be ready first.

I mentioned in my last address that with respect to this eclipse the Council, acting in accordance with the recommendations of the Eclipse Committee, had decided to confine themselves to an expedition to Grenada, without attempting another to Benguela on the Western Coast of Africa, which if sent out from this country would have been a good deal more costly, and of which the success, judging by such accounts of the climate of Benguela and its neighbourhood as we could procure, seemed very doubtful. The Committee guaranteed, however, £100 towards the expense of a small expedition from the Cape in case Her Majesty's Astronomer at that place should be in a condition to organise one. Sir W. J. Hunt-Grubbe, the Admiral in command at that station, was prepared to render every assistance in his power. Ultimately, however, it was not found practicable to organise an expedition from the Cape, and so the English observations of the eclipse were confined to those taken at Grenada. I have heard that the day of the eclipse was fine at Benguela, but there were no astronomers of any nation there to take advantage of it. It may be doubted, however, whether, in spite of the fineness, the haze which is said to prevail so much on that coast at that time of year, might not materially have interfered with the observations.

The boring in the Delta of the Nile has been continued, by the favour of the War Office, under the able and zealous superintendence of Captain Dickinson, R.E. As I mentioned last year, the Committee thought it best to concentrate their efforts on a single boring until rock should be reached, or else a stratum of such a character as to show that the alluvial or drifted deposit had been got through. This result has not at present been obtained. The boring at Zagazig reached the depth of 324 feet, when the tube broke, and stopped for the time further progress. It is, however, a matter of interest and importance to know that the drift or deposit extends to so great a depth. Geologists attach so much importance to the prosecution of the inquiry that at the suggestion of the Delta Committee an application was made to the Government Grant Committee for a grant of £500, which was acceded to by the Committee. This sum would not suffice for the prosecution of the inquiry to the extent contemplated; but it was thought that with such a sum as a nucleus extraneous pecuniary assistance might be obtained from Societies or individuals specially interested in the inquiry, and the Council have authorised the Delta Committee to avail themselves of such aid.

The meetings of Council and Committees continue to be very numerous, and no less than twenty-two Committees and Sub-Committees have been at work during the session.

The number of papers communicated to the Society continues to increase. In 1884–5 the number was 93; in 1885–6 it was 113; and in the past session, 129.

Since the last Anniversary one complete part of the 'Philosophical Transactions,' and thirty-two separate papers towards the new volume have been published; the whole comprising no less than 1482 pages of letterpress and seventy-six plates. In the same period twelve numbers of the 'Proceedings,' containing 984 pages, have appeared.

The task of preparing the MS. of the Catalogue of Scientific Papers, decade 1874 to 1883, has proved far heavier than was anticipated, and the matter very far exceeds in bulk that of the previous decade. The cataloguing of papers from the volumes in our own library has long been finished, but the work of gleaning stray papers from works in other libraries which we do not possess has proved more arduous than was expected, and even now is not quite completed. It is confidently hoped, however, that the MS. will be completed for the press during the coming session.

The distribution and exchange of duplicates from our library, commenced last session, has been continued, and several defective series among the periodicals on our shelves have been made good. The general work of the library has received careful attention at the hands of Mr. Alfred White, who shortly before the last Anniversary was appointed to the office of Assistant Librarian.

The Copley Medal for the year has been awarded to the eminent botanist, your former President, Sir Joseph Dalton Hooker. It is impossible, within the limits to which I must confine myself on the present occasion, to do more than briefly refer to some of the more salient features of his scientific career, extending as it does over nearly half a century of unceasing intellectual activity; and I need hardly say that in attempting to give some idea of important labours which lie outside my own studies, I am dependent on the kindness of scientific friends.

As a traveller, he can perhaps only compare with Humboldt in the extent to which he has used travel as an instrument of research. To quote a remark by Professor Asa Gray, "No botanist of the present century, perhaps of any time, has seen more of the earth's vegetation under natural conditions." His Antarctic voyage in 1839–43 supplied the material for a series of well-known works of first-rate importance on the vegetation of the southern hemisphere; and these in their turn formed the basis of important general discussions. The journey to India in 1847–51 yielded, in the Himalayan journals, as Humboldt has remarked, "a perfect treasure of important observations." The maps made of the passes into Thibet are even still unsuperseded. The fine work on the "Sikkim Rhododendrons" was at once a revelation to the botanist and to the horticulturist. His account of the glacial phenomena of the Himalayas supplied facts both to Darwin and to Lyell. A journey to Morocco in 1871 and a later visit to North America led to important conclusions on plant distribution.

Perhaps Sir Joseph Hooker's most important place in scientific history will be found in the rational basis upon which he placed geographical botany. De Candolle, while admitting the continuity of existing floras with those preceding them in time, still adhered in principle to the multiple origin of species. To quote a remark by Professor Asa Gray—"De Candolle's great work closed one epoch in the history of the subject, and Hooker's name is the first that appears in the ensuing one." According to Lyell, "the abandonment of the old received doctrine of the 'immutability of species' was accelerated in England by the appearance in 1859 of Dr. Hooker's 'Essay on the Flora of Australia.'" This Essay effected a revolution. It was quickly followed in 1860 by the classical essay on the "Distribution of Arctic Plants," and in 1866 by the Nottingham Lecture on Insular

Floras. The fact of widely *dissevered* localities for species, which De Candolle found an insuperable obstacle to abandoning the doctrine of multiple origin, has, in the hands of Hooker and A. Gray (as stated by Bentham), afforded the most convincing proof of the genetic relationship of the floras of which such species are components.

In systematic botany, Hooker has perhaps had no rival since Robert Brown. The "Genera Plantarum," the joint work of himself and his friend Bentham, and the "Flora Indica," to the completion of which our colleague is devoting the leisure of a well-earned retirement, form only as it were the head of an immense body of taxonomic memoirs.

Nor have his services to botanical science been confined to geographical botany and to taxonomy. His researches on various groups, such as *Welwitschia* and others, deal in a masterly way with morphological problems of the highest interest and of extreme difficulty.

While no one would attempt to minimise the commanding and unique position of Mr. Darwin, the scientific historian of the future will recognise how much the development of the modern theory of evolution, from its first conception in the mind of Mr. Darwin, was facilitated by the interaction upon one another of the work and minds of Darwin, Hooker, and Lyell. It was due to the earnest efforts of his two friends that Mr. Darwin was induced to publish the first sketch of the origin of species at all. And no one, had he been alive, would have more cordially recognised than Mr. Darwin how vast an armoury of facts the wide botanical experience of Hooker constantly placed at his disposal in fortifying and supporting his main position.

Of the two Royal Medals, it is customary, though it is not an invariable rule, to award one for mathematics or physics, and the other for biological science.

The medal which, in accordance with the usual rule, has been devoted to mathematics and physics, has this year been awarded to Colonel A. Clarke for his comparison of standards of length, and determination of the figure of the earth.

Colonel Clarke was for some twenty-five years the scientific and mathematical adviser for the Ordnance Survey, and whilst acting in that capacity he became known to the whole scientific world as possessing a unique knowledge and power in dealing with the complex questions which arise in the science of geodesy.

His laborious comparison of the standards of length, carried out under General Sir Henry James, R.E., are universally regarded as models of scientific precision.

His determination of the ellipticity and dimensions of the earth from the great arcs of meridian and longitude involved a very high mathematical ability and an enormous amount of labour. The conclusion at which he arrived removed an apparent discrepancy between the results of pendulum experiments and those derived from geodesy, and is generally accepted as the best approximation hitherto attained as to the figure of the earth.

The accounts of these investigations have been published in a number of memoirs, several of which have been communicated to the Royal Society.

In 1880 he published a book on Geodesy, which, besides giving an accurate account of that science, embodies the main results of the work of his life.

In the biological division of the sciences the Royal Medal has this year been awarded to Professor Henry N. Moseley for his numerous researches in animal morphology, and especially his investigations on Corals and on Peripatus.

The result of his elaborate investigations on corals, an account of which has been published in the 'Philosophical Transactions,' was to show that the Milleporidæ and the Stylasteridæ were not, as had been thought, Anthozoan in nature, but were composite coral-forming hydroids. Many new genera and species were described by him in these memoirs, and in fact not merely was a new group of organisms, the Hydrocorallinæ, indicated, but the complete morphology and systematic subdivisions of that order were worked out.

Moseley's memoir on Peripatus is not less remarkable. He was the first to point out the true nature of this remarkable animal, and to demonstrate that it was in reality an archaic Arthropod. The subsequent investigations of Balfour and Sedgwick have further increased the importance of Moseley's discovery.

Moseley's memoir on the Land Planarians of Ceylon ('Phil. Trans.,' 1872) is an important contribution to the anatomy of the Turbellaria. He was the first to apply the method of section-cutting to the Planarians, and his paper is full of new facts of great importance, which have stood the test of subsequent work over the same ground.

Besides these three great memoirs published in the 'Philosophical Transactions,' Moseley has published numerous minor discoveries, and his spectroscopic observations on the colouring matters of marine organisms have proved the starting-point of valuable investigations.

Mention must not be omitted of Moseley's admirable book, 'Notes of a Naturalist on the "Challenger,"' which has been justly compared, for the varied ability, interest, and activity which it evinces on the part of the author, to Darwin's 'Voyage of the "Beagle."'

Since the date of the works above referred to, Moseley has been chiefly active in the discharge of his duties as Linacre Professor, and the success with which he has directed the work of his pupils is evinced by the important memoirs on zoological subjects which several of them have produced whilst working under his direction. He has himself also published a remarkable discovery with regard to the Chitons. In the shells of many genera and species of these molluscs he has detected highly developed eyes, of which he has described the minute structure.

The Davy Medal for the year 1882 was awarded by the Council to Professors Mendelejeff and Lothar Meyer conjointly, "For their Discovery of the Periodic Relations of the Atomic Weights." This relation, now known as the "Periodic Law," has attracted great attention on the part of chemists, and has even enabled Professor Mendelejeff to predict the properties of elements at the time unknown, but since discovered, such as Gallium for instance.

But while recognising the merits of chemists of other nations we are not to forget our own countrymen; and accordingly the Davy Medal for the present year has been awarded to Mr. John A. R. Newlands for his discovery of the Periodic Law of the Chemical Elements. Though in the somewhat less complete form in which the law was enunciated by him, it did not at the time attract the attention of chemists, still in so far as the work of the two foreign chemists above mentioned was anticipated, the priority belongs to Mr. Newlands.

The Statutes relating to the election of Council and Officers were then read, and Professor Clifton and General Walker having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Professor George Gabriel Stokes, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.—{ Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.

Sir William Bowman, Bart., M.D.; Henry Bowman Brady, F.L.S., F.G.S.; Professor Arthur Cayley, D.C.L., LL.D.; W. T. Thiselton Dyer, M.A.; Professor David Ferrier, M.A., M.D.; Edward Frankland, D.C.L.; Arthur Gamgee, M.D.; Professor Joseph Henry Gilbert, M.A.; Professor John W. Judd, P.G.S.; Professor Herbert McLeod, F.I.C.; William Pole, Mus. Doc.; William Henry Preece, M.I.C.E.; Admiral Sir George Henry Richards, K.C.B.; Professor Arthur William Rücker, M.A.; the Earl of Rosse, D.C.L., LL.D.; Sir Bernhard Samuelson, Bart., M.I.C.E.

The thanks of the Society were given to the Scrutators.

Balance Sheet, 1887.

Statement of Receipts and Expenditure from November 14, 1886, to November 12, 1887.

Receipts	£	s.	d.	£	s.	d.
To Balance at bank, 13th November, 1886				1,838	5	10
,, Balance in hand, Catalogue Account	13	9	7	24	9	11½
,, ,, Petty Cash	11	0	4½			
,, Annual Contributions, 171 at £4	684	0	0	1,023	0	6
,, 113 at £3 (and Bank charge 6*d.* refunded)	339	0	6			
,, Admission Fees				10	0	0
,, Compositions				60	0	0
,, Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				262	0	0
,, Rents:	£	s.	d.			
Fee Farm, Lewes	18	12	10	118	10	11
Mablethorpe Estate	99	18	1			
,, Ground Rents				606	1	2
,, Dividends (exclusive of Trust Funds)				1,893	19	3
,, do. Jodrell Fund				150	12	5
,, Interest on Mortgage Loan				580	16	8
,, Sale of Transactions and Proceedings				750	8	9
,, Eclipse Expedition, Grants	350	0	0	373	8	1
,, ,, ,, Repayments	23	8	1			
				£7,691	13	6½

Expenditure	£	s.	d.	£	s.	d.
By Salaries, Wages, and Pension				1,506	6	0
,, Catalogue of Scientific Papers				166	12	3
,, Books for the Library				247	17	8
,, Printing and Advertising Transactions, and Separate Copies to Authors and Publisher	691	16	4	2,482	15	10
,, Ditto Proceedings, Nos. 247 to 257	498	18	7			
,, Ditto Miscellaneous	102	15	9			
,, Paper for Transactions and Proceedings	196	17	6			
,, Binding ditto	50	15	6			
,, Engraving and Lithography	941	12	2			
,, Soirée and Reception Expenses				145	1	1
,, Coal, Lighting, &c.	74	12	6	901	8	8
,, Office Expenses	39	12	9½			
,, House Expenses	543	1	11			
,, Tea Expenses	16	16	11			
,, Fire Insurance	56	15	0			
,, Taxes	57	0	6			
,, Advertising	17	9	6			
,, Postage, Parcels, and Petty Charges	52	11	6			
,, Miscellaneous Expenses	43	8	0½			
,, Law Charges				28	19	4
,, Electric Lighting				554	8	2
,, Lendenfeld Monograph				48	5	0
,, Advance to Circumpolar Fund				20	4	11
,, Purchase of £332 14*s.* 6*d.* India 4 per Cent. Stock				340	4	1
,, ,, £547 7*s.* 6*d.* Reduced 3 per Cent. Consols				551	9	7
,, Eclipse Expedition (Total expenditure in excess of Grants, £205 9*s.* 7*d.*)				447	14	7
,, Balance on hand, Catalogue Account	14	9	7	18	5	11½
,, Ditto, Petty Cash	3	16	4½			
,, Balance at Bankers				232	0	5
				£7,691	13	6½

Trust Funds.

	£	s.	d.	£	s.	d.
To Balance at Bank 13th November, 1886:—						
General Account	1,378	0	7			
Circumpolar Expedition Account	420	6	9	3,195	10	7
Fee Reduction Fund Account	31	12	10			
Scientific Relief Fund Account	1,365	10	5			
,, Scientific Relief Fund, Dividends and Donations	9,410	1	3			
,, Donation Fund, Dividends and Transfer from Handley Fund	414	1	2			
,, Rumford Fund, Dividends	67	10	3			
,, Bakerian and Copley Medal Fund, Dividends	58	4	0			
,, Keck Bequest, Dividends	23	4	6			
,, Wintringham Fund, Dividends	34	17	4			
,, Croonian Lecture Fund, Rent received	50	9	8	11,478	7	8
,, Davy Medal Fund, Dividends	31	19	4			
,, Gassiot Trust, Dividends and Investment realised	733	2	11			
,, Jodrell Fund, Dividends	150	12	5			
,, Fee Reduction Fund, Dividends	406	11	8			
,, Darwin Memorial Fund, Dividends	77	8	3			
,, Circumpolar Expedition, advance from Royal Society General Account (1887)	20	4	11			
				£14,673	18	3

	£	s.	d.	£	s.	d.
By Scientific Relief Fund, Investments and Grants	10,201	17	3			
,, Donation Fund, Grants, &c.	721	17	0			
,, Rumford Medal, &c., Fund	134	7	7			
,, Bakerian and Copley Medal Fund, Medal, and Gift	54	11	11			
,, Keck Bequest, Payment to Foreign Secretary	23	4	6			
,, Wintringham Fund, Payment to Foundling Hospital	34	15	10			
,, Croonian Lecture Fund, Payments	2	6	10	12,937	7	4
,, Davy Medal Fund, Gold Medals	32	4	6			
,, Gassiot Trust, Payments to Kew Committee and Investments	868	4	10			
,, Jodrell Fund, Transfer to Royal Society General Account	150	12	5			
,, Fee Reduction Fund, transfer to Royal Society General Account (1887)	262	0	0			
,, Darwin Memorial Fund, Expenses	10	13	0			
,, Circumpolar Expedition, Expenses	440	11	8			
,, Balance at Bankers:—						
General Account	986	12	0			
Fee Reduction Fund	176	4	6	1,736	10	11
Scientific Relief Fund	573	14	5			
				£14,673	18	3

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £100 per annum.

Ground Rent of House No. 57, Basinghall Street, rent £380 per annum.

" " of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on Government Annuities and Bank Stock (produced £550 3*s*. 8*d*. in 1886–87).

£21,547 7*s*. 6*d*. Reduced 3 per Cent. Annuities { £15,499 19*s*. 9*d*., General Purposes. £6,047 7*s*. 9*d*., Handley Fund. }

£15,000 Mortgage Loan, 4 per Cent.

£19,464 0*s*. 2*d*. Consolidated Bank Annuities, being £16,011 19*s*. 1*d*., namely:—

	£	*s*.	*d*.
Donation Fund	6,339	0	1
Rumford Fund	2,322	19	0
Wintringham Fund	1,200	0	0
Gassiot Trust	350	0	0
Sir J. Copley Fund	1,666	13	4
General Purposes	4,133	6	8

and £3,452 1*s*. 1*d*. in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£403 9*s*. 8*d*. New $2\frac{1}{2}$ per Cent. Stock.—Bakerian and Copley Medal Fund.

£12,182 14*s*. 10*d*. New Three per Cent. Stock { £7,000, Scientific Relief Fund. 5,182 14*s*. 10*d*., Jodrell Fund. }

£1,000 India $3\frac{1}{2}$ per Cent. Stock.

£600 Midland 4 °/ₒ, Debenture Stock.—Keck Bequest.

£5,660 Madras Railway Guaranteed 5 per Cent. Stock { General Purposes, £5,000. / Davy Medal Fund, £660.

£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

£6,396 Great Northern Railway 4 per Cent. Debentures { Scientific Relief Fund, £5,000. / The Trevelyan Bequest, £1,396.

£4,000 Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.

£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.

£5,600 „ „ „ „ 4 % Consolidated Guaranteed Stock.—Scientific Relief Fund.

£5,000 North Eastern Railway 4 °/ₒ Preference Stock.—General Purposes.

£5,000 London and North Western Consolidated 4 °/ₒ Preference Stock.—General Purposes.

£2,000 South Eastern Railway 4°/ₒ Debenture Stock.—Darwin Memorial Fund.

JOHN EVANS, *Treasurer.*

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct.

G. G. STOKES.
E. FRANKLAND.
GEO. HENRY RICHARDS.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct.

JAMES COCKLE.
WILLIAM HUGGINS.
H. T. STAINTON.
G. J. SYMONS.

Trust Funds. 1887.

Scientific Relief Fund.

	£	s.	d.
L. & N.W.R. 4 per cent. Consolidated Guarantee Stock	5,600	0	0
New 3 per Cent. Annuities	7,000	0	0
Great Northern 4 per cent. Debenture Stock	5,000	0	0
Uninvested	286	12	9

Dr.

	£	s.	d.	£	s.	d.
To Balance { Uninvested	1,332	5	0			
To Balance { In hand	33	5	5			
				1,365	10	5
,, Dividends				582	16	3
,, Donations				8,827	5	0
				£10,775	11	8

Cr.

	£	s.	d.	£	s.	d.
By Grants				329	0	0
,, Purchase of £5,600 L. & N. W. Ry. 4 per cent. Consolidated Guarantee Stock				6,706	7	9
,, Purchase of £2,600 Great Northern Railway 4 per cent. Debenture Stock				3,166	9	6
,, Balance uninvested	£286	12	9	573	14	5
,, ,, in hand	287	1	8			
				£10,775	11	8

Donation Fund.

£6,339 0*s.* 1*d.* Consols.

The Trevelyan Bequest. £1,396 Great Northern Railway 4 per Cent. Debenture Stock.

	£	s.	d.
To Balance	417	16	3
,, Dividends	238	6	1
,, Transfer from Handley Fund	175	15	1
	£831	17	5

	£	s.	d.
By Grants	721	17	0
,, Balance	110	0	5
	£831	17	5

Rumford Fund.

£2,322 19*s*. Consols.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	134	17	7	By Medals	58	14	10
,, Dividends, 1887	67	10	3	,, Payment to S. B. Langley	75	12	9
				,, Balance	68	0	3
	£202	7	10		£202	7	10

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s*. 4*d*. Consols.
£403 9*s*. 8*d*. New 2½ per Cent. Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	101	10	6	By Gold Medal	4	11	11
,, Dividends, New 2½ per Cent. Stock	9	15	6	,, Professor Franz E. Neumann, Sir J. Copley's Gift	50	0	0
,, Dividend—Sir J. Copley's Fund	48	8	6	,, Balance	105	2	7
	£159	14	6		£159	14	6

The Keck Bequest.

£600 Midland Railway 4 per Cent. Debenture Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Dividends, 1887	23	4	6	By Payment to Foreign Secretary	23	4	6

Wintringham Fund.

£1,200 Consols.

	£	s.	d.		£	s.	d.
To Balance	34	15	10	By Payment to Foundling Hospital, 1887	34	15	10
,, Dividends, 1887	34	17	4	,, Balance	34	17	4
	£69	13	2		£69	13	2

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

	£	s.	d.		£	s.	d.
To Rent	50	9	8	By Law Expenses	2	6	10
				,, Balance	48	2	10
	£50	9	8		£50	9	8

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	d.
To Balance	77	8	8	By Gold Medals	32	4	6
,, Dividends	31	19	4	,, Balance	77	3	6
	£109	8	0		£109	8	0

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.
£350 3 per Cent. Consols.

	£	s.	d.		£	s.	d.
To Balance	173	9	5	By Payments to Kew Committee	490	8	7
„ Dividends	498	8	7	„ Purchase of £200 Italian Irrigation Bonds	227	5	0
„ Italian Irrigation Bonds, drawn	234	14	4	„ „ £150 3 per Cent. Consols	150	11	3
				„ Balance	38	7	6
	£906	12	4		£906	12	4

Handley Fund.

£6,047 7*s.* 9*d* Reduced 3 per Cent. Annuities.

	£	s.	d.		£	s.	d.
To Dividends, 1887	175	15	1	By transfer to Donation Fund	175	15	1

The Jodrell Fund.

£5,182 14*s.* 10*d.* New 3 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Dividends, 1887	150	12	5	By transfer to Royal Society General Account	150	12	5

Fee Reduction Fund.

£4,000 Metropolitan Consols 3½ per Cent.
£7,000 London and North Western Railway 4 per Cent. Debentures.

	£	s.	d.		£	s.	d.
To Balance	31	12	10	By transfer to Royal Society General Account (1887)	262	0	0
„ Dividends, 1887	406	11	8	„ Balance	176	4	6
	£438	4	6		£438	4	6

Darwin Memorial Fund.

£2,000 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	438	2	4	By Printing and Engraving	10	13	0
,, Dividends, 1887	77	8	3	,, Balance, Capital £350 2 8 / ,, ,, Interest £154 14 11	504	17	7
	£515	10	7		£515	10	7

ADMINISTRATIVE FUND.

Circumpolar Expedition Fund.

	£	s.	d.		£	s.	d.
To Balance	420	6	9	By Expenses	11	3	2
,, Transfer from Royal Society General Account (1887)	20	4	11	,, Payment to Paymaster General	429	8	6
	£440	11	8		£440	11	8

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1886 ..	5	49	192	172	100	518
Since Elected ..	..	..	+ 1	+ 1	+ 14	+ 16
Since Deceased ..	..	− 1	− 5	− 8	− 2	− 16
Nov. 30, 1887 ..	5	48	188	165	112	518

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XLI, p. 396).

1886–87.

	£
Prof. P. G. Tait, for Reduction of Observations, by Forbes' method, for determination of Thermal Conductivity	40
A. P. Laurie, for a Research into the Electromotive Force of Metals and Alloys in constant Voltaic Cells	15
A Committee, for the purpose of investigating chemically the Water of the Clyde river entrance and lock-systems	150
The Solar Eclipse Committee of the Royal Society	100
The Pendulum Committee of the Royal Society, for the expense of Pendulum Observations to be undertaken at the Kew Observatory	100
R. Kidston, to continue his Investigations into the distribution of the Carboniferous Flora, and to prepare lists of Fossil Plants characteristic of the upper, middle, and lower divisions of the Coal Measures as developed in Britain	40
A Committee, for Illustrations to Mr. Hamilton's Work on the Central Nervous System	300
Dr. Warner, to assist in enumerating, analysing, and studying Nerve-muscular Movements in Man, as signs indicating the action of nerve-centres	30
Carried forward................	£775

Brought forward................	£775
Dr. J. W. Fraser, for the Investigation of the Actions of infused Beverages of various kinds on peptic and pancreatic Digestion of Nitrogenised and Hydrocarbonaceous foods	15
Prof. V. Horsley, for an Inquiry into the function of the Thyroid Gland with relation to the Causation of the diseases known as Myxœdema, Goitre, &c.	50
Dr. L. C. Wooldridge, for Continuation of Experiments on the Physiology and Pathology of the Blood	40
W. J. Harrison (*a*), to investigate more completely the Cambrian rocks discovered at Dosthill, in Warwickshire, £10; (*b*) and to collect and study the rocks constituting the Lower Keuper Conglomerate, at Sedmore, near Stourbridge, £10....	20
Dr. R. Stockman, for a Research into the Physiological Action of Borneo Camphor and some allied bodies	20
H. N. Ridley, to explore the Natural History of Fernando Noronha	150
Dr. A. Downes, for Apparatus to be used in Researches on (1) the action of light on micro-organisms, (2) the duration of life of micro-organisms, (3) a means of measuring the actinic value of light by oxalic acid................................	12
Dr. P. F. Frankland, for assistance in further Investigations of Micro-organisms, their distribution and vitality	50
The China Flora Committee, to continue the preparation and printing of the Index Floræ Sinensis	200
The Delta Boring Committee of the Royal Society, for continuing the boring operations in the Delta of the Nile........	200
Dr. F. R. Japp, for an Investigation of the Reactions of Ketones, Diketones, and allied compounds	75
Prof. Humpidge, to continue the Research on the Specific Heats of the pure elements in the solid state, and at varying temperatures	20
H. Tomlinson, for further Research on the influence of Stress and Strain on the physical properties of matter	150
V. H. Veley, for an investigation into the rate of Evolution of a Gas from a homogeneous liquid	50
Sir W. Thomson, for the reduction and full discussion, by the method of Harmonic Analysis, of a series of Tide Records taken at Ostend and Dover, and covering a period of seven years for each port	50
A. M. W. Downing, for payment of a Computer to aid in comparing the places of Stars in Gould's Argentine General Catalogue of 32,448 stars, with those in Stone's Cape Catalogue of 12,441 stars	25
Carried forward................	£1,902

Brought forward	£1,902	0
Dr. G. A. Atkinson, for a Research on the Chemistry and Pharmacology of the Nitrites of Sodium, Potassium, Ethyl, and Amyl, and of Nitro-glycerine	15	0
Dr. C. R. A. Wright, for the investigation of a class of Voltaic Cells (mostly novel) in which the essential chemical action is the formation of metallic or other oxides by Atmospheric Oxidation	50	0
Prof. Ramsay, for Investigations on Evaporation and Dissociation	50	0
S. U. Pickering, for an Investigation of the Heat of Neutralisation of the Amines	60	0
G. J. Symons, for the erection of additional Rain Gauges in the Lake District, and for replacing those worn out	12	7
W. H. Perkin, Jun., for a Research on the Chemical Constitution of the alkaloid Berberin, and of the colouring matters Brasilin, Hæmatoxylin, and Santalin	50	0
J. Joly, for Improvements in the apparatus for the method of Condensation in Calorimetry; for further enquiry into the specific heats of the Sulphides; and other researches	30	0
H. G. Colman and W. H. Perkin, jun., for a Research on the action of Methyl-Tetramethylene Bromide on Aceto-acetic and Malonic Ethers	20	0
Prof. J. Emerson Reynolds, for payment of an Assistant to carry out the analytical portion of his work on Silicon Compounds	60	0
Dr. G. H. Bailey, to examine the Action of Water at high pressure in effecting the alteration of Silicates, &c., and two other researches (see full Application)	30	0
A. Harden and Dr. Perkin, junr., for a Research on the Constitution of Dehydracetic Acid and its derivatives	20	0
Dr. Hodgkinson, for expense of an Assistant in further investigating the Hydrocarbons Di-fluoryl and Di-acenaphthen and some derivatives of Phenyl Acetic acid	50	0
A. Scott, to determine with the greatest possible accuracy the true Combining Volumes of H and O when uniting to form water	50	0
Dr. P. F. Frankland, for further investigating the Chemical Changes effected by Specific Micro-organisms	25	0
The Krakatoa Committee, in aid of the expense of printing their Report	250	0
Rev. A. E. Eaton, for the cost of Drawings to elucidate the classification of Terrestrial Isopod Crustacea	50	0
Carried forward	£2,724	7

Brought forward.................£2,724	7
The Marine Biological Association, for the Investigation of the Flora and Fauna of Plymouth Sound, with especial relation to the physical conditions related thereto	250 0
Prof. Schäfer, for payment of an Assistant to aid in prosecuting a Research into the functions of the Nervous System, especially of the Cerebral Cortex..........................	100 0
Dr. T. L. Brunton, for Investigations on the connexion between Chemical Constitution and Physiological Action	100 0
John Beard, for Researches in Elasmobranch and Ganoid development	150 0
Prof. W. C. McIntosh, for continuation of the Researches on the Development of Fishes, and especially the investigation of those in which the post-larval stages are unknown	50 0
Prof. C. Lapworth, for the Study of the Stratigraphical sequence on the lower Palæozoic Rocks of Wales and the West of England, and of the Graptolites contained therein	100 0
Prof. W. A. Herdman, for the Exploration by dredging, tow-netting, &c., of Liverpool Bay, and the neighbouring parts of the Irish Sea, in order to determine accurately the Fauna and Flora of the different parts, and the conditions under which the various species are found	25 0
J. Rattray, to prepare a Monograph of the Diatomaceæ	100 0
Francis Gotch, for the investigation of the Electric Fishes with special reference to the functional activity of the Electric Organ	75 0
A Committee, for the purpose of sending a Collector to obtain Zoological Specimens in the less known West Indian Islands..	100 0
J. Starkie Gardner, to work out a bed of Limestone containing Fossil Plants at Ardtun exposed by quarrying operations last year	50 0
W. F. Denning, for further observation of Shooting Stars and their radiant points, with special reference to the stationary radiation of Meteors	30 0
Prof. T. R. Jones, for further elucidation of the Fossil Ostracoda	100 0
Prof. W. K. Parker, for further Researches in the Morphology of the Vertebrata	300 0
J. Clark, for the examination in detail of the Protoplasmic Movements exhibited by the lower organisms, and by the ordinary vegetable cell..	70 0
	£4,324 7

Dr.	£	s.	d.	*Cr.*	£	s.	d.
To Balance November 30, 1886..	279	15	4	By Appropriations, as above............	4,324	7	0
,, Grant from Treasury.......	4,000	0	0	Printing, Postage, Advertising, and other Administrative Expenses...........	54	14	9
,, Repayments	109	15	3	By Balance on hand, Nov. 30, 1887	22	5	6
,, Interest on Deposit.........	11	16	8				
	£4,401	7	3		£4,401	7	3

Account of Grants from the Donation Fund in 1886–87.

	£	s.	d.
W. de la Rue, for the completing of his Catalogue of Latitude and Longitudes of Solar Spots, £200. On account	81	17	0
Prof. Humphry, to assist Mr. C. B. Lockwood in his investigations on the Development of the Heart	30	0	0
Dr. Sclater, to assist Mr. Quelch in obtaining specimens of the young of the Hoatzin (*Opisthocomus cristatus*)	10	0	0
Prof. Schäfer, to assist Mr. J. R. Bradford in his researches upon the salivary secretion	15	0	0
Prof. Bonney, to assist Mr. A. T. Evans in examining sections, pits, &c., in the Midland Counties where the Bunter Conglomerate is well exposed..................	10	0	0
The Delta Boring Committee, for continuation of borings in the Delta of the Nile	200	0	0
Dr. Gaskell, in aid of his researches on the structure, distribution, and function of the Vascular and Visceral Nerves	40	0	0
G. R. Vine, to complete descriptions of British Fossil Polyzoa and other Organisms	10	0	0
W. Christie, for the adaptation and freight of instruments to observe the Total Solar Eclipse of August 19th (observer, H. Turner)	30	0	0
W. T. Thiselton Dyer, to assist Mr. W. Hemsley in drawing up in alphabetical form a digest of the existing information, and most accessible literature, relating to the Vegetable Productions and Resources of the various possessions of the Empire	20	0	0
Carried forward................	£446	17	0

Brought forward	£446	17	0
Dr. S. H. Vines, to assist Mr. Vaisey in his investigations into the Histology and Morphology of Mosses in particular, and of the Muscineæ in general	25	0	0
Dr. Cash, in aid of his investigation on the subject of Intestinal Rest and Movement	35	0	0
G. J. Symons, to redetermine the temperatures of the Hot Springs in the Pyrenees, as laid down by Prof. J. D. Forbes ('Phil. Trans.,' 1836)	40	0	0
Dr. Gill, to assist in defraying expenses incurred in carrying on his researches in Stellar Photography	150	0	0
Prof. Pritchard, for experiments to be made with reference to the Paris Congress on Stellar Photography, with two mirrors of the same aperture, but of different focal length, one about the half of the other	25	0	0
	£721	17	0

Report of the Kew Committee for the Year ending October 31, 1887.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

Mr. Warren de la Rue, *Chairman.*

Captain W. de W. Abney, R.E.
Prof. W. G. Adams.
Staff-Commander E. W. Creak, R.N.
Prof. G. C. Foster.
Mr. F. Galton.
Admiral Sir G. H. Richards, K.C.B.
The Earl of Rosse.
Mr. R. H. Scott.
Lieut.-Gen. R. Strachey, C.S.I.
General J. T. Walker, C.B.

The Committee regret to announce the death, in July last, of their late member, Lieut.-General W. J. Smythe, R.A. He had held a seat upon the Committee since 1871, but for some years past, owing to his residence in Ireland, he had not been able to take part in their meetings.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches and Marine Chronometers.
7th. Miscellaneous.

I. Magnetic Observations.

Throughout the past year the magnetographs have worked in a satisfactory manner, and the usual determinations of the scale values of all the instruments were made in January last.

Owing to the gradual secular change of Declination the distance between the dots of light upon the cylinder of the magnetometer had become too small for satisfactory registration, and in consequence it was found necessary to re-adjust the instrument by altering slightly the inclination of the mirror attached to the magnet.

The values of the ordinates of the different photographic curves determined then were as follows :—

Declination : 1 inch=0° 22′·04. 1 cm.=0° 8′·7.

Bifilar, January 10, 1887, for 1 inch δH=0·0255 foot grain unit.
„ 1 cm. „ =0·00046 C.G.S. unit.
Balance, January 11, 1887 „ 1 inch δV=0·0281 foot grain unit.
„ 1 cm. „ =0·00051 C.G.S. unit.

In the case of the bifilar magnetometer it was also found necessary to re-adjust the instrument, at the same time its sensibility was slightly altered, after which the scale value was again determined with the following result :—

Bifilar, January 18, 1887, for 1 inch δH=0·0280 foot grain unit.
„ 1 cm. „ =0·00051 C.G.S. unit.

With regard to magnetic disturbances, no very exceptional movements have been registered during the year.

The principal oscillations, however, were recorded on the following dates : November 2 to 6, 1886; February 13 and 14, April 5 to 7, August 2, and September 26 and 27, 1887. Much interest was evinced in the curve for February 23, which registered the occurrence of an earthquake.

In February last new adjusting screws were fitted to one of the microscopes attached to the Kew dip-circle No. 33.

Information on matters relating to terrestrial magnetism and various data have been supplied to Professor Mascart, Professor Adams, Dr. Atkinson, Professor Schering, Dr. Rijckevorsel, and Messrs. Archbutt and Stanley.

Professors Rücker and Thorpe visited the Observatory in January last, and made a series of base observations, prior to their departure for Ireland to finish their magnetic survey of the British Isles, which was commenced in 1883. On returning to England a further series of observations were made at Kew in October in order to complete the survey.

The monthly observations with the absolute instruments have been made as usual, and the results are given in the tables forming Appendix I of this Report.

The following is a summary of the number of magnetic observations made during the year :—

Determinations of	Horizontal Intensity........	28
„	Inclination................	110
„	Absolute Declination........	40

Several additional sets of observations of Absolute Declination have been made with the view of investigating certain changes in the values of the torsional effect of the suspending thread upon the determination of the true position of the magnet employed.

The magnetograph curves made use of in the preparation of the tables of diurnal range of Declination (see Appendix, Table III) have been drawn from the original photographs by means of an eidograph kindly lent by Captain W. J. L. Wharton, F.R.S., the Hydrographer.

Magnetic Reductions.—At the request of Professor Balfour Stewart, F.R.S., copies of the Kew declination disturbances for the years 1858–1865, together with the daily wind values for the years 1858 to 1869, have been made and forwarded to him; the Rev. S. J. Perry has also received copies of the records of certain selected days of magnetic disturbance for 1886.

Krakatoa Eruption.—In May last, at the request of the Krakatoa Committee of the Royal Society, a memorandum was prepared for that body on the magnetic effects recorded at the various observatories over the globe which occurred at the time of the great explosion of August 27, 1883, in the Straits of Sunda.

Magnetic Stations.—A list of all known magnetic stations has been prepared jointly by General Sir J. H. Lefroy and the Superintendent for publication by the Committee of the British Association on magnetic reductions, and will be published in the Annual Report for the current year.

It contains references to all localities on the surface of the globe where continuous observations of terrestrial magnetism have been made for periods of at least one month in duration, and gives, together with the geographical position of the stations, references to the publications where the results of such observations are to be found, as well as the names of the authorities, whenever these could be ascertained.

Falmouth Magnetographs.—At the request of the Secretary of the Royal Cornwall Polytechnic Society, the specifications for the magnetographs supplied to the Falmouth Observatory last year, which were drawn up by Mr. Whipple, have been revised and printed with illustrations in the Annual Report of that Society for 1886.

Sectional Lines.—In addition to the sectional lines obtained for the purpose of plotting down magnetic observations on the international scale, as suggested by General Sir J. H. Lefroy, and as mentioned in last report, the Committee have had a number of copies struck off from the stone on tracing paper for the use of observers who may desire to make tracings of existing curves on the same scale.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of atmospheric pressure, temperature, and humidity, wind (direction and velocity), bright sunshine, and rain, have been maintained in regular operation throughout the year.

The standard eye observations for the control of the automatic records have been duly registered during the year, together with the daily observations in connexion with the U.S. Signal Service synchronous system. A summary of these observations is given in Appendix II.

The tabulation of the meteorological traces has been regularly carried on, and copies of these, as well as of the eye observations, with notes of weather, cloud, and sunshine have been transmitted to the Meteorological Office.

The following is a summary of the number of meteorological observations made during the past year:—

Readings of standard barometer	2540
,, dry and wet thermometers	3465
,, maximum and minimum thermometers	730
,, radiation thermometers	880
,, rain gauges	730
Cloud and weather observations	1877
Measurements of barograph curves	8740
,, dry bulb thermograph curves	9395
,, wet bulb thermograph curves	8665
,, wind (direction and velocity)	17242
,, rainfall curves	680
,, sunshine traces	2182

In compliance with a request made by the Meteorological Council to the Committee, Mr. Whipple visited and inspected during his vacation the Observatories at Falmouth and Valencia, and the Anemograph at Mountjoy Barracks, Dublin.

Mr. Baker also visited the Aberdeen and Stonyhurst Observatories for the purpose of inspection.

With the sanction of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to, and published by 'The Times' and 'The Torquay Directory.' Data have also been supplied to the Council of the Royal Meteorological Society, the editor of 'Symons's Monthly Meteorological Magazine,' the Secretary of the Institute of Mining Engineers, Captain Abney, Messrs. Gwilliam, Rowland, and others. The cost of these abstracts is borne by the recipients.

The standard barometer (Adie 657) was fixed in the magnetograph room adjacent to the barograph, and read five times daily at observation hours, in order to compare its indications with those of the standard barometer in another part of the building.

Readings were continuously made from January 1 to July last, and are now under discussion.

Turf has been laid down under the screen of the thermograph with a view to avoiding effects of radiation as much as possible.

The use of meteorological self-recording instruments having been partially discontinued at Armagh, Mr. Whipple dismounted and packed the barograph and thermograph, and they have been returned for storage to the Observatory.

Electrograph.—The new quadrant electrometer, constructed on Mr. de la Rue's principle, with Professor Clifton's improvements, together with a chloride of silver battery of 60 cells, for the purpose of maintaining the potential of the quadrants at a certain point, gave great satisfaction during the year, and was found to be a marked improvement upon the older form of the instrument.

On September 2, during a high wind, a part of the instrument was accidentally set on fire by the gas-burner, and the apparatus narrowly escaped destruction.

Before re-starting the instrument it is proposed to make some minor alterations, suggested by experience, in the recording apparatus, &c.

The portable Thomson electrometer (White No. 53) having been put in thorough order, has been lent, in accordance with instructions received from the Meteorological Council, to the Hon. Ralph Abercromby, for the purpose of making observations on the Peak of Teneriffe.

Mr. Abercromby visited the Observatory for the purpose of familiarising himself with the use of the instrument, the scale value having previously been redetermined, by the kindness of the Chairman, at his laboratory in Portland Place.

III. Solar Observations.

The sketches of Sun-spots, as seen projected on the photoheliograph screen, have been made on 180 days, in order to continue Schwabe's enumeration, the results being given in Appendix II, Table IV.

Transit Observations.—347 observations of solar and 80 of sidereal transits have been taken, for the purpose of keeping correct local time at the Observatory, and the clocks and chronometers have been compared daily.

The following clocks, French, Shelton K. O., Shelton 35, and the chronometers, Molyneux No. 2125, Breguet No. 3140, and Arnold 86, are kept carefully rated as time-keepers at the Observatory.

The mean-time clock, Dent 2011, was bolted to the wall of the chronometer-room for use in daily comparisons with the chronometers on trial.

Old Solar Observations.—The library of the Observatory has received a present from Wm. J. Davies, Esq., of a MS. volume of sun-spot observations made at Edmonton, Middlesex, from August, 1819, to March, 1823. It is intended to enumerate the spots after the Schwabe method, so as to carry the Observatory catalogue of the new groups of sun-spots back to 1819.

Kew Solar Photographs.—At the request of the Chairman, the MS. sun-spot measurements and reductions from February, 1862, to December, 1863, together with the tables for computing the spotted positions, as well as the Kew working catalogues from 1864 to 1872, have been forwarded to Mr. A. L. Soper for the purpose of further discussion.

IV. Experimental Work.

Photo-nephograph.—The cameras used in cloud photography having been put in order, and had new adjustable rapid shutters fitted, were again brought into use, and by request of the Meteorological Council 24 sets of photographs comprising 90 negatives were taken on 14 days, chief attention being directed to the photographing of high cirrus clouds.

Prints of all the pictures have been made on cyanotype paper, which together with the observational data have been transferred to the Meteorological Office for the reduction and computation of cloud heights and velocities, Professor Stokes's cloud projection apparatus having also been transferred there for the purpose.

Solar Radiation.—The observations of the black bulb thermometers in vacuo made during 1886 were reduced and discussed, and the results found to be no more satisfactory than those obtained in previous years, the vacua in all of the instruments having deteriorated, and their readings having become lowered during the time they were under observation, whilst the readings differed considerably amongst themselves.

The Chairman having undertaken to submit the tubes to a lengthy exhaustion, three instruments were fitted with new jackets and sealed on to the air-pump in his laboratory.

They were there exhausted almost daily, the atmospheric pressure being reduced to and maintained at about 0·06 M from April to the end of September. On October 1st they were removed from the laboratory and replaced on the stand at the Observatory, having been read daily ever since.

Large differences are still found to exist in the readings of the similar and similarly placed instruments.

Pendulum Experiments.—The Indian Pendulum Apparatus, returned from the United States by Professor Peirce, was put up in the pendulum room specially erected for its accommodation in the South Hall of the Observatory, and certain preliminary swings made in the presence of both General Walker and Colonel Heaviside, R.E., which sufficed to show that the apparatus had not undergone any material changes since it left Kew in 1881.

It was, however, found that the vacuum chamber had received such structural damage in transit as to render it incapable of exhaustion to a sufficiently high degree to make the observations comparable with those previously made by Captains Basevi and Heaviside. It was accordingly returned to the maker, Mr. Adie, of London, for thorough repair, and has recently been again erected in its place, and found in a very satisfactory condition, so that the required preliminary observations may now be re-commenced.

By the kindness of Mr. W. H. Preece, F.R.S., the Committee were favoured with the loan of a recording chronograph for use in registering the coincidences. Experience proved that it was unnecessary, and the apparatus has since been returned to the General Post Office.

At the suggestion of Colonel Heaviside, photographs of the invariable pendulums were obtained on their removal from their cases after travelling, in order that a memorandum might be preserved of their figure and shape on their return to the Kew Observatory.

V. Verification of Instruments.

The following magnetic instruments have been purchased on commission and their constants determined:—

An Inclinometer for the Tokio University, Japan.
An Inclinometer for the Mauritius Observatory.
1 Collimating Magnet for Professor F. Brioschi, Rome.
2 Collimating Magnets and an Inclinometer for Professor Naccori, Turin University, Italy.
1 Magnet for declination and a pair of Inclinometer needles for Lisbon Observatory.

The total number of other instruments compared in the past year was as follows:—

Air-meters	5
Anemometers	3
Aneroids	83
Artificial Horizons	2
Barometers, Marine	89
,, Standard	4
,, Station	26
Compasses	2
Carried forward	251

Brought forward	251
Hydrometers	274
Inclinometers	3
Magnets	6
Rain Gauges	7
Range Finders	17
Sextants	145
,, Shades	52
Sunshine Recorders	2
Theodolites	11
Thermometers, Arctic	98
,, Avitreous	2641
,, Chemical	95
,, Clinical	8668
,, Deep sea	35
,, Meteorological	1370
,, Mountain	30
,, Solar radiation	9
,, Standards	43
Unifilars	4
Total	13,761

Duplicate copies of corrections have been supplied in 75 cases.

The number of instruments rejected on account of excessive error, or which from other causes did not record with sufficient accuracy, was as follows:—

Thermometers, clinical	64
,, ordinary meteorological	50
Various	232

5 Standard Thermometers have also been calibrated, and supplied to 4 societies and individuals during the year.

There are at present in the Observatory undergoing verification, 21 Barometers, 462 Thermometers, 2 Hydrometers, and 10 Sextants.

The Committee, after considering the question of certifying the various and numerous classes of instruments on the hydrometer principle, have authorised the Superintendent of the Observatory to refuse to verify any instruments except such as either indicate specific gravities directly or whose indications bear a known and well-defined relation to specific gravities.

Several patterns of range-finders, for use both at sea and on shore, have also been tested; two additional movable adjustable collimators with scales having been fitted to the sextant testing apparatus, to enable it to be used for this purpose.

Aneroid Altitude Scales.—The Committee have also had their attention called to the fact that several scales are in use for graduating the scales of heights upon the dials of aneroids. Upon consideration the Committee recommend as desirable the employment of Sir G. B. Airy's scale, published in 1867, and the equivalent metric scale, in all cases where possible.

Two highly sensitive aneroids with Bourdon tubes substituted for the ordinary corrugated vacuum boxes, constructed by MM. Richard, Frères, of Paris, have been examined for constancy at the request of the Meteorological Council. It was found, however, that the metal of the tubes underwent changes similar to those experienced by the usual aneroid boxes, and the instruments were therefore liable to the same errors as the common aneroid.

A *Beckley Rain Gauge* was obtained, fitted with a Stonyhurst discharger, and, after due trial, sent to the Mauritius Observatory, where an opportunity will be afforded of noting the behaviour of the improved instrument during tropical rains.

Hygrometers.—Dr. Doberck, the Director of the Hong Kong Observatory, having obtained a Royal Society grant for the purpose of re-calculating Hygrometric tables, Mr. Whipple procured and forwarded to him examples of Alluard's, Crova's, and Dines' instruments, after making preliminary observations at Kew.

Hot Springs.—Mr. G. J. Symons, F.R.S., having received a grant from the Government Grant Fund of the Royal Society, for the purpose of investigating the temperatures of certain hot springs in the South of France and the Pyrenees, had a number of specially prepared thermometers carefully verified at the Observatory, both before and after he had made use of them abroad.

Anemometer Constants.—At the request of Colonel Knight, F.R. Met. Soc., a number of comparisons with the standard Anemometer were made of a small Robinson's Anemometer he had constructed, having cups fitted to arms of variable length, and the moving parts provided with friction rollers.

Anemographs.—Mr. Whipple has, at the request of the Imperial Chinese Customs, superintended the construction of a new Beckley Anemograph for use at Formosa, as well as a similar instrument for Dr. A. S. Viegas, of the Coimbra Observatory, and two smaller instruments for the Meteorological Council.

Navy Telescopes.—By the kindness of the Astronomer Royal the Kew Observatory has been favoured with descriptions of the tests applied to Navy telescopes supplied by contractors to H.M. Service, and also with descriptions of the apparatus employed at the Royal Observatory, Greenwich, for applying the tests.

A standard Admiralty telescope has been purchased, and the necessary appliances are now being constructed, with the view of enabling the Committee to apply similar tests to telescopes submitted to them for verification by opticians and others.

VI. Rating of Watches.

The arrangements for rating watches mentioned in previous Reports have been carried on during the year with continued success, and up to the present 1344 watches have been examined and reported upon.

510 entries of watches were made as contrasted with 490 during the corresponding period of last year. They were sent for testing in the following classes:—

For class A, 463; class B, 25; and class C, 22.

Of these 174 failed to gain any certificate; 19 passed in C, 21 in B, 296 in A, and 13 of the latter obtained the highest possible form of certificate, the class A *especially good.*

In Appendix III will be found statements giving the results of trial of the 26 watches which obtained the highest numbers of marks during the year, the premier position being attained—with 88·1 marks—by a keyless, double-roller, going-barrel watch, submitted by Jos. White, Earlsdon, Coventry.

This total exceeds that of last year, and it is also extremely satisfactory to note that a very marked increase has taken place in the number of watches which have gained more than 80 marks.

As some inconvenience was caused by the employment of temporary expedients to maintain the large watch-safe at an average of 65° F. for the "middle-temperature" test, a burner was procured and fitted up with a shield, and the safe can now be kept at the desired point, whilst at the same time no deleterious fumes of coal-gas can penetrate into the interior chamber.

The three rating safes are therefore now maintained by means of gas and ice at practically the three constant temperatures of 40°, 65°, and 90° F. respectively, all the year round.

Special attention continues to be given to the examination of *pocket chronographs*, in accordance with the request of the Cyclists' Union.

Rating of Chronometers.—Since the institution of chronometer trials, as mentioned in last year's Report, 27 movements have been examined, and certificates issued giving the mean daily rate and variation of rate at each change of temperature.

The trial occupies 35 days, divided into 5 periods of 6 days each, and 5 intermediate days, namely, 1 day at the commencement of each period of test:—

1st period.	Chronometer at temperature of	55° F.	or	13° C.
2nd „	„ „	70°	„	21° „
3rd „	„ „	85°	„	29° „
4th „	„ „	70°	„	21° „
5th „	„ „	55°	„	13° „

Certificates are granted to chronometers which have undergone 35 days' test as specified above, and whose performance is such that:—

1. The mean of the differences in each stage of the examination, between (*a*) the average daily rate during that period, and (*b*) the several daily rates, does not exceed one second in any one of the stages.

2. The mean daily rate has not been affected by change of temperature more than one-sixth of a second per 1° F., which is about a quarter of a second per 1° C.

3. The mean daily rate has not exceeded ten seconds in any stage of the test.

A Kullberg's temperature regulator has been fitted by the maker to the chronometer oven, and a Richard thermograph is also arranged to work in the case with the chronometers, affording a continuous record of the temperatures which they have experienced during the whole of their trial.

The range of temperature from 55° to 85° F., to which the marine chronometers are submitted, has been decided upon after careful consideration, as being amply sufficient for determining the behaviour of chronometers under conditions to which they are usually exposed at sea, and no serious objections have yet been received from makers or others to the adoption of the above range.

VII. Miscellaneous.

Photographic Paper, &c.—This has been supplied to the Observatories at Coimbra, Colába, Falmouth, Lisbon, Mauritius, Stonyhurst, and Toronto. It has also been supplied to the Meteorological Office, the U.S. Navy Department, and others.

Anemograph sheets have, in addition, been forwarded to Madras, and blank forms for the entry of observations to several persons.

Extension of Building.—The Committee having decided on building an additional floor on the east wing of the Observatory, for the purpose of providing increased space for carrying on the Observatory work, now very much cramped, have obtained from the Council the promise of a loan of £200 if needed.

Application has therefore been made to the Chief Commissioner of Works and Public Buildings for permission to proceed with the work, but the reply granting leave has not yet been received.

Mr. Whipple has recently designed two new simple forms of maximum pressure anemometers, which he exhibited before the Royal Meteorological Society, and described in a paper read on April 20th.* They were also shown at the Falmouth Exhibition of the Royal Cornwall Polytechnic Society in September.

Stevenson's Screen.—The Stevenson's screen fixed on the lawn was blown over during the gale of September 2nd last, but fortunately the injury done by the accident was very trivial, as the screen was not in use at the time.

Exhibitions.—Photographic curves and pictures have been by request shown at the Exhibition at Newcastle-upon-Tyne and the Royal Jubilee Exhibition, Manchester.

Library.—During the year the library has received as presents the publications of—

26 Scientific Societies and Institutions of Great Britain and Ireland, and

98 Foreign and Colonial Scientific Establishments, as well as numerous private individuals;

the special thanks of the Committee being due to Dr. Neumayer, the Director of the Deutsche Seewarte, Hamburg, for a complete set of the publications of that establishment since 1876.

Workshop.—The machine tools procured for the use of the Kew Observatory by grants from the Government Grant Fund or the Donation Fund, have been kept in thorough order.

House, Grounds, and Footpath.—These have all been kept in order during the year.

PERSONAL ESTABLISHMENT.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant.
H. McLaughlin, Librarian.
E. G. Constable, Observations and Rating.
W. Hugo, Verification Department.
M. Baker, Messenger and Care-taker, and nine other Assistants.

(Signed) WARREN DE LA RUE, *Chairman.*

November 25th, 1887.

* See 'Quart. Jour. Roy. Met. Soc.,' vol. 13, p. 224.

The Kew Observatory. Account of Receipts and Payments for the year ending October 31st, 1887.

Dr. **RECEIPTS.**

	£	s.	d.	£	s.	d.
To Balance from 1885–86	£576	14	6			
Royal Society (Gassiot Trust)	490	8	7			
,, (Pendulum)	150	0	0			
Meteorological Office (Allowance)	400	0	0			
Experimental Work	61	19	3			
Magnetic Work	28	1	4			
Miscellaneous	88	3	6			
Verifications	824	3	0			
Rating of Watches and Chronometers	405	5	4			
				3024	15	6
Meteorological Office for Postages and Porterages				26	2	5
Commissions executed for Colonial and Foreign Institutions, &c.				570	18	0
				£3621	15	11

PAYMENTS. *Cr.*

	£	s.	d.	£	s.	d.
Salaries and Extra	£1397	2	3			
Gas, Fuel, Rent, Insurance, &c.	188	3	4			
Stationery, Postages, Caretaker, &c.	131	15	3			
Instruments, Apparatus, &c.	44	4	10			
Experimental work	34	1	2			
Miscellaneous	50	7	7			
Exhibition	2	1	9			
Verifications	109	14	1			
Rating of Watches and Chronometers	114	16	7			
Pendulum	98	0	9			
				2170	7	7
Postages and Porterages, &c., for Meteorological Office				30	0	3
Commissions executed for Colonial and Foreign Institutions, &c.				645	16	5
Balance—Bank of England	681	11	2			
London and County Bank	87	14	10			
Cash in hand	6	5	8			
				775	11	8
				£3621	15	11

November 10, 1887. Examined and compared with the Vouchers, and found correct. (Signed) GEO. HENRY RICHARDS, *Auditor.*

ASSETS.

	£	s.	d.
By Balance as per Statement	775	11	8
Meteorological Office, Allowances, Experimental, and Sundries	100	17	3
Verification Fees due, &c.	88	9	4
Rating of Watches, &c.	59	7	11
Photographic Paper	7	4	2
Commissions, &c.	21	7	1
Blank Forms	25	19	3
Standard Thermometers in Stock	89	4	0
	£1168	0	8

LIABILITIES.

	£	s.	d.
To Gas, Fuel, and House Account	6	17	6
Balance, Pendulum Account	40	10	1
Chemicals, &c.	4	15	11
Purchases on Commission	3	0	0
Balance	1112	17	2
	£1168	0	8

November 11, 1887. (Signed) G. M. WHIPPLE, *Superintendent.*

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N. Long.* $0^h\ 1^m\ 15^s{\cdot}1$ *W., for the year October* 1886 *to September* 1887.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnets 101 B and N E being employed for the purpose.

The Dip observations were made with Dip-circle Barrow No. 33, the needles 1 and 2 only being used; these are $3\frac{1}{2}$ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000{,}000{,}213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0{}^2}\right)$, is −0·00186.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected 1,250 feet north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, has been carefully determined.

The observations have been made and reduced by Mr. T. W. Baker.

Table I.

Observations of Dip or Inclination.

Month.	Mean Inclination.
1886.	
November 2......	67° 37′·5
3......	67 39·0
Mean......	67 38·3
November 25......	67 37·2
26......	67 37·7
Mean......	67 37·5
December 28......	67 40·5
29......	67 39·4
30......	67 38·0
Mean......	67 39·3
1887.	
January 26......	67 38·0
28......	67 36·6
Mean......	67 37·3
February 21......	67 39·0
22......	67 37·1
23......	67 38·4
Mean......	67 38·2
March 22......	67 37·8
23......	67 37·1
Mean......	67 37·5
1887.	
April 25......	67° 37′·9
27......	67 35·4
28......	67 35·5
Mean......	67 36·3
May 24......	67 36·8
26......	67 37·5
Mean......	67 37·2
June 24......	67 35·9
25......	67 35·7
Mean......	67 35·8
July 26......	67 35·6
28......	67 35·0
Mean......	67 35·3
August 26......	67 35·6
29......	67 36·2
Mean......	67 35·9
September 22......	67 35·7
24......	67 37·5
Mean......	67 36·6

Table II.

Observations for the Absolute Measurement of Horizontal Force.

Month.	Log $\frac{m}{X}$ mean.	Log mX mean.	Value of m*.
1886.			
November 4th	9·12210	0·30690	0·51820
,, 29th	9·12069	0·30786	0·51794
December 31st..........	9·12105	0·30767	0·51804
1887.			
January 27th...........	9·12141	0·30785	0·51836
February 24th	9·12077	0·30799	0·51806
March 28th	9·12078	0·30799	0·51806
April 25th	9·12060	0·30816	0·51806
May 31st	9·12063	0·30835	0·51819
June 29th and 30th......	9·12080	0·30824	0·51823
July 29th	9·12061	0·30815	0·51806
August 30th and 31st....	9·12052	0·30766	0·51771
September 23rd	9·12003	0·30803	0·51764

Table III.—Solar Diurnal Range of the Kew Declination as derived graphically from selected quiescent days.

Hour.	Summer mean.	Winter mean.	Annual mean.
1887.			
Midnight	−0′·9	−1′·6	−1′·3
1	−1·0	−1·3	−1·2
2	−1·4	−1·3	−1·4
3	−1·8	−1·4	−1·6
4	−2·2	−1·3	−1·8
5	−3·0	−1·2	−2·1
6	−4·2	−1·4	−2·8
7	−4·8	−2·0	−3·4
8	−4·4	−2·3	−3·4
9	−3·3	−2·3	−2·8
10	−0·4	−0·9	−0·7
11	+3·7	+1·4	+2·6
Noon	+6·3	+3·4	+4·9
13	+6·4	+3·9	+5·1
14	+5·8	+3·3	+4·6
15	+4·1	+2·1	+3·1
16	+2·2	+1·1	+1·6
17	+1·0	+0·4	+0·7
18	+0·2	+0·3	+0·3
19	0·0	+0·1	0·0
20	−0·2	−0·3	−0·3
21	−0·3	−0·8	−0·6
22	−0·5	−1·4	−1·0
23	−0·8	−1·5	−1·2

* m = magnetic moment of vibrating magnet.

Table IV.

Month.	Declination.	Magnetic Intensity.								
		English Units.			Metric Units.			C. G. S. Measure.		
	Mean of Observations.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
	West. ° ′ ″									
1886. October	18 13 38	3·9120	9·5093	10·2825	1·8038	4·3846	4·7441	0·1804	0·4385	0·4744
November	18 14 1	3·9227	9·5288	10·3046	1·8087	4·3936	4·7513	0·1809	0·4394	0·4751
December	18 10 33	3·9202	9·5372	10·3115	1·8076	4·3974	4·7545	0·1808	0·4397	0·4755
1887. January	18 7 11	3·9194	9·5197	10·2946	1·8072	4·3893	4·7467	0·1807	0·4389	0·4747
February......	18 12 18	3·9229	9·5350	10·3105	1·8088	4·3964	4·7540	0·1809	0·4396	0·4754
March	18 14 28	3·9228	9·5293	10·3050	1·8083	4·3938	4·7515	0·1809	0·4394	0·4752
April	13 13 20	3·9245	9·5238	10·3008	1·8095	4·3913	4·7495	0·1810	0·4391	0·4750
May..........	18 15 3	3·9252	9·5326	10·3091	1·8098	4·3953	4·7534	0·1810	0·4395	0·4753
June	18 13 5	3·9239	9·5185	10·2956	1·8093	4·3888	4·7471	0·1809	0·4389	0·4747
July..........	18 11 52	3·9244	9·5157	10·2932	1·8095	4·3875	4·7460	0·1810	0·4388	0·4746
August	18 11 50	3·9226	9·5161	10·2928	1·8036	4·3877	4·7458	0·1809	0·4388	0·4746
September	18 11 31	3·9265	9·5310	10·3043	1·8104	4·3946	4·7529	0·1810	0·4395	0·4753

APPENDIX II.

Meteorological Observations.—Table I.

Mean Monthly results.

Months.	Thermometer.								Barometer.*					Mean vapour-tension.
	Mean.	Means of—			Absolute Extremes.				Mean.	Absolute Extremes.				
		Max.	Min.	Max. and Min.	Max.	Date.	Min.	Date.		Max.	Date.	Min.	Date.	
1886.	°	°	°	°	°	d. h.	°	d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Oct.	53·1	59·1	47·5	53·3	76·7	4 2 P.M.	37·3	22 8 A.M.	29·799	30·432	29 10 P.M.	28·643	16 9 A.M.	·336
Nov.	43·7	48·4	38·2	43·3	58·6	1 3 P.M.	27·6	24 8 "	29·926	30·739	24 9 A.M.	28·964	6 10 "	·251
Dec.	36·5	41·7	31·4	36·6	53·3	6 1 P.M.	17·7	20 8 "	29·711	30·599	31 10 "	28·312	9 5 "	·184
1887.														
Jan.	35·7	39·8	31·3	35·6	51·4	19 2 "	14·9	2 7 "	30·019	30·695	21 11 "	28·838	5 9 "	·189
Feb.	38·9	44·8	33·3	39·1	53·8	5 2 "	20·7	17 8 "	30·337	30·759	7 Noon.	29·753	2 7 P.M.	·193
March..	38·1	44·3	32·8	38·6	55·7	27 3 "	23·8	19 7 "	30·083	30·662	2 9 A.M.	28·991	23 8 A.M.	·181
April...	43·8	52·4	36·4	44·4	64·5	19 4 "	27·4	17 6 "	30·010	30·737	17 7 "	29·310	24 9 "	·204
May ...	50·0	57·1	43·3	50·2	67·6	8 4 "	34·8	1 5 "	30·019	30·429	8 10 "	29·269	20 6 "	·271
June...	60·2	69·9	50·8	60·4	81·0	15 6 "	44·1	27 4 "	30·197	30·493	10 {11 P.M. & Midt.}	29·556	3 {1 & 2 A.M.}	·360
July ...	65·2	75·6	54·9	65·3	85·3	3 4 "	45·3	18 {4 & 5 A.M.}	30·050	30·393	1 1 A.M.	29·578	27 4 A.M.	·403
Aug.	61·5	71·7	52·1	61·9	84·5	6 4 "	41·5	15 5 "	29·988	30·389	3 8 "	29·461	31 6 "	·358
Sept. ...	54·0	60·7	47·3	54·0	67·8	5 {2 & 4 P.M.}	32·9	29 7 "	29·944	30·493	19 9 "	29·266	2 7 "	·320
Means..	48·4	55·5	41·6	48·6	..	....	..	....	30·007	..	....	..	....	·271

The above Table is extracted from the "Hourly Readings," vols. 1886-87, of the Meteorological Office, by permission of the Meteorological Council.

* Reduced to 32° at M.S.L.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall *.			Weather. Number of days on which were registered								Wind †. Number of days on which it was								
		Total.	Maximum.	Date.	Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	Calms.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Variable.
1886.		in.	in.																		
October ..	7	2·310	0·385	12	23	..	..	..	3	16	1	8	2	4	5	2	4	6	5	1	2
November	7	2·845	0·570	10	17	..	..	..	3	14	..	6	3	4	3	..	5	7	3	2	3
December	5	3·465	1·270	26	20	3	1	..	9	8	2	7	3	2	..	..	2	10	5	4	5
1887.																					
January ..	8	1·465	0·545	3	17	7	..	..	4	19	..	11	3	1	1	2	10	6	3	2	3
February .	6	0·570	0·190	17	6	..	..	..	9	11	1	6	2	7	5	2	1	8	1	1	1
March ...	6	1·705	0·405	15	12	3	..	..	4	9	1	7	3	6	4	1	1	2	3	6	5
April	5	1·310	0·315	23	12	..	1	..	5	9	2	2	9	6	1	..	4	6	1	2	1
May	8	1·680	0·300	3	20	..	1	1	1	17	..	..	11	4	1	..	1	3	4	4	3
June	5	1·130	0·570	2	4	..	..	..	11	7	..	3	7	5	5	..	1	6	3	1	2
July	5	0·820	0·295	24	10	..	..	2	7	2	..	5	3	2	2	1	5	12	1	2	3
August ..	5	2·680	1·135	17	10	..	..	2	8	7	..	8	4	2	5	..	6	6	3	3	2
September	7	2·165	0·425	16	13	..	..	1	3	15	1	5	7	2	1	..	1	12	4	2	1
Totals..		22·145			164	13	3	6	67	134	8	68	57	45	33	8	41	84	36	30	31

* Measured at 10 A.M. daily by gauge 1·75 feet above surface of ground. † As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*.)			Minimum temperature on the ground.			Horizontal movement of the Air.*		
	Total number of hours recorded.	Percentage of possible sunshine.	Greatest daily record.	Date.	Mean.	Highest.	Date.	Mean.	Lowest.	Date.	Average hourly Velocity.	Greatest hourly Velocity.	Date.
1886.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
October	77 42	23	8 30	2	93	122	4	42	29·4	22	10	37	24
November	45 54	17	6 12	4	72	100	1	33	23·4	19	8	33	6
December	71 54	29	6 12	25	65	82	8	26	14·5	20	13	42	8
1887.													
January	36 30	14	6 54	26	58	90	5	27	10·8	2	8	34	3
February	67 42	24	7 48	26	75	102	25	29	18·3	18	12	36	3
March	98 6	26	9 36	13	79	109	26	29	17·9	17	10	43	23
April	170 42	41	12 36	20	103	120	19	31	19·2	18	14	38	{ 1, 6
May	137 42	29	13 54	15	105	126	9	39	27·3	15	11	33	20
June	245 12	50	15 48	13	122	137	8	44	32·3	27	9	25	17
July	280 54	56	14 18	3	132	141	12	46	33·6	19	9	29	27
August	240 54	53	12 54	9	125	140	7	43	29·2	15	8	27	31
September	119 6	31	11 18	8	111	123	7	41	26·9	25	10	39	2

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

Table IV.

ummary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days without spots.
1886.			
October	14	4	4
November	8	2	6
December	13	4	5
1887.			
January	10	3	3
February	12	4	4
March	11	4	4
April	17	2	7
May	16	5	2
June	21	5	0
July	23	6	5
August	20	3	8
September	15	2	12
Totals	180	44	60

APPENDIX III.—Table I.

RESULTS OF WATCH TRIALS. Performance of 26 Watches which obtained the highest number of marks during the year.

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate. — +Gaining. −Losing.	Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference of mean daily rate: Between pendant up and dial up.	Between pendant up and pendant right.	Between pendant up and pendant left.	Between dial up and dial down.	Difference between extreme gaining and losing rates.	Marks awarded for: Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	Total Marks. 0—100.
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
Jos. White, Coventry	29999	Single overcoil, *d.r., *g.b.	+0·4	0·4	0·03	−0·5	+1·3	+0·6	+0·4	4·5	32·7	37·7	17·7	88·1
Baume & Co., London	2646†	Single overcoil, d.r., g.b.	+2·4	0·5	0·02	−1·5	−2·1	−2·6	−1·8	4·25	30·0	36·2	18·9	85·1
W. Holland, Rockferry	3563	Single overcoil, d.r., fusee	+3·3	0·6	0·003	−0·2	−1·1	+0·5	+2·8	5·25	27·1	36·2	19·8	83·1
Stauffer & Co., London	120186	Single overcoil, d.r., g.b., bar-lever	+3·8	0·4	0·04	+0·8	−4·5	−1·1	−1·1	6·25	31·4	34·3	17·2	82·9
Donne & Son, London	1545	Duo-in-Uno, d.r., resting barrel	−1·1	0·6	0·03	−0·1	+0·6	+2·0	−0·5	5·0	27·9	37·0	17·8	82·7
D. Buckney, London	30191	Single overcoil, d.r., fusee	+0·8	0·5	0·02	−0·8	+3·9	+2·7	+3·7	6·75	30·4	33·1	18·9	82·4
Baume & Co., London	2528	Single overcoil, d.r., g.b., bar-lever	+3·4	0·4	0·10	−1·3	−1·3	−0·8	+1·8	6·5	31·5	37·4	13·2	82·1
Stauffer & Co., London	123174	Single overcoil, d.r., g.b., bar-lever	+1·9	0·7	0·03	+0·1	−1·6	+0·8	−0·8	5·25	26·8	37·2	17·9	81·9
Stauffer & Co., London	111353	Single overcoil, d.r., g.b.	+1·5	0·5	0·03	−4·7	−1·3	+0·2	+3·0	8·5	29·0	34·6	18·0	81·6
Stauffer & Co., London	122288	Single overcoil, d.r., g.b.	+0·9	0·6	0·06	+2·3	+2·7	+0·7	−0·8	4·5	28·7	36·6	16·3	81·6
Stauffer & Co., London	114351	Single overcoil, d.r., g.b.	+2·4	0·4	0·05	+2·4	−2·8	+3·9	+0·7	8·25	32·9	31·4	17·0	81·3
Baume & Co., London	2529	Single overcoil, d.r., g.b., bar-lever	+4·3	0·5	0·06	−0·2	−3·5	−1·7	−0·4	6·75	29·8	35·4	16·0	81·2
Jas. White, Coventry	29026	Single overcoil, s.r., g.b.	−0·9	0·6	0·06	+0·6	−1·3	−2·8	−1·4	6·0	28·6	36·2	16·3	81·1
Jas. White, Coventry	29024	Single overcoil, s.r., g.b.	+0·4	0·6	0·02	−4·7	−0·4	−1·4	+3·6	8·5	27·5	35·0	18·5	81·0
Stauffer & Co., London	123173	Single overcoil, d.r., g.b., bar-lever	+1·9	0·5	0·04	−1·7	−3·0	−5·3	+1·9	7·75	30·9	33·0	17·1	81·0
Stauffer & Co., London	122289	Single overcoil, d.r., g.b.	+3·6	0·6	0·03	+4·1	+1·5	−0·3	−1·4	7·75	28·8	34·1	18·0	80·9
Usher & Cole, London	25738	Flat spring, s.r., g.b.	+0·5	0·7	0·03	−1·5	−2·9	−0·8	+1·6	6·75	26·5	36·3	18·0	80·8
R. Crook, London	899	Single overcoil, s.r., g.b.	−2·3	0·6	0·04	−0·8	+2·3	−3·0	+0·1	7·25	28·4	34·8	17·6	80·8
Baume & Co., London	2530	Single overcoil, d.r., g.b., bar-lever	+1·5	0·4	0·10	+1·4	+1·0	+3·8	−2·5	7·0	31·8	35·4	13·5	80·7
Stauffer & Co., London	123158	Single overcoil, d.r., g.b., bar-lever	−4·8	0·4	0·09	−0·4	+0·4	+3·1	−1·0	8·0	31·2	35·5	14·0	80·7
Stauffer & Co., London	120184	Single overcoil, d.r., g.b., bar-lever	−1·0	0·5	0·05	+0·8	−2·0	−3·7	−1·2	6·0	29·6	34·3	16·7	80·6
A. P. Roger, Guernsey	123172	Single overcoil, d.r., g.b.	−0·5	0·6	0·01	+1·0	−0·7	−3·4	−4·6	8·0	28·5	33·0	19·1	80·6
J. Player, Coventry	24619	Single overcoil, d.r., g.b.	+5·7	0·6	0·08	+0·1	−0·7	−0·1	−0·8	5·75	27·1	38·6	14·7	80·4
H. Golay, London	14776	Double overcoil, d.r., g.b.	+3·0	0·4	0·05	+0·5	−4·6	+2·2	−2·5	7·5	32·0	31·8	16·5	80·3
A. E. Fridlander, Coventry	52465	Single overcoil, d.r., g.b.	+2·8	0·4	0·09	+1·2	−2·1	−2·2	−0·2	5·75	31·0	34·5	14·6	80·1
Stauffer & Co., London	120842	Single overcoil, d.r., g.b., bar-lever	+1·6	0·5	0·08	+4·0	+1·3	+1·9	−0·6	6·5	30·8	34·9	14·4	80·1

* d.r., double-roller; s.r., single-roller; g.b., going barrel.

† Split seconds chronograph without minute-recorder.

Table II.

Highest Records obtained by Complicated Watches during the year.

Description of watch.	Number.	Deposited by	Marks awarded for			Total marks, 0—100.
			Variation.	Position.	Temperature.	
Minute and seconds chronograph and repeater ..	14768	H. Golay, London	32·1	34·2	10·7	77·0
,, ,, ,, ,, ..	14750	H. Golay, London	27·7	33·3	11·3	72·3
Perpetual calendar and repeater	01922	S. Smith and Son, London	26·1	34·2	16·7	77·0
,, ,, ,, ...	14759	H. Golay, London	28·3	35·5	10·7	74·5
Split-seconds and minute-recorder chronograph .	9818	E. R. Shipton, London	26·6	34·5	17·6	78·7
,, ,, ,, ,, .	07369	H. Golay, London	28·8	30·2	14·7	73·7
Split-seconds chronograph (without minute-dial)	2646	Baume and Co., London......	30·0	36·2	18·9	85·1
Ordinary minute and seconds chronograph.....	2427	Baume and Co., London......	27·6	35·8	16·4	79·8
,, ,, ,, ,, ,, ...	123820	Stauffer and Co., ,, ...	27·8	32·4	16·9	77·1
,, ,, ,, ,, ,, ...	2426	Baume and Co., ,, ...	28·0	33·2	15·5	76·7
Repeaters	31578	H. Capt, Geneva	30·0	34·3	14·2	78·5
,, ...	14752	H. Golay, London	26 8	33·9	13·8	74·5

APPENDIX IV.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument	1869
The Science and Art Department, South Kensington..	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Lieutenant A. Gordon, R.N.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. One Bifilar Magnetometer. One Declinometer. Two Tripod Stands.	1883
General Sir H. Lefroy, R.A., F.R.S.	Toronto Daily Registers for 1850–3	1885
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.	1883
Professor O. J. Lodge	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Mr. W. F. Harrison.	Condensing lens and copper lamp chimney ..	1883
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885
Professor Rücker ...	Tripod Stand	1886
Lord Rayleigh......	Standard Barometer (Adie, No. 655)	1885
University of Japan..	Barrow's Inclinometer, No. 24.............	1887

December 8, 1887.

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

Mr. William Whitaker was admitted into the Society.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.
Sir William Bowman.
Dr. Frankland.
Sir G. H. Richards.
The Earl of Rosse.

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

The following Papers were read:—

I. "On the Bone in Crocodilia which is commonly regarded as the Os Pubis, and its representative among the Extinct Reptilia." By H. G. Seeley, F.R.S., Professor of Geography in King's College, London. Received October 24, 1887.

Normally three elements enter into the construction of the pelvic girdle, each of which unites with the other two, and contributes to the formation of the acetabulum for the femur. The ilium and ischium are always more or less ossified, but sometimes the pubis remains represented by cartilage throughout life. Among the Amphibia the pubis is often in this cartilaginous state in its living representatives, so that only two bones and a cartilage usually contribute to form the articular cup for the femur. Among Urodeles the pubic cartilage is perforated by a foramen, which corresponds with the foramen in the ossified pubis of a lizard, and appears to carry the obturator nerve; so that its identification with the pubis is established. But the pubis and ischium are often connate; by which term I designate that embryonic state in which no division of the primitive cartilage into separate elements has occurred. It is not quite so evident that the similarly placed cartilage in certain Anura is homologous with that of Urodeles, since it is not perforated in the same way; and it becomes smaller in aged specimens by the ischium encroaching upon it. It is absent in the genus Hyla; such absence, considered with the presence of a pubis in Dactylethra, would support the conclusion that the pubis and ilium are connate in Anura.

In the young of Crocodilia the same three elements contribute to the formation of the articular cup for the femur, the ilium above, the ischium behind, and a cartilage in front.

Professor Hoffmann* regarded this cartilage as the pubis, and then the bone which is anterior to it, and had previously been identified as the pubis, became the pre-pubis.

Professor Huxley questioned this identification, and regarded the bone as the ossified part, and the cartilage apparently as the unossified portion of the pubic element of the pelvis.† This cartilage is well known to decrease in dimensions with the age of the crocodile, but it does not disappear by augmenting the extent of the supposed pubic bone; though if it were really a pubic cartilage, its ossification should cause the anterior bony element to extend into the acetabulum. But instead of disappearing in this way, ossification has the effect of continuing to exclude the supposed pubic bone from the acetabulum; so that the cartilage in the young animal does not give rise to a separate osseous element in the adult, but disappears by so ossifying as to approximate the ischium and ilium. At first there is a gap between the ischium and ilium anteriorly, which the cartilage fills, but eventually in old animals these bones almost meet each other, and the cartilage is ossified. Thus in the adult animal the acetabulum is formed by two bones, one being the ilium, and the other in the position otherwise occupied by the ischium and pubis. Therefore it follows, either (1) that the pubic cartilage, if originally distinct in the young, becomes incorporated by ossification with the ilium and ischium; or that (2) the pubis in crocodiles does not enter into the acetabulum, but is a pre-acetabular ossification. I am aware of no exception to the law that the pubis contributes to the os innominatum when it has a separate existence, and therefore it seems to me more probable to suppose that ischium and pubis should be connate, like the same elements in some salamanders, and undivided, than that crocodiles should so differ in plan of the skeleton, as to have the pubis removed from connexion with the ilium, and from the acetabulum. If the former view prevails, then the acetabular cartilage in crocodiles is never a pubic cartilage, but only the unossified part of the ilium or ischio-pubic bone which

* 'Niederl. Archiv Zool.,' vol. 3, 1876, p. 144.

† 'Roy. Soc. Proc.,' vol. 28, 1879, p. 394. Prof. Huxley's language is not quite clear on this point. He says (p. 398) "It is the osseous portions of the pubes which are commonly described as the entire bone." "These apparently anomalous elements of the pelvis are readily moveable upon their fibro-cartilaginous connexions with the acetabulum. But in no essential respect do they differ from ordinary pubes."

occupies the place of the pubis, and so it is manifest that if the pubis enters into the acetabulum in crocodiles, it must be found in the anterior process of what is commonly named the ischium, which fills the anterior corner of the pelvic basin. And since no exception to this position of the pubis among vertebrates is known, by which it articulates with the ilium, the conclusion is legitimate, in the absence of evidence to the contrary, that a bone which does not unite with the ilium by bony union, and which is carried in front of a process which occupies the usual position of the pubis, cannot be the pubic bone. It would be equally unprecedented for the pubis and ischium to be connate in a reptile, were it not that this condition is already established in the existing Amphibia, and were there not strong reasons for regarding the crocodiles as descended from extinct allies of the Amphibian class; while in an early stage of development all the pelvic elements in crocodiles are connate. Hence I concur with Professor Huxley in regarding Professor Hoffmann's identification of the pelvic acetabular cartilage as the pubis as untenable.

Professor Hoffmann regards the bone which is commonly identified as the pubis as being the pre-pubis.* Professor Huxley, on the other hand, regards it as being a portion of the pubis, and bases his identification mainly on the relations of this bony element to the pelvic muscles. There is an *à priori* objection to the latter interpretation, because it would introduce a joint in the middle of the pubis, making one part connate with the ischium, and the other part a free bone. Of such a condition I am not aware that vertebrate osteology offers any example, and the improbability against it being a true interpretation seems to be great. That a bone may ossify from several centres is evident from the bones of Mammalia and certain young birds and lizards; but in crocodiles none of the bones are thus characterised, and therefore I can only conclude with Hoffmann, that the ossification is a distinct element of the skeleton, which is connected with the pubic portion of what I term the ischio-pubic bone, and is in the position of the pre-pubic bone; but it does not necessarily follow that it is identical with the pre-pubic cartilage, which has been termed by Hoffmann and Huxley the epipubis. For the cartilage, whether in Amphibians or Mammals, is developed from the median line of the pubic symphysis; while in crocodiles the pubes do not form a symphysis, because they are not developed distally, and the pre-pubic ossification is situate immediately below the acetabulum, in the position of the pectineal process; so that while the Amphibian cartilage is in the position of the marsupial bones of mammals, the crocodilian

* Professor Haughton ('Ann. Mag. Nat. Hist.,' vol. 1, 1868, p. 282) bases the nomenclature of the bones on the muscular anatomy, and terms the reputed pubis the marsupial bone; the ischium then becomes the pubis; while the ilium is the ilium in its anterior part, and the ischium in the posterior part.

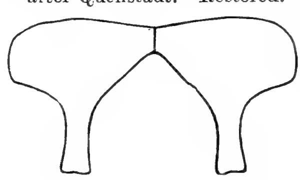
T 2

bone corresponds in position with the pre-pubic bone of Ornithosaurs, which is always developed on the anterior pubic border towards the acetabulum, at some distance from the pubic symphysis. The extinct allies of the Crocodilia may throw some light upon the nature of this ossification.

First, in the Teleosauria the bone is much more slender and less expanded at its anterior end, in all the species from the Lias and Lower Oolitic rocks. The diminution in size of the bone in Oxford clay representatives of the group is more marked, and in some undescribed types in the collection of A. Leeds, Esq., is reduced to a mere bony style without any expansion at either end, comparable in form and substance to a lucifer match. One stage more of diminished development would obliterate the bone altogether, but no such condition has yet been discovered. It has the same osseous attachment as in Crocodilia, and the ischio-pubic bone is of the same type in Teleosauria as in Crocodiles.

In the Ornithosauria the plan of the pelvis is different. First, there is the complete ossification of the three constituent bones, with the ilium prolonged in front of the acetabulum as well as behind it, the ischium and pubis united by symphysis, and all three bones contributing to form the imperforate acetabulum. Secondly, there is a pre-pubic ossification in front of the pubis, with a narrow attachment below the acetabulum. These pre-pubic bones vary in form in the different genera. In Dimorphodon they are triangular; in Cycnorhamphus they are shaped like a capital T, and so expanded anteriorly that

Pre-pubic bones of *Cycnorhamphus suevicus*, after Quenstadt. Restored.



a, attachment to the pubic bones.

the cross bars from the two sides met in the median line; in Rhamphorhynchus the shape is T, an inverted capital L, but the bones of the two sides are united together into a transverse bow-shaped bar. Other modifications are found in the group, but the most common are approximations to the form of the pre-pubic bones of crocodiles. Since these bones have the same relations to the pubes which the pre-pubic bones of crocodiles exhibit, I regard them as being homologous, and if the distal part of the Ornithosaurian pubis were not developed, the homology would be supported by a similar plan of pelvic construction.

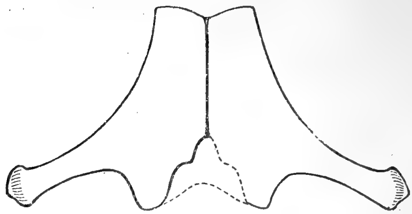
Finally, there is some evidence that a similar structure is developed in the abdominal region of the allies of Iguanodon, without being in direct union with the pelvis. As long since as 1841 Dr. Mantell, F.R.S., figured in the 'Philosophical Transactions' (Pl. 8, fig. 2) an undetermined Wealden bone, which is now in the British Museum, registered as No. 2218. It is not quite perfect, but is suggestively similar to the pre-pubic bones of Ornithosaurs. A thick sutural surface shows that it met a similar bone in the median line. Subsequently Mr. S. H. Beckles, F.R.S., obtained another specimen into which two such bones entered as constituents, which was exhibited during the British Association Meeting at Brighton, in 1872, and ultimately described and figured by Mr. J. W. Hulke, F.R.S.*

Bones, like that figured by Mantell, have long been in the British Museum. And Professor Cope has figured a pair of similar bones in *Diclonius mirabilis.*† Mr. Hulke interpreted Mr. Beckles' specimen as consisting of the clavicles and interclavicle. Mr. W. Davies, F.G.S., had interpreted the isolated bones like Mantell's No. 2218, as clavicles, and his determination had been accepted by Professor O. C. Marsh; so that until M. Dollo regarded them as parts of the sternum they had been regarded as clavicles. Dr. George Baur, of Yale College, subsequently in 1885‡ suggested that the supposed sternal apparatus of Iguanodon should be turned round, so that the supposed clavicles would become posterior processes of the sternum, and this view has been supported by Cope and adopted by myself. I suppose that the interpretation of the Beckles specimen was a consequence of its condition of preservation, by which fractures came to simulate sutures. Those fractures which are assumed to limit the clavicles, so as to allow the supposed interclavicle to extend between them, follow very different courses from the natural limits of the bones. If Mantell's fossil already referred to is superimposed upon Mr. Beckles' specimen, then it is manifest that the broad part of the specimen is made up of two such bones which meet by median suture, and extend for two-thirds the length of the specimen. Some trace of the transverse suture may perhaps be seen which separates these bones from a thin ossification which extends beyond them. Hence there can be no interclavicle between the supposed clavicles; and the evidence for the identification of the clavicles disappears. Turning the specimen so that the supposed clavicles point posteriorly, they will be found to make a remarkable approximation in form to the pre-pubic

* 'Geol. Soc. Quart. Journ.,' vol. 41, 1885, Pl. XIV, p. 473.

† 'American Naturalist,' Feb. 1886, p. 154.

‡ 'Zoologischer Anzeiger,' No. 205, p. 561.



? Pre-pubic bones of Iguanodon, restored from No. 2218, Brit. Mus.

bones of the crocodile. Professor Huxley* figured the ventral aspect of a *Crocodilus acutus* at about the close of embryonic life, in which a considerable fibrous development was found anterior to each pre-pubic bone. This condition varies with age. In a large adult crocodile from Abyssinia, in the British Museum, the fibro-cartilages anterior to the pre-pubic bones are united in the mesial line, anteriorly, so as to form one mass. A similar condition is seen in younger specimens. And if such a structure were sufficiently ossified to be preserved in a fossil state, it would closely reproduce the form of the anterior portion of Mr. Beckles' fossil, while the marks of lateral attachment in this part of the fossil correspond with the grooves for the last pair of abdominal ribs. Therefore I regard this specimen as probably representing in the Iguanodon the pre-pubic bones of crocodiles, as well as the cartilage connecting them with the ribs, which Huxley terms the epi-pubis. On its mode of attachment to the pubis I offer no suggestion, and there is no evidence available. But since the bone in crocodiles and Ornithosaurs is attached in the region of the pectineal process, it is probable that it is connected with the extension of the pectineal process in Dinosaurs, which Professor O. C. Marsh has named the pre-pubis, and I would bring the pubic bone in the Ornithischia into harmony with the process in Crocodiles by suggesting that the distal part of the bone, like the anterior process, is entirely absent, so that only the sub-acetabular part which supports the pre-pubis remains in crocodiles; and I suppose that if the pubis had been prolonged distally in the crocodile, it might have included the foramen for the obturator nerve.

If this interpretation of Dr. Mantell's undetermined bone should be sustained, it would contribute a new and distinctive element to the Iguanodont pelvis, as remarkable as the pubic modification in an Ornithosaur or Crocodilian, and distinct from either. The evidence from the fossil allies of crocodiles by no means demonstrates the nature of what I have termed the pre-pubic bones, though it shows that pre-pubic ossifications exist which cannot be confounded with the pubis, which may resemble the Crocodilian bone in form and in

* 'Roy. Soc. Proc.,' vol. 28, Pl. 8.

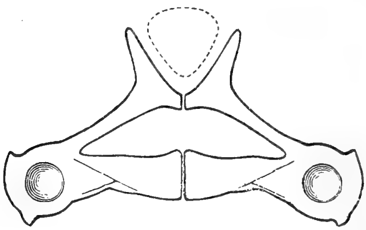
being anterior in position to the acetabulum, and in similar isolation from the ilium.

In the current identification of this bone every consideration has been made subordinate to the embryological evidence as stated by Rathke. Yet although his excellent description unaccompanied by figures has been regarded as conclusive that the bone under discussion is the pubis, it seems to me necessary to reconsider the evidence before the matter can be thus settled. The pelvis of a crocodile in that stage of development which corresponds with the middle period of incubation, if I rightly interpret the author's meaning, is apparently more like that of an emu than is the adult animal in so far as the ischium and pubis are concerned; while the relative shortness of the pubis is suggestively Iguanodont.

Rathke's statement is as follows: the ilium, ischium, and pubis of each side unite to form a single unbroken cartilaginous mass. The two ilia are short, rather broad, plates, as in the full-grown animal, and extend somewhat outward beyond the transverse processes of the sacral vertebræ. The ischia were also similar to those of the full-grown animal, consisting of tolerably thick plates, somewhat expanded transversely at their median union, but are not so broad in proportion to their length as in the full-grown crocodile. The pubis was somewhat shorter than the ischium, and in proportion to the other parts of the pelvis was much shorter than in later life, and not directed so much forward. It extended downward nearly parallel to the ischium, almost along its whole length, only separated from it by a small interspace, and uniting with it at its upper end. Ventrally the pubes are widely separated, and have the hinder small half of the connexion with the yolk-sac opening between them. They preserved a similarity in shape to that of the mature crocodile, but the distal ends were not so wide in proportion to their length, and the other parts are not so slender as in later life. And on a subsequent page the author again remarks, "The direction of the pubis in embryonic life remains different to that of the adult, but in the middle of the embryonic period there comes to be a division in the cartilaginous plate which hitherto had represented the ilium, ischium, and pubis."

The early condition of the pelvic elements is so interesting in its parallelism of the ischium and pubis, and in the subsequent change of direction of the reputed pubis, that I applied to Professor W. K. Parker, F.R.S., for help. He at once sent me three examples of *Crocodilus palustris*, one about mature in the egg, another with the head 3 cm. long, the body about 4·5 cm., and the tail about 7 cm., which was about half grown, and a smaller specimen about a third grown.

On examination I found that the pubes in the half-grown specimen were developed as in the adult, even to the fibrous extensions in front of the bones and behind them, and the only important difference was the presence of the median notch between their anterior corners for the hinder half of the yolk sac and a direction rather less anterior. In the smallest specimen the element which Rathke terms the pubis does not diverge anteriorly from the ischium to the same extent as in the adult, but the elements have the usual form and meet in the middle line, and the difference is unaccompanied by any divergence



Pelvic cartilage of *Crocodilus palustris*, showing the relations of the pre-pubic plates to each other when one-third developed in the egg.

of plan in the pelvis. The unsegmented cartilage is a different kind of evidence from the same cartilage divided into elements which become permanent as ossifications. And I hold segmentation which occurs subsequently at a distance from the acetabulum, which isolates a bone from the pubic region of the pelvis and the ilium, to be evidence that the structure so separated is pre-pubic; while the only other segmentation separates the ilium from the bone which supports the pre-pubis. From this it follows that the pubis has never been distinct from the ischium, and is not developed distally in crocodiles. The notch which defines the anterior part of the ischio-pubic bone corresponds with the great notch in the acetabulum of the mammalian pelvis, and that notch is situate between the ischium and pubis. The cartilage which completes the anterior margin of the acetabulum seems to me to be inseparable from the ilium; and to be continuous at its border with the fibrous sheath for the head of the femur. I should attribute its existence to the absence of distal development of the pubis, because this would remove the usual pressure of the bone upon the anterior corner of the acetabulum, which stimulates ossification of the ilium.

If the interpretation thus made is morphologically sound, it has an important bearing upon the affinities and classification of the Crocodilia.

II. "The Post-embryonic Development of *Julus terrestris*." By F. G. HEATHCOTE, M.A. Communicated by ADAM SEDGWICK, F.R.S. Received November 16, 1887.

(Abstract.)

With regard to the development of the cœlom and generative organs, I have obtained the following results. The somites divide into two parts, as described for *Strongylosoma* by Metschnikoff, one part remaining in the body and the other part projecting into the legs. The cavities in these two parts together constitute the cœlom. The part within the legs breaks up and the cells give rise to muscles. The part within the body passes dorsalwards along the thin sheet of mesoblast which unites it to its fellow of the other side, so that the two vesicle-like parts meet above the nerve-cord in the middle line. They join so as to form a single tube, the generative tube. The young ova, as well as the follicle cells surrounding them, are formed by cells proliferated from the walls of this generative tube. The body parts of the somites of the antennæ and mandibles break up and disappear, but those of the third pair of appendages give rise to the pair of salivary glands. There are two pairs of somites to each double segment.

In the development of the nerve-system, I find that there are two cerebral grooves formed as in *Peripatus*. They disappear early in the development. The ventral nerve-system, which at first consists of two separate cords united by a thin median part, undergoes a process of concentration which results in the presence of a single stout cord showing slight traces of its former double condition. At an early period of development there is a cavity present in each ganglion. This cavity soon disappears, leaving no trace. Two ganglia are developed to each double segment.

The tracheæ are formed as epiblastic invaginations at the sides of and rather behind the legs. These invaginations swell out inside the body so as to form two vesicles, and as the development proceeds two diverticula are given off from each vesicle, one running beneath the nerve-cord to meet its fellow of the other side, the other running dorsally, parallel to the body-wall. Both these diverticula break up to form the tracheal tubes, the remaining part of the vesicle forming the tracheal pit. There are two pairs of these tracheal invaginations to each double segment.

The stink glands are formed as invaginations of the epiblast, and a second coat (muscular) is added later in the development. There is only one pair to each double segment.

The heart is formed from mesoblast cells in the body cavity.

These cells which were directly derived from the hypoblast in the early stages of development, form a network in the body-cavity. The heart is the result of a joining together of the meshes of this network, and thus is formed by the confluence of a series of spaces in the mesoblast, and has nothing to do with the development of the cœlom. The heart is placed in the middle dorsal line between the gut and the body-wall. It has two pairs of arteries leading into the spaces of the fat body in each double segment, and two pairs of ostia. The part of the body-cavity in which it lies is shut off from the rest of the body-cavity by an imperfect pericardial membrane which is continuous with the fat bodies. The tube of the heart is composed of three coats, an inner structureless membrane, a median muscular coat, the fibres of which are disposed circularly in alternate broad and narrow bands, and an outer connective tissue coat. The fat bodies are also formed from the same network of mesoblast cells which in this case secrete oil globules.

The body-cavity is a series of spaces between the gut and the body-wall, and is divided up by the mesoblast cells already referred to. It is distinct from the cœlomic cavities of the somites, and is therefore a pseudocœle.

The eye-spots are all formed in the same manner. The hypodermis thickens and a cavity appears within it bounded by pigment. This cavity becomes a distinct vesicle. The front wall of the vesicle becomes very thin and furnishes the lens, while the cells of the back (*i.e.*, most internal) wall and sides become elongated and form the retinal elements of the eye. The nuclei of the front wall become very faint and finally disappear, while the rest of the vesicle remains continuous with the hypodermis of the body-wall. The cells of the vesicle are at first separate from the ganglion cells of the nerve-system, but a connexion takes place very early. A number of very small cells appear within the walls of the vesicle at a very early period, and I believe them to be derived from the mesoblast cells in the body-cavity, but of this I am not certain. They eventually become the pigment cells described by Grenacher.

The most striking feature of the development is the reduction of the ventral part of the young animal and the increase of the dorsal. In the just hatched animal the ventral region is nearly as large as the dorsal, and the legs are wide apart, having a distinct space between them. As development progresses the dorsal region is increased, while the ventral is contracted till the bases of the legs are close together. The corresponding concentration of the nerve-cord I have already mentioned. In a paper on *Euphoberia*, a Carboniferous Myriapod, Mr. Scudder points out that one of the principal points in which the genus differs from existing Diplopoda is the development of the ventral region. The relations of the dorsal and ventral regions of the body of the *Euphoberia* correspond exactly to the condition of the young *Julus*.

With regard to the double segments of *Julus*, Newport held that each double segment corresponded to two segments originally distinct which had fused together; subsequent writers have held that each double segment is a single segment which has developed a second pair of legs. Now considering the double segments with regard to the development as well as to the adult condition, we see that the mesoblastic segmentation is double, so are the tracheal, the nervous, and circulatory systems. The only part of these double segments which is single is the dorsal plate with its stink glands which arise as invaginations in it; this dorsal plate being so enlarged as to form a complete ring round the body of the adult. Looking at the palæontology, we find that in the Archipolypoda, a family including the Archidesmidæ, Euphoberidæ and Archijulidæ, the dorsal plate did show distinct traces of a division. Therefore I think that each double segment represents two complete segments, the dorsal plates of which have fused together to make one plate.

III. "On the Sexual Cells and the early Stages in the Development of *Millepora plicata*." By SYDNEY J. HICKSON, M.A. Cantab., D.Sc. Lond., Fellow of Downing College, Cambridge. Communicated by Professor M. FOSTER, Sec. R.S. Received November 19, 1887.

(Abstract.)

The investigations were made upon several specimens of *Millepora plicata* I found growing in abundance on the fringing reefs of Talisse Island, N. Celebes.

The young sexual cells, both male and female, are found in the ectoderm of the cœnosarcal canals, between the dactylozooids and the gastrozooids.

At an early stage they leave the ectoderm, and by perforating the mesoglœa take up a position in the endoderm.

The ova at an early stage become stalked. The stalk of the ovum, which is simply a modified pseudopodium, serves to keep the ovum attached to the mesoglœa.

The stalk may at times be completely withdrawn, and the ovum by amœboid movements migrate along the lumen of the canal to a more favourable locality, where it becomes again attached to the mesoglœa by a stalk.

Before maturation the germinal vesicle disappears, and a spindle-shaped body with longitudinal striæ appears, which throws out the first polar globule.

A second and larger spindle appears after the first polar globule is thrown out, which in its turn discharges the second polar globule.

The mature ova of *M. plicata* are very small ($\frac{1}{100}$ mm. in diameter), and contain no yolk globules or granules.

After maturation the ova are impregnated. The heads of two or three spermatozoa may be seen within a single ovum, the flagella remaining on the surface.

After fertilisation the germinal vesicle is again apparent, and at a later stage it is seen to contain a number of nucleoli.

The germinal vesicle next fragments, the fragments being scattered over that pole of the ovum which previously contained the germinal vesicle, *i.e.*, the pole nearest to the stalk.

The fragments at a later stage travel towards the middle of the ovum, where they form an equatorial zone.

This equatorial zone of fragments divides into two parties, which travel towards the poles.

The fragments during these movements increase in size and in number, and in the next stage observed they are scattered over the whole ovum.

This stage corresponds with the morula stage of other embryos, the fragments of the original germinal vesicle being the nuclei of its constituent cells. Very faint markings in the substance of the embryo indicate the outlines of the cells.

The embryo next assumes the form of a solid blastosphere, in which stage it migrates into the gastrozooid, and its subsequent history is lost by its being discharged most probably by the mouth to the exterior.

No trace of any medusa or medusiform gonophore or sporosac was found either on the dactylozooids or gastrozooids containing ova or embryos.

The young male sexual cells or spermospores are at an early stage distinguished from the young ova by their large nucleus containing a coarse protoplasmic meshwork.

The nucleus fragments and the fragments soon come to occupy the whole spermospore.

The spermospore is matured in the canals, and then migrates into the basal endoderm of the dactylozooids, where its wall disappears, and a colony of young spermoblasts pass into the cavity of the zooid. These push out the wall of the zooid into the form of sporosacs, which they occupy until they are mature. The sporosacs do not seem to be formed before the advent of the spermoblasts. There is no spadix nor any other indication of their being degenerate medusiform gonophores. In a very few cases they were found in the gastrozooids.

The origin of the sexual cells in Millepora support the views of the Hertwigs and Weismann that the ectoderm is the original seat of the sexual cells in the Hydrozoa.

The absence of segmentation may probably be accounted for by the migratory habits of the embryo after development has commenced. The fact that no sperm-morula is formed supports this view. The evidence before us does not support the view that the ovum of Millepora formerly contained much yolk and has subsequently lost it.

I am inclined to believe that the Hydrocorallinæ belong to a separate stock of the Hydrozoa, which probably never possessed medusiform gonophores. Millepora is not related to Hydractinia.

IV. "On Photometry of the Glow Lamp." By Captain ABNEY, R.E., F.R.S., and Major-General FESTING, R.E., F.R.S. Received November 21, 1887.

In a paper which we read before the Royal Society ('Roy. Soc. Proc.,' No. 232, 1884) it was shown when a carbon filament or a platinum wire *in vacuo* was gradually raised in temperature, that the different rays in the visible and invisible regions of the spectrum followed a law governing their intensity.

In the dark region of the spectrum (below the red) if the abscissæ to a curve represented watts (current × potential), and the ordinates the intensity of the ray under consideration, the curve so formed was hyperbolic, approaching more nearly to the parabolic form as the red was approached. In the visible spectrum the parabolic curve was actually reached, the vertices of the parabolas moving along the axis of abscissæ; the shift being greater the more refrangible the rays under consideration. This implied that until a certain number of watts had been expended the ray was absent. Further, we had shown in the 'Philosophical Magazine' for September, 1883, that when measured by a thermopile,

$$\text{total radiation} \propto (\text{watts} - \text{constant}).$$

In the visible radiation of an incandescent filament in a glow lamp we are only dealing, however, with a small portion of the radiation, and therefore could not expect it to follow such a simple law as that which governs total radiation. It appeared probable, however, that as the intensity of any individual ray in this part of the spectrum increased parabolically, the sum of all the visible rays ought also to follow very closely the same form of curve, the vertex of such parabola lying at some point in the axis of abscissæ between the vertices of the parabolas of the extreme visible rays. It likewise appeared probable that when the rays of extreme refrangibility were absent or in defect, as is the case when the filament is red hot, the parabola would fail to represent the intensity of visible radiation.

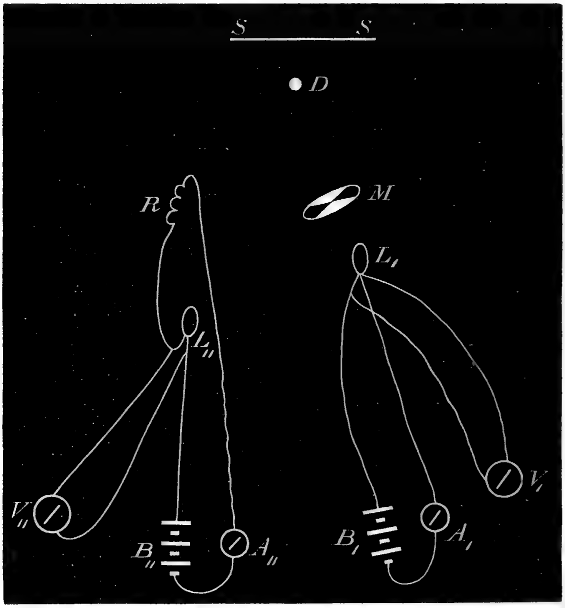
In the communication we have already referred to one example of the applicability of the parabolic formula was given, for white light, but by itself it was hardly conclusive. We, therefore, conducted a series of experiments to ascertain if our anticipations were correct.

An incandescence lamp was selected as a standard lamp, through which a fixed current was maintained. This we used instead of a standard candle or other variable light. We then selected a second similar lamp, of which to measure the light when currents of various strengths were passed through it.

The shadow and grease-spot methods were both experimented with, the former being perhaps the most exact. Whichever method is, however, employed, it was inexpedient to move either lamp towards or from the source, or to vary the distance of the source from the lamp, as the carbon filaments show more or less illuminating surface to the screen according as they are close or distant from it. It therefore became necessary to adopt some other plan for altering the intensity of the light falling on the source from the comparison lamp.

In the Rumford (shadow) method, fig. 1 will give the general idea of the arrangements.

FIG. 1.



The shadows cast by the rod D from the two sources of light, L_1 and L_{11}, were made just to touch each other, on the white screen SS, and to fall within a rectangle cut out of black paper, which deadened the light on the rest of the screen. Each lamp (L_1 and L_{11}) was in connexion with an ampère-meter and volt-meter (A_1, A_{11} and V_1, V_{11}). In front of L_1 (the comparison lamp) was placed an electromotor which caused a pair of sectors of variable aperture to rotate between it and the screen.

Evidently two methods are open to equalise the illumination of the screen from each source:—

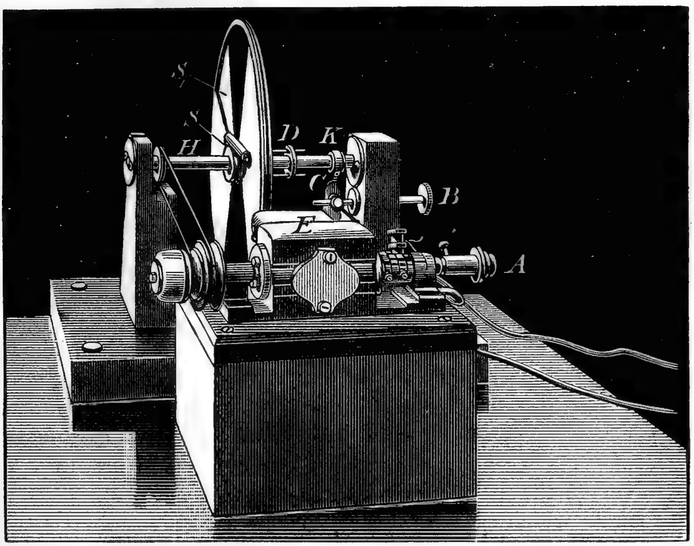
1st. Cutting off more or less light from L_1.

2nd. Varying the current in L_{11} by means of the variable resistance in the circuit.

The first plan necessitates the opening and closing of the sectors whilst rotating, and the second the alteration of the resistance, &c., at will. Whichever method was adopted the lamp L_1 was brought to a bright yellow glow, and the lamp L_{11} had a current passed through it which, when the minimum resistance was in circuit in R, produced a brilliant white light. Such intense heat the filament would not be able to stand for any considerable time.

When measurements were to be taken by the first plan the instrument shown in the annexed diagram, fig. 2, was employed. A pair of

Fig. 2.

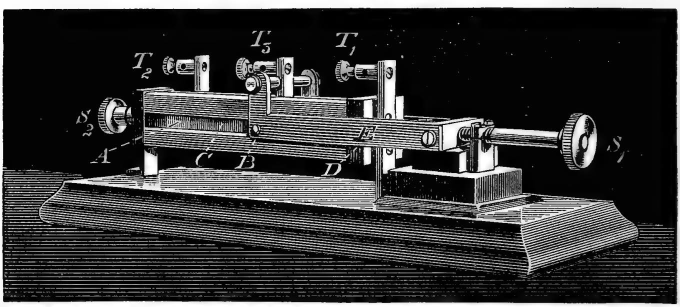


sectors, S (each of 90°), are mounted on a horizontal axis, a similar pair, S_1, are carried on a short sleeve, to which are attached two horizontal pins, passing through holes in the flange D of another sleeve C. A stud in this last engages with a screw thread of long pitch cut on the axis. A horizontal movement of C thus causes it, as well as the sectors S_1, to rotate with reference to the axis and the other sectors (S), and therefore alters the aperture. This movement is given by means of a vertical lever engaging with the groove K on C, and which is actuated by the screw B. The aperture can thus be varied between 0° and 180°, whether the instrument be in motion or at rest. The instrument has been described at length, as it varies in some particulars from previous ones, having been made under our own supervision. The edge of S is graduated into degrees so that the amount of aperture is at once known.

A certain current is passed through L_{11}, which is noted, and the sectors opened or closed till the shadows cast by the rod appear equally luminous. The motor is stopped and the aperture read off. Three or four readings for each current passing through L_{11} are taken, and then the current is altered, and a new set of three or four readings made. The current is altered so that the light from L_{11} varies from extreme brightness to a dull red.

By the other plan the sectors shown in fig. 2 are detached from the motor, and card disks placed on A. A resistance, R, fig. 3, has now

Fig. 3.



to vary to bring the light equal to the standard light diminished by the disks rotating between the lamp L_1 and the screen with known apertures. For the resistance at first we used a non-conducting tube, in which about forty spherical pellets of hard carbon were inserted. At one end of the latter was a brass plate to which one terminal of the battery was attached, and at the other a screw was inserted, which was attached to the other terminal of the battery through the lamp. This screw pressed the pellets together to any required degree, diminishing the resistance or increasing it as occasion required. This answered fairly well, but not so well as would be desired, as the response to the screw was somewhat sluggish. Mr. Varley supplied us with one of his carbonised cloth resistances, which consists essentially of a series of square pieces of carbonised cloth more or less in contact. The figure represents the one we had made for us. 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. When only one resistance has to be inserted, T_1 and T_3, T_1 and T_2, or T_1 and T_3 may be used for connecting on to the leads with one pole of the battery and the ampère-meter. It was in this way the resistance was used in the case in point. The lamp L_{11} by the use of this could be raised from black heat to bright white, and a small turn of the screw altered the resistance very considerably. We used two sets of sectors; one pair enabled us to use an aperture from 135° to 90°, and the other pair from 90° to 0°. The light from L_1 was diminished by the first pair of sectors being placed on A (fig. 2), the current $\propto$ potential being noted. The resistance of the current passing through L_{11} was then altered till the illumination of the two shadows on the screen appeared equal, the screw S (fig. 3) being turned backwards and forwards, first one shadow and then the other being made to appear too dark. By diminishing the oscillations the neutral point can be very readily arrived at, even though the colours of the lights may be very different (see Bakerian Lecture, 1886, "Colour Photometry," by the authors). The readings of V_{11} and A_{11} were then read and noted. The apertures of the sectors were altered, and the same operations gone through. From observations thus made the curves were constructed, enabling the theory propounded to be tested.

The grease-spot plan of photometry was arranged in a somewhat similar manner, L_{11} being on the opposite side of the screen SS, and the rod being abolished. In this method the room has to be dark so as to admit of no reflection. At first we were not prepared for any great exactitude with it, but finally we came to the conclusion that it was very reliable, a conclusion that Mr. W. H. Preece* came to when he constructed his photometric arrangements.

Having described the arrangements for taking the measurements, it remains to give the conclusions at which we arrived after making a large number of experiments.

* 'Roy. Soc. Proc.,' vol. 36, 1884, p. 270.

Let

W be the watts,
c ,, current,
p ,, potential,
y ,, intensity of light;

all other letters being constants.

In order for the curve of intensity to be parabolic

$$\text{(i.)} \quad W - m = n\sqrt{y},$$

m being the number of watts at which the vertex of the parabola lies.

From the equations given in the paper already referred to ('Phil. Mag.,' September, 1883), where

$$c = ap + bp^{3|2},$$

the above equation (i) may be written—

$$p^2(a + bp^{\frac{1}{2}}) - m = n\sqrt{y};$$

when p is fairly large this becomes—

$$\text{(ii)} \quad p^2 - h = k\sqrt{y} \quad \text{approximately.}$$

Similarly it may be shown that—

$$\text{(iii)} \quad c^2 - s = t\sqrt{y} \quad \text{approximately.}$$

The following tables will show the application of (i). It must be understood that the measures of current and potential are not given in ampères and volts, and that as a consequence the watts are only represented by watts × a constant. The first three tables show the exactitude of the method of measurement where the resistance is altered, and the fourth table the exactitude when the rotating sectors are altered.

Table I.—A Woodhouse and Rawson lamp, changing the resistance and reading current and potential.

C.	P.	Watts × a constant.	Aperture of disk.	Calculated aperture (shadow).	Calculated aperture (grease-spot).*
13·4	38·0	509·2	180	180·23	181·6
12·75	36·1	460·3	135	135·20	134·2
12·5	35·5	443·8	120	121·44	120·0
12·3	35·0	430·5	110	110·95	109·4
12·2	34·7	423·3	105	105·47	105·5
11·9	33·8	402·2	90	90·11	91·1
11·7	33·1	387·3	80	79·99	80·1
11·5	32·5	373·7	70	70·34	71·2
11·2	31·7	353·9	60	60·20	61·3
10·9	30·75	335·2	50	49·40	48·7
10·6	29·9	316·9	40	40·42	39·8
10·2	28·8	293·9	30	30·11	29·7
9·65	27·55	265·4	20	20·07	19·5
9·35	26·7	249·6	15	15·07	15·0
9·0	25·6	230·4	10	10·09	
8·4	24·2	203·3	5	4·75	

$m = 144.$ $n = 27·2.$

Table II.—Swan lamp-light measured by changing the resistance and reading current and potential.

C.	P.	Watts × a constant.	Aperture of disk.	Calculated aperture.
23·2	24·2	562·4	180	180·63
22·0	22·2	488·4	133†	132·71
21·6	21·6	466·6	120	119·89
21·4	21·4	458·0	115	114·91
20·6	19·9	410·0	90	89·14
20·2	19·2	387·8	79†	78·68
19·7	18·7	368·4	70	69·88
19·2	18·0	345·8	60	60·37
18·6	17·2	320·0	50	50·00
17·9	16·3	291·8	40	40·25
17·1	15·3	261·6	30	30·8
15·9	13·8	220·4	20	19·98
14·3	12·0	171·6	10	10·11
13·0	10·6	137·8	5	5·29

$m = 50·52$ $n = 38·0.$

The following shows the readings in full of one set of observations :—

* This column shows results given by the grease-spot method. The watts are not given, but merely the results, to enable a comparison to be made between the accuracy of the two methods.

† In these two the sectors were supposed to be set at 135 and 80 respectively, but after the set had been taken and the sectors stopped it was found they read as in the table.

Table III.

Current.					Potential.					Aperture.		
Readings.				Mean.	Readings.				Mean.	Watts × a constant.	Calculated.	Set on the sector.
1	2	3	4		1	2	3	4				
19·3	19·2	19·3	19·3	19·3	11·4	11·5	11·4	11·4	11·4	220	180	180
18·5	18·5	18·5	..	18·5	10·8	10·85	10·75	..	10·8	200	135·5	135
17·1	17·1	17·0	17·1	17·1	10·3	10·3	10·3	10·3	10·3	176	89·5	90
16·8	16·8	16·8	16·7	16·8	10·1	10·1	10·1	10·1	10·1	170	79·6	80
16·5	16·4	16·5	..	16·5	9·9	10	9·9	..	9·9	163	71·7	70
16·1	16·1	..	..	16·1	9·75	9·75	..	..	9·75	157	59·9	60
15·6	15·7	15·6	15·6	15·6	9·6	9·5	9·5	9·6	9·55	149	49·2	50
15·1	15·0	15·2	15	15·1	9·3	9·3	9·5	9·2	9·4	142	40·7	40
14·5	14·5	14·5	..	14·5	9·1	9·1	9·0	..	9·1	132	30·0	30
13·7	13·65	13·7	13·7	13·7	8·7	8·7	8·65	8·7	8·7	119	18·7	20
12·2	12·2	12·2	..	12·2	7·9	7·9	7·9	7·9	7·9	96·5	5·6	10
10·6	10·5	10·6	..	10·6	7·3	7·1	7·2	..	7·2	76·3	0·14	5

$m = 71·5.$ $n = 11·04.$

The following is an example of measuring by using known currents and cutting off more or less of the comparison light by the sectors. The observations have been given in full to show the deviation of individual observations from the mean :—

Table IV.

C.	P.	Watts × constant.	Aperture of disks to balance light.	Mean observed aperture.	Calculated aperture.
7·9	28·1	222	90*	90	91·2
7·75	27·4	212	81, 81, 80	80·7	80·6
7·6	26·8	204	72·5, 72·5, 72·5	72·5	72·5
7·25	25·5	185	56·5, 56, 56·0, 55, 55·5	55·8	55·6
7·00	24·8	174	46·5, 47, 46·5, 45·5	46·4	46·6
6·7	23·8	159	37, 35, 36, 36·	36·0	36·0
6·25	22·2	139	23, 24, 23·5, 24	23·8	23·6
5·85	20·6	120·5	14·15, 15, 14·5, 14	14·5	14·6
5·7	20·1	114	11·75, 11·75, 11·75	11·75	11·75
5·4	19·1	102	8, 8, 7·5, 8, 7·5	7·6	7·7
5·3	18·7	99	6·5, 6·5, 6·5, 7	6·6	6·8

$m = 53.$ $n = 17·7.$

The foregoing examples will give an idea of the accuracy with which measurements may be made by either method, and of the exactness with which the parabolic curve is followed. It seems that the photometry of incandescence lamps may be well carried out by measuring the watts. It may be objected that each observation requires readings of the galvanometers, but this is avoided by the use of the formula given in the beginning of this paper. Two observations of current and potential enable the constants to be calculated, and after that one galvanometer alone need be used; by preference that one giving comparative volts. The current is calculated from such a reading and subsequently the watts.

Mr. W. H. Preece, in his paper† already alluded to, came to the conclusion that the intensity of the light emitted from a glow lamp varied as the sixth power of the current. This formula is fairly exact within limits, but it is obviously empyric, since where the current is small enough only to cause dark radiation it must fail. The example that he gives would require some slight rectification before it can be used as in the method given above; since the small distances at which the candle he employed was placed from the screen make it necessary to apply corrections for the thickness and length of flame.

* The light was fixed so as to balance as nearly as possible when the sectors were at their full aperture.

† 'Roy. Soc. Proc.,' vol. 36, p. 270.

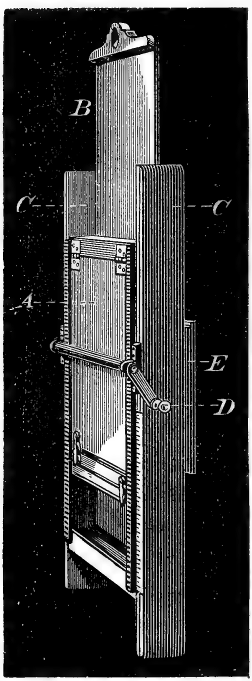
Mr. Preece's table is as follows; the last column is derived from the parabolic formula using C^2-s instead of $W-m = a\sqrt{y}$.

Table V.

Distance of source of light from illuminated surface.	Equivalent degree of illumination.	Current in lamp.	$C^6 \times 15 \cdot 994$.	Parabolic formula.
0·50 feet	64·000	1·260	64·000	64·00
0·75 ,,	28·445	1·100	28·335	32·83
1·00 ,,	16·000	0·959	12·442	16·00
2·00 ,,	4·000	0·790	3·888	4·41
3·00 ,,	1·778	0·690	1·726	1·78
4·00 ,,	1·000	0·651	1·217	1·00

It having been shown that the parabolic formula applies to visual measures of an incandescence light, it appeared that the same ought to hold good for the total light which is photographically active. These rays may be taken to lie between the blue and the extreme ultra-

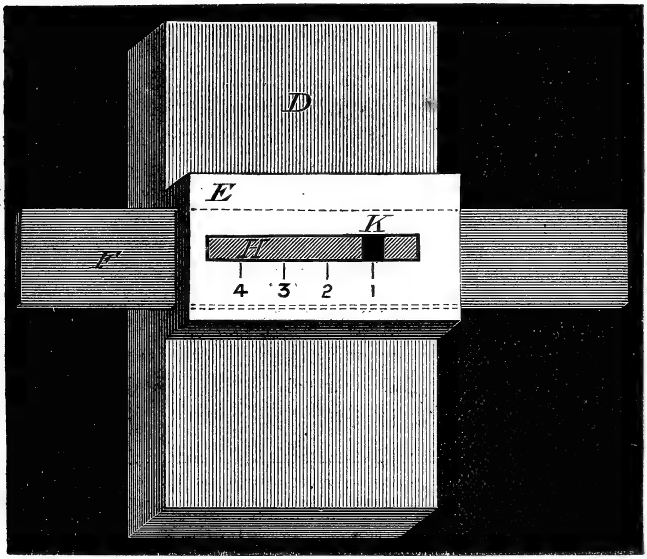
Fig. 4.



violet of the spectrum, and consequently the vertex of the parabola should lie further towards the blue of the spectrum than it does with the visual rays. The method of testing was as follows.

An ordinary dark slide, A, carrying a sensitive plate, was placed in grooves, CC, attached to a board, D, against which the slide (when its front B was drawn out) could be raised or lowered as occasion required by means of a rack and pinion motion working by the handle D.

Fig. 5.



E is a projection in which a slot, H, is cut, and through which a card having a square aperture, K, can slide. K can be covered by means of a cardboard screen. The bottom of the plate in A is first brought opposite the aperture K, which is placed opposite the number marked 1 on E. The lamp is placed 4 feet away, the volts and ampères noted, and exposure is given to the small square of the plate seen through the square in K for any time which may be fixed upon. The slide A is lowered, so that a fresh portion of the plate is brought opposite to K (K being covered up) a different current passed through the lamp, and another exposure given, and so on. When the top of the plate has been arrived at by the motion of A, the card F is moved till K is opposite 2, and the same procedure repeated. Six to ten exposures can be made in the same row.

When the second row is exhausted, K is placed opposite 3, and such a current is passed through the lamp that it emits a medium light. A time scale is then made by giving different lengths of exposure at each movement of the plate in its last half. The plate is then taken out of the slide, and the images developed. By this means both a time scale and a measure of intensity for different temperatures are on the same plate in the shape of squares of different density of deposit. When the negative is dry it is placed in an apparatus which works on the principle of the optical lantern, and is described in the paper written by one of us, "Atmospheric Absorption of Sunlight" ('Phil. Trans.,' 1887), and the density of each square measured. The "intensity" measures are then compared with the time scale, and the value of the intensity calculated. From these values can be determined if the curve of intensity and watts increases parabolically. It might be objected that increase in intensity is not convertible into "time of exposure." Careful experiments have been made as to this, and for the range of time which is comprised in the seconds of exposure given no appreciable error ensues.

The following is an example of an experiment conducted in the above manner:—

Table VI.

Time Scale.

No. of aperture.	Exposure given to portion of plate.	Light transmitted through developed plate.
	secs.	
1	5	55·0
2	10	47·5
3	15	39·5
4	20	33·0
5	25	27·5
6	30	23·2
7	35	20·0
8	40	18·1
9	45	16·0
10	50	14·1
11	55	13·2
12	60	12·4

Bare glass = 56.

Table VII.

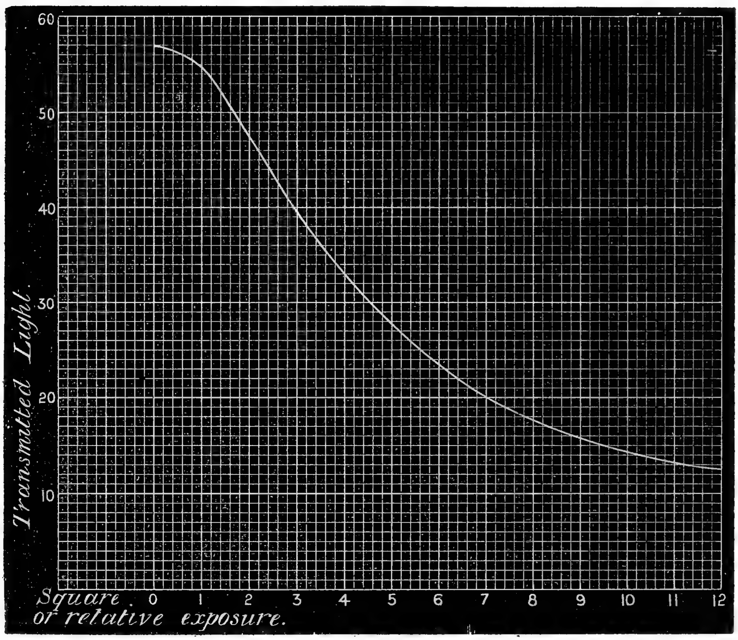
No. of exposure.	Lamp.			Exposure in seconds of small squares.	Light transmitted.	Equivalent time exposure.	Mean.	Reduction to one minute's exposure.	Calculated intensity.	By least squares.
	Current.	Potential.	Watts × constant.							
1......	15·0	37·5	562	15	21·5	66	66	264	264	264·8
2......	,,	,,	,,	,,	21·5	66				
3......	14·1	35·3	498	30	16·25	88	88·5	177	185	176·2
4......	,,	,,	,,	,,	16·00	89				
5......	13·2	33·3	440	30	25	56	56·5	113	114·5	111·1
6......	,,	,,	,,	,,	24·5	57				
7......	12·6	32·1	404	30	32·5	40	41	82	79·2	78·3
8......	,,	,,	,,	,,	31·5	42				
9......	11·6	29·8	346	60	35	36	35	35	36·0	37·4
10......	,,	,,	,,	,,	37	34				
11......	11·1	28·6	317	120	31·5	42	42	21	21	22·1
12......	,,	,,	,,	,,	31·5	42				
13......	10 3	26·9	277	180	33	40	38·5	12·8	6·8	8·4
14......	,,	,,	,,	,,	34·5	37				
15......	9·4	24·8	231	240	42·5	25	25	6·2	0·9	
16......	,,	,,	,,	,,	42·5	25				
17......	9·2	24·4	224	360	39	31	31	5·2	0	
18......	,,	,,	,,	,,	39	31				
19......	8·6	22·9	197	480	48	17	18	2·25	0	
20......	,,	,,	,,	,,	$47\frac{1}{2}$	19				

$m = 225.$ $n = 20{\cdot}08.$

It may be well to point out exactly how the above table was devised. From the time scale (Table VI) the diagram, fig. 6 (on large dimensions) was constructed. Take Nos. 1 and 2, Table VII, the exposures given to each were the same, viz., 15 seconds. The light transmitted through each square was read off, and found to be identical, viz., 21·5. In the scale diagram the abscissa of the curve having the ordinate 21·5 is 66, which is the equivalent time exposure, which is also a measure of the intensity. If the exposure had been prolonged to 1 minute it would be equivalent to an intensity of 264 (since the measured exposure was only 15 seconds). The other intensities were calculated in a similar manner. The intensities 21 and 264 were taken as points on the parabola, and the intermediate intensities calculated using the formula $W - m = n\sqrt{y}$.

Fig. 6.

Time Scale Diagram.



N.B.—It will be noticed that in the above diagram the abscissæ are marked as square or relative exposure. A reference to the time scale table will show that the exposure = number of square × 5. For simplicity's sake the number of the square has been taken as the unit of time.

The following is another example of the photographic measurement.

Table VIII.

Time Scale.

No. of exposure.	Time.	Readings of density.
15	180	7·5
16	160	8·5
17	140	9·5
18	120	12·0
19	100	17·0
20	90	20·7
21	80	25·1
22	70	30·0
23	60	37·5
24	50	46·5

Table IX.

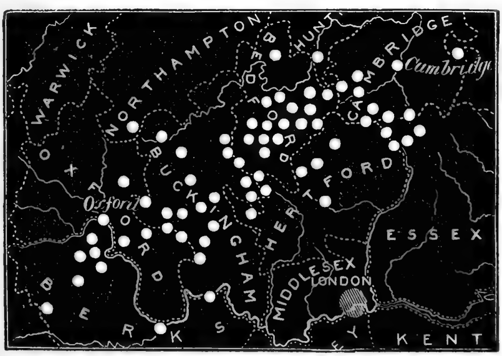
No. of exposure.	Lamp. Current.	Lamp. Potential.	Lamp. Watts × constant.	Exposure in seconds.	Light transmitted through negative.	Equivalent exposure.	Mean.	Reduced to one minute's exposure.	Calculated intensity.
1........	8·6	32·7	281	15	7	184	(for 15 sec.) 186	744	736
2........	,,	,,	,,	7	21·5	88			
3........	8·0	30·2	242	30	not readable		(for 15 sec.) 134	536	529
4........	,,	,,	,,	15	10	134			
5........	7·7	29·0	223	40	not readable		(for 20 sec.) 144	432	432
6........	,,	,,	,,	20	9	144			
7........	6·6	24·9	186	60	8	168	(for 60 sec.) 172	172	176
8........	,,	,,	,,	40	12·5	118			
9........	6·1	22·9	139	60	10·5	130	(for 60 sec.) 130	130	130
10........	,,	,,	,,	30	33·5	65			
11........	5·4	20·0	102	120	9·75	138	(for 120 sec.) 139	69·5	69·2
12........	,,	,,	,,	60	31	70			
13........	4·4	16·2	71	600	9·5	140	(for 600 sec.) 141	14·1	15·2
14........	,,	,,	,,	300	29·5	71			

The above show that the parabolic form seems to be followed, but owing to the want of *absolute* uniformity in all parts of a photographic plate, and that errors may arise from want of exact exposure, and, again, from reading the densities, the values obtained are not so accordant as those taken by the visual method.

V. "On the Detonating Bolide of November 20th, 1887." By G. J. Symons, F.R.S. Received December 8, 1887.

Shortly after November 20th it was generally reported that an earthquake shock had been felt in the South Midland counties of England, and the author began to collect and examine the facts. It appeared that the records from Oxfordshire, and the western stations generally, indicated that much louder sounds were heard there than at the eastern stations, *e.g.*, Essex and Cambridge. The author thought that, although the phenomenon had been almost universally ascribed to an earthquake, it was more probably due to an explosive bolide, and on receiving from one of the local scientific societies a request for assistance in tracing the shock, the author suggested the alternative explanation. Mr. Fordham has subsequently written to say that he has already found one person who saw the meteor from Hertford, which he describes as "a brilliantly luminous body travelling across the sky from N.E. to W." It is further stated that a portion of the meteor was seen to fall from the main body.

Considering that the morning, as shown by the records of the Royal Meteorological Society, was both misty and cloudy, and that at the hour at which it appeared, Sunday morning, 8.20 A.M., there would be broad daylight, it is improbable that many persons saw it. Judging by the descriptions of the noise, as well as by the path roughly indicated by the Hertford observation, it seems likely that it ex-



ploded over the south of Oxfordshire—but further details are much wanted.

The meteor must apparently have been very large, as the explosion was heard or felt over an area of upwards of 2000 square miles—the area being 84 miles in length, from about S.W. to N.E., *i.e.*, from the confines of Wiltshire to Newmarket, Cambridgeshire, and of an average breadth of about 25 miles. The sites whence returns have been received are shown on the annexed map.

Presents, December 8, 1887.

Transactions.

Cambridge, Mass.:—Harvard College. Bulletin of the Museum of Comparative Zoology. Vol. XIII. Nos. 4-5. 8vo. *Cambridge* 1887; Memoirs. Vol. XVI. Nos. 1–2. 4to. *Cambridge* 1887. The Museum.

Harvard University. A Record of the Commemoration on the two hundred and fiftieth Anniversary of the founding of Harvard College. Large 8vo. *Cambridge* 1887. The University.

Essex:—The Essex Naturalist. 1887. Nos. 5–9. 8vo. *Buckhurst Hill.* The Essex Field Club.

Falmouth:—Royal Cornwall Polytechnic Society. 54th Annual Report. 8vo. *Falmouth* 1886. The Society.

Hermannstadt:—Siebenbürgischer Verein für Naturwissenschaften. Verhandlungen und Mittheilungen. Jahrg. XXXV-XXXVII. 8vo. *Hermannstadt* 1885–87. The Verein.

Jena:—Medicinisch-naturwissenschaftliche Gesellschaft. Jenaische Zeitschrift für Naturwissenschaft. Band XX. Heft 4. Band XXI. Hefte 1–2. 8vo. *Jena* 1887. The Society.

Lausanne:—Société Vaudoise des Sciences Naturelles. Sér. 3. Vol. XXII. No. 95. Vol. XXIII. No. 96. 8vo. *Lausanne* 1887. The Society.

Liverpool:—Astronomical Society. Journal. Vols. III–V. Vol. VI. Part 1 (with Supplementary Number). 8vo. *Liverpool* 1884–87; Transactions. Nos. 2–4. 8vo. *Liverpool* 1884; Laws of the Society. 8vo. *Liverpool* 1886; List of Members. 8vo. *Liverpool* 1887. The Society.

London:—Iron and Steel Institute. Journal. 1887. No. 1. 8vo. *London.* The Institute.

Linnean Society. Proceedings. Sessions 1883–84, 1886-87. 8vo. *London.* The Society.

Odontological Society. Transactions. Vol. XIX. No. 8. 8vo. *London* 1887. The Society.

Transactions (*continued*).

Photographic Society of Great Britain. Photographic Journal and Transactions. Vol. XI. No. 9. Vol. XII. No. 1. 8vo. *London* 1887.

Physical Society. Proceedings. Vol. VIII. Part 4. Vol. IX. Part I. 8vo. *London* 1887. The Society.

Royal Asiatic Society. Journal. Vol. XIX. Parts 3–4. 8vo. *London* 1887. The Society.

Royal Horticultural Society. Journal. Vol. IX. 8vo. *London* 1887. The Society.

Royal Institute of British Architects. Journal of Proceedings. Vol. III. Nos. 16–18. Vol. IV. Nos. 1–2. 4to. *London* 1887; Transactions. Vol. III. 4to. *London* 1887; Kalendar, 1887–88. 8vo. *London.* The Institute.

Royal Medical and Chirurgical Society. Medico-Chirurgical Transactions. Vol. LXX. 8vo. *London* 1887. The Society.

Royal Meteorological Society. Quarterly Journal. Vol. XIII. Nos. 62–63. 8vo. *London* 1887; The Meteorological Record. Vol. VI. No. 24. Vol. VII. Nos. 25–26. 8vo. *London* 1886–87; Hints to Meteorological Observers. Second edition. 8vo. *London* 1887. The Society.

Royal United Service Institution. Journal. Vol. XXXI. Nos. 140–141. 8vo. *London* 1887; Index of the Lectures and Papers. Vol. XXI–XXX. 8vo. *London* 1887; A Brief History of the Institution. 8vo. *London* 1887. The Institution.

Moscow:—Société Impériale des Naturalistes. Bulletin. Année 1887. No. 2–3. 8vo. *Moscou* 1887. The Society.

Neuchatel:—Société des Sciences Naturelles. Bulletin. Tome XV. 8vo. *Neuchâtel* 1886. The Society.

Newcastle-upon-Tyne:—North of England Institute of Mining and Mechanical Engineers. Transactions. Vol. XXXVI. Parts 3–4. 8vo. *Newcastle* 1884. The Institute.

New York:—American Geographical Society. Bulletin. Vol. XIX. No. 2–5. 8vo. *New York* 1887. The Society.

American Museum of Natural History. Bulletin. Vol. II. No. 1. 8vo. *New York* 1887. The Museum.

Nottingham:—University College. Calendar. 1887–88. 12mo. *Nottingham.* The College.

Paris:—Société Géologique de France. Bulletin. Sér. 3. Tome XIV. No. 8. Tome XVI. Nos. 1–26. 8vo. *Paris* 1887. The Society.

Société Mathématique de France. Bulletin. Tome XV. No. 4. 8vo. *Paris* 1887. The Society.

Transactions (*continued*).

Société Philomathique. Bulletin. Sér. 7. Tome XI. Nos. 2–3. 8vo. *Paris* 1887. The Society.

Philadelphia:—Academy of Natural Sciences. Proceedings. 1887. Part 1. 8vo. *Philadelphia.* The Academy.

American Philosophical Society. Proceedings. Vol. XXIV. No. 125. 8vo. *Philadelphia* 1887. The Society.

Pisa:—Società Toscana di Scienze Naturali. Atti. Vol. VIII. Fasc. 2. 8vo. *Pisa* 1887; Processi Verbali. Vol. V. 8vo. [*Pisa*] 1887. The Society.

Plymouth:—Plymouth Institution. Annual Report. Vol. IX. Part 3. 8vo. *Plymouth* 1887. The Institution.

Stockholm:—K. Vetenskaps-Akademie. Bihang till Handlingar. Band XI. Häfte 2. 8vo. *Stockholm* 1887; Öfversigt. Årg. XLIV. Nos. 5–6. 8vo. *Stockholm* 1887. The Academy.

Vienna:—K. K. Naturhistorisches Hofmuseum. Annalen. Band I. Nr. 3–4. Band II. Nr. 1–3. Large 8vo. *Wien* 1886–87. The Museum.

K. K. Zoologisch-Botanische Gesellschaft. Verhandlungen. Band XXXVII. Hefte 1–2. 8vo. *Wien* 1887. The Society.

Washington:—National Academy of Sciences. Memoirs. Vol. III. Part 2. 4to. *Washington* 1886. The Academy.

Smithsonian Institution. Fourth Annual Report of the Bureau of Ethnology. 4to. *Washington* 1882–3; Smithsonian Miscellaneous Collections. Vols. XXVIII—XXX. 8vo. *Washington* 1887; Report. 1885. Part 1. 8vo. *Washington* 1886. The Institution.

Observations and Reports.

Christiania:—Norwegian North Atlantic Expedition. 1876–78. Report. Parts 17–18. 4to. *Christiania* 1887. The Editorial Committee.

India:—Great Trigonometrical Survey of India. Account of Operations. Vol. IVa. 4to. *Dehra Dun* 1886; Synopsis of Results. Vol. VIIa. 4to. *Dehra Dun* 1887; General Report of the Operations of the Survey of India Department, 1885–86. Folio. *Calcutta* 1886. Survey of India Department.

London:—Local Government Board. Report of a Committee to Inquire into M. Pasteur's Treatment of Hydrophobia. Folio. *London* 1887. Sir J. Paget, Bart., F.R.S.

Stationery Office. "Challenger" Expedition. Report. Zoology. Vols. XX–XXII. 4to. *London* 1887. The Stationery Office.

Observations, &c. (*continued*).

Melbourne:—Observatory. Monthly Record. January to June, 1887. 8vo. *Melbourne.* The Observatory.

Milan:—Reale Osservatorio di Brera. Pubblicazioni. No. VI. No. XXXII. 4to. *Milano* 1875, 1887. The Observatory.

New Haven:—Yale University. Astronomical Observatory. Transactions. Vol. I. Part 1. 4to. *New Haven* 1887. The Observatory.

New York:—Columbia College. Library. Fourth Annual Report. 8vo. *New York* 1886. The College.

Cooper Union for the Advancement of Science and Art. Annual Reports. 1884–86. 8vo. *New York* 1887. The Union.

Paris:—Service Hydrographique de la Marine. Annales Hydrographiques. No. 696. 8vo. *Paris* 1887. The Service.

Washington:—Signal Office. Reports. 1885. Parts 1–2. 8vo. *Washington* 1885. The Office.

U. S. Fish Commission. Bulletin. Vol. VI. 8vo. *Washington* 1887. The Commission.

U. S. Geological Survey. Bulletin. Nos. 34–39. 8vo. *Washington* 1886–87; Monographs. Vol. X. 4to. *Washington* 1886; Sixth Annual Report. 1884-85. 4to. *Washington* 1885. The Survey.

Journals.

Annals of Mathematics. Vol. III. No. 4. 4to. *Charlottesville* 1887. The University of Virginia.

Bullettino di Bibliografia e di Storia delle Scienze Matematiche e Fisiche. Tomo I–III. Tomo XIX. Luglio-Dicembre. 4to. *Roma* 1868–70, 1886. The Prince Boncompagni.

Fortschritte der Physik. 1881. 8vo. *Berlin* 1887. Physikalische Gesellschaft zu Berlin.

Horological Journal (The) Vol. XXIX. Nos. 347–348. Vol. XXX. Nos. 349–351. 8vo. *London* 1887. The Horological Institute.

New York Medical Journal (The) Vol. XLV. Vol. XLVI. Nos. 1–18. Small folio. 1887. The Editor.

Revista do Observatorio. Anno II. Num. 3–9. Large 8vo. *Rio de Janeiro* 1887. The Observatory.

Timehri. Journal of the Royal Agricultural and Commercial Society of British Guiana. Vol. V. Part 2. Vol. I (New Series). Part 2. 8vo. *Demerara* 1886–87. The Society.

December 15, 1887.

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

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

The President read a letter from H.M. Secretary of State for Home Affairs, announcing that Her Majesty had "been graciously pleased to command that the Royal Society be allowed to enjoy the privilege, on all fit and proper occasions, of presenting their addresses to the Sovereign on the Throne."

The President was requested to convey to the Home Secretary the thanks of the Society for his communication, and to express their satisfaction that Her Majesty had been graciously pleased to honour the Society with this mark of the Royal recognition.

The following Papers were read:—

I. "Note on the Development of Feeble Currents by purely Physical Action, and on the Oxidation under Voltaic Influences of Metals not ordinarily regarded as spontaneously oxidisable." By C. R. ALDER WRIGHT, D.Sc., F.R.S., Lecturer on Chemistry and Physics, and C. THOMPSON, F.C.S., F.I.C., Demonstrator of Chemistry, in St. Mary's Hospital Medical School. Received November 24, 1887.

In the course of a series of further experiments on cells set up with "aeration plates" (plates simultaneously in contact with the air and the electrolytic fluid used in the cells—'Roy. Soc. Proc.,' vol. 42, p. 212), we have made a large number of determinations of the E.M.Fs. developed with incorrodible aeration plates of various kinds (*e.g.*, platinum foil, spongy platinum, thin sheet gold, &c.) when opposed to the same oxidisable metal, such as copper or zinc, in contact with the same electrolytic fluid, *e.g.*, dilute sulphuric acid or caustic soda solution. The details of these observations, when completed, will form the subject of a future paper; whilst making them we have noticed that if two or more different kinds of aeration plates be set up on the surface of the fluid contained in a shallow basin in which the oxidisable metal is immersed, and sufficient time be allowed to elapse to enable the films of air attracted to the aeration plates to attain a condition of equilibrium, different constant values are usually obtained for the E.M.Fs. generated by opposing to the oxidisable metal first one and then the other of any given pair of aeration plates, the currents generated being rendered throughout of too small density for "running down" to take place during the observations by interposing a large resistance in the circuit. If when this state of constancy has been attained the two aeration plates be opposed to each other with a considerable resistance in circuit, a current passes from the one giving the higher value when opposed to the oxidisable plate through the external circuit to the other. This current at first is of such magnitude as to correspond exactly with the E.M.F. due to the difference between the E.M.Fs. exhibited when the two plates respectively are opposed to the oxidisable metal, but after some time it gradually diminishes; even after several days, or even weeks, however, it is usually still measurable. If a miniature silver voltameter be included in the circuit, in many cases an appreciable amount of crystalline silver is found to be slowly deposited on the negative electrode of the voltameter, which may conveniently be a thin gold wire immersed to a depth of a few millimetres in silver nitrate solution, a silver plate or wire forming the positive electrode. Thus, for example, in various experiments the following figures were obtained, the aeration plates being arranged on the surface of dilute sulphuric acid:—

Nature of aeration plates.	Time in days.	Silver deposited in voltameter in milligrams.	Average current in microampères during the period.
1. Spongy platinum and smooth platinum foil	14	10·5	7·7
2. Ditto ditto	18	1·5	0·8
3. Spongy platinum and smooth gold plate	24	6·5	2·8
4. Spongy gold and smooth gold gold plate	14	1·0	0·7
5. Spongy gold and platinum foil	24	1·25	0·5

Analogous results were obtained in various other cases with different electrolytic fluids, *e.g.*, spongy silver and smooth silver sheet with caustic soda solution. In every case the action was greatest at first and gradually diminished, but never became absolutely *nil*. The larger the surface of the aeration plates, as might be expected, the greater was the average current; thus in experiment No. 1 above, plates exposing about 20 sq.c. surface (one side, superficial measurement not reckoning inequalities of sponge) were used, and in No. 2 plates only about one-fifth that size.

It is obvious that during the passage of a current the dilute sulphuric acid between the two plates must be electrolysed, so that hydrogen would tend to be liberated on the surface of the plate acquiring the higher potential, and oxygen on that of the other; the hydrogen whilst nascent would necessarily be more or less completely oxidised to water by the oxygen of the film of condensed air, so that on the whole the net chemical action in the cell itself would be either *nil* (if all hydrogen were so re-oxidised) or one *absorbing* heat (if some of the hydrogen escaped oxidation). The oxygen slowly evolved would escape as such, being dissolved by the surrounding fluid. The effect of this should accordingly be that the efficiency of the air film on the first plate would be more or less depreciated, and that on the second exalted; in point of fact, if the two aeration plates in such an arrangement, which has been generating a current for some time, be (by means of an appropriate switch) disconnected from one another and successively opposed to a given oxidisable plate, the one does give a considerably lower and the other usually an appreciably higher value than the constant ones previously obtained (before the two aeration plates were directly opposed to one another) on opposing each severally to the oxidisable metal; whilst on allowing the cell to stand for some time generating no current, the lower value gradually rises and the raised one falls until sensibly the old constant values are again obtained.

We noticed, moreover, that when aeration plates of platinum-foil or sponge are used opposed to *silver* plates in conjunction with a fluid capable of dissolving silver oxide (such as dilute sulphuric or acetic acid or ammonia solution) distinctly larger amounts of current are usually developed than when opposed to carbon or gold plates, and that simultaneously silver passes into solution, the silver plate acquiring the lower potential, diminishing in weight, and, in short, behaving precisely as though it were an oxidisable metal, such as zinc or copper. Obviously this is due to the circumstance that with silver the oxygen liberated attacks the metal of the plate acquiring the lower potential; but the remarkable part of the action is that this attack is only partial, so that the amount of silver dissolved is invariably *less than that equivalent to the current passing*, *i.e.*, less than that deposited in a silver voltameter included in the circuit. Thus the following numbers were obtained in a series of experiments, in each of which four similar cells containing platinum sponge aeration plates arranged in series were used in order to shorten the time of observation. The electrolytic fluids used in the various cases were respectively:—

A. Acetic acid solution, approximately of strength

$$14C_2H_4O_2,100H_2O.$$

B. Ditto also containing sodium acetate, approximately of strength $10C_2H_4O_2,10NaC_2H_3O_2,100H_2O$.

C. Ammonia solution originally of strength $10NH_3,100H_2O$, but considerably weakened during the experiment by evaporation.

D. Ammonia solution also containing ammonium sulphate; originally $10NH_3,2\cdot5(NH_4)_2SO_4,100H_2O$.

E. Ammonia solution also containing sal-ammoniac; originally of strength $10NH_3,5NH_4Cl,100H_2O$.

F. Dilute sulphuric acid, $4H_2SO_4,100H_2O$.

Time in hours.	Silver dissolved		Silver deposited in volta-meter.	Per cell per 24 hours.		Difference.
	In 4 cells jointly.	Per cell.		Silver deposited.	Silver dissolved.	
A. 180	0·003	0·00075	0·0025	0·00033	0·00010	0·00023
B. 130	0·0045	0·001125	0·0020	0·00037	0·00021	0·00016
C. 68	0·015	0·00375	0·0060	0·0021	0·0014	0·0007
D. 136	0·1095	0·027375	0·037	0·0065	0·0048	0·0017
E. 44	0·086	0·0215	0·027	0·0147	0·0117	0·0030
F. 96	0·348	0·087	0·097	0·0242	0·0217	0·0025

The difference between the silver dissolved and that deposited by the current is thus relatively much larger with the weakest currents, representing 43—69 per cent. of the latter in cases A and B; 26—33 per cent. in cases C and D; and 10—20 per cent. in cases E and F.

It is obvious that if silver will dissolve in acids, &c., under the comparatively feeble oxidising influence of an aeration plate, much more rapid solution might be anticipated by substituting for such a plate platinum immersed in a powerfully oxidising fluid such as strong nitric acid, or sulphuric acid solution of chromic anhydride. In point of fact, we have found that on setting up such cells where the silver was immersed in dilute sulphuric acid (*i.e.*, Grove's cell with silver instead of zinc, and so on), electromotors of notable power are produced, at any rate until the silver plate becomes coated with sparingly soluble sulphate. Even in these cases, however, perfect correspondence between the amount of silver dissolved and that deposited in a voltameter included in the circuit does not subsist, the latter being always measurably the greater. Thus in several experiments with such cells, when the current was so regulated by interposing suitable resistances that the silver deposition in the voltameter was brought down to 0·1 to 0·2 gram of silver per 24 hours, the silver deposited always exceeded that dissolved by 0·001 to 0·003 gram. Similarly two duplicate cells set up with silver plates immersed in ammonia solution containing sal-ammoniac of strength about

$$5NH_3,5NH_4Cl,100H_2O,$$

and opposed to platinum immersed in sulphuric acid solution containing chromic anhydride, gave the following figures, much more resistance being in circuit in the second experiment than in the first.

Time.	Silver deposited in voltameter.	Silver dissolved from plate.	Difference.
18 hours	0·514 gram	0·510	0·004
46 ,,	0·107 ,,	0·106	0·001

A similar cell containing ammonia solution without sal-ammoniac, and consequently having a very large internal resistance, caused only 0·013 gram of silver to be dissolved in eighteen hours, whilst 0·015 gram was deposited; in this case a visible film of silver peroxide was formed on the silver plate (a wire of pure metal).

Just as silver is capable of being dissolved in an appropriate fluid when opposed to an aeration plate, so may several other metals not ordinarily prone to atmospheric oxidation; thus mercury with dilute sulphuric acid as fluid, and an aeration plate of platinum sponge, generates a measurable continuous current, forming *mercurous sulphate* in so doing, so that after some time the liquid becomes turbid through separation of that sparingly soluble salt, and the filtered fluid precipitates calomel on addition of dilute hydrochloric acid. Acetic acid acts similarly, but far less energetically. Potassium cyanide solution, on the other hand, causes a much more rapid solution of mercury, forming *mercuric potassiocyanide;* it is noticeable that in this case only 100 parts of mercury go into solution for 108 of silver deposited in the voltameter, whereas when sulphuric acid is used 200 parts of mercury become sulphate per 108 of silver deposited.

If gold be substituted for mercury in this latter arrangement, rapid solution takes place with formation of *aurocyanide of potassium*, 196 parts of gold being dissolved per 108 of silver thrown down in the voltameter; the rate of action here, as in other analogous cases, can be notably increased by placing the gold plate and potassium cyanide solution in one basin and the aeration plate (platinum sponge) in another with sulphuric acid, uniting the two fluids by a wide siphon, so as to superadd to the other E.M.Fs. in operation that due to the mutual neutralisation of the acid and alkali.

Palladium behaves precisely as gold, 52 parts of metal being dissolved per 108 of silver deposited; local action sometimes causes in each case *excess* of amount dissolved relatively to the current passing, the opposite result to that observed with the silver cells above described.

Of course, if more powerful oxidising agents are used than simple aeration plates (such as platinum in sulphuric-chromic solution), the action goes on in all such cases still more rapidly; thus, for example, we did not succeed in dissolving gold in dilute hydrochloric alone by the use of an aeration plate simply; but on replacing this by a platinum plate immersed in sulphuric-chromic liquor connected by a siphon with the dilute hydrochloric acid in which the gold was immersed, chlorination of the gold was readily effected with the formation in the first instance of *aurous chloride*, which rapidly broke up into particles of spongy gold and auric chloride in solution.

II. "The Early Development of the Pericardium, Diaphragm, and Great Veins." By C. B. LOCKWOOD, F.R.C.S., Hunterian Professor of Anatomy in the Royal College of Surgeons of England. Communicated by G. M. HUMPHRY, F.R.S. Received November 26, 1887.

(Abstract.)

The history of the development of the pericardium, diaphragm, and great veins is traced by means of rabbit's embryos ranging from the eighth to the seventeenth day of intrauterine life.

The splanchnic origin of the two halves of the heart is briefly illustrated, and each separate half is shown to project into the foremost end of the cœlom. The approximation of the halves of the heart, and of the cœlom in which they are contained, and the formation of the mesocardium posterius and anterius, is next narrated. The course of the omphalomesenteric veins to the heart along the splanchnic wall of the cœlom is then traced, and those vessels are shown to divide the cœlom into two parts, a "cardiac" and a "pleuro-peritoneal." At the beginning of the ninth day the cœlom consists of two halves which are some distance apart towards the tail end, but converge towards the head to open behind the omphalomesenteric veins, into the cardiac portions of the cœlom. To adopt a rough comparison, the cœlom is, at the beginning of the ninth day, not unlike a pair of trousers; the cardiac portion would correspond to that part of the trousers which receives the pelvis, whilst the hinder parts of the cœlom would correspond to the places for the legs. To

carry the simile a step further, it might be said that the omphalomesenteric veins would run round the front of the trousers opposite the bend of the groins.

An adhesion of the omphalomesenteric veins to the somatopleure, at the level of the hinder end of the heart, is next described, and identified with the mesocardium laterale, and is shown to be the way by which the umbilical veins find a passage to the heart. Those vessels develop in the somatopleure, and are by means of it brought in relation with the endometrium in a manner which is described. The portions of the omphalomesenteric veins which cross the ventral splanchnic boundary of the cœlom are held by the mesocardium posterius, and by the mesocardium laterale, close to the dorsal wall of the cœlom, and, in consequence, as the cardiac and pleuro-peritoneal portions of the cœlom expand, the part bounded by the omphalomesenteric veins remains stationary and narrow. This narrow part of the cœlom is named the "iter venosum," because the great veins have so much to do with its formation, and, subsequently, with its closure.

The development of a septum, the septum transversum, between the cardiac and pleuro-peritoneal portions of the cœlom, is attributed to the fixation of the omphalomesenteric veins. When, in due course, the heart expands and is carried tailwards by the cranial flexure and its own growth, those vessels continue to hold the ventral splanchnic wall of the cœlom close against the dorsal wall, and in consequence it becomes retroflected behind the heart. This retroflected portion stretches from one mesocardium laterale to the other, across the axis of the embryo; its front surface is in contact with the heart, and its hinder surface is covered with hypoblast in which the liver originates; thus a ventral diaphragm is formed between the liver and the heart.

The appearance of other somatic veins, namely, of the anterior cardinals and afterwards of the posterior cardinals, is noted. The former develop first and empty into the umbilical veins just as they (the umbilical veins) open into the omphalomesenteric; when the posterior cardinal veins appear they join the anterior cardinals, so that a portion of each of the latter nearest the heart becomes the Cuvierian duct.

Until the middle of the ninth day the embryo lies with its back to the uterus. The way in which it turns its right side and afterwards its venter towards the uterus is described, and also the infolding of the somatopleure and splanchnopleure, and its effect upon the relations of the great veins and septum transversum.

The commencement of the umbilical veins and early formation of the placenta are next illustrated. The allantois of the rabbit is shown to be exceedingly rudimentary, and to take no part in the formation of the placenta, which is developed in connexion with somatic structures.

The further development of the ducts of Cuvier is then explained, and those vessels are shown to end, as did the jugulars from which they are formed, by opening into the mouths of the umbilical veins quite close to the heart. In the next stage of development, owing to the expansion of the heart, the omphalomesenteric veins, umbilical veins, and Cuvierian ducts, acquire separate openings into the heart, and at the same time the right and left umbilical veins, just before entering the heart, communicate with the venous spaces of the liver, and have through them an alternative route to the heart. Whilst these changes are in progress, the left omphalomesenteric vein, where it is related to the liver, becomes occluded with liver substance. The gradual conversion of the mesocardium laterale and septum traversum into a dorsal pericardium and ventral diaphragm is then described, and afterwards the closure of the iter venosum by the apposition of the Cuvierian ducts and the sides of the trachea and œsophagus; whilst this is in progress the subclavian veins appear and empty themselves into the Cuvierian ducts, which in this way become the right and left superior venæ cavæ.

During the twelfth day the umbilical veins lose their direct opening into the heart, and the left vein, taking advantage of the alternative route through the liver, passes through the substance of that organ to end in the right omphalomesenteric vein close to the heart. The channel which unites the left umbilical vein to the right omphalomesenteric vein is the ductus venosus Arantii. When the permanent kidneys and hind limbs develop, a vein passes from them into the cardiac end of the right omphalomesenteric vein, so that it becomes the terminal end of the inferior vena cava.

Whilst these changes are in progress numerous mesenteric veins develop, and open into the hinder portion of the right omphalomesenteric, which then becomes the portal vein, and at first empties into the sinus venosus Arantii. The hepatic portion of the left omphalomesenteric vein is quite obliterated, and that vessel ceases to enter the heart; however, its hinder part may persist and carry blood from the mesentery into the portal vein, with which it acquires communications.

About the middle of the twelfth day, and when the iter venosum is upon the point of closure, the dorsal diaphragm develops as a crescentic fold projecting from the side body-wall close to the superior venæ cavæ, and uniting the dorsal pericardium to the dorsal body-wall. As the thorax develops this dorsal diaphragm travels further tailwards, its hindermost dorsal attachments being united to the fore-end of the urogenital ridge, and its ventral attachments with the dorsal part of the liver and the mesoblast which covers it.

The growth and development of the dorsal diaphragm is traced until, upon the thirteenth day, it unites with the dorsal mesentery, and forms a complete partition between the thorax and abdomen. Finally, the development of the crura and other muscular portions of the diaphragm is mentioned.

III. "An Investigation into the Function of the Occipital and Temporal Lobes of the Monkey's Brain." By SANGER BROWN, M.D., and E. A. SCHÄFER, F.R.S., Jodrell Professor of Physiology in University College, London. Received November 24, 1887.

(Abstract.)

This paper contains a record of a series of experiments on the brain of monkeys, which consisted in the establishment of definite lesions of the occipital and temporal lobes, and the observation of the results of such lesions. Drawings showing exactly the extent of the lesion in each case accompany the paper.

Presents, December 15, 1887.

Transactions.

Calcutta:—Asiatic Society of Bengal. Journal. Vol. LIV. Part 2. No. 4. Vol. LV. Part 2. No. 5. Vol. LVI. Part 2. No. 1. 8vo. *Calcutta* 1887; Proceedings. 1887. Nos. 6–8. 8vo. *Calcutta.* The Society.

Cambridge:—Philosophical Society. Proceedings. Vol. VI. Part 2. 8vo. *Cambridge* 1887. The Society.

Devonshire:—Devonshire Association. Report and Transactions. Vol. XIX. 8vo. *Plymouth* 1887; The Devonshire Domesday. Part 4. 8vo. *Plymouth* 1887. The Association.

Glasgow:—Philosophical Society. Proceedings. Vol. XVIII. 8vo. *Glasgow* 1887. The Society.

Heidelberg:—Naturhistorisch-Medicinischer Verein. Verhandlungen. Band IV. Heft 1. 8vo. *Heidelberg* 1887. The Verein.

Leipzig:—Astronomische Gesellschaft. Vierteljahrsschrift. Jahrg. XXII. Hefte 2–3. 8vo. *Leipzig* 1887. The Society.

Fürstlich-Jablonowski'sche Gesellschaft. Jahresbericht. 1887. 8vo. *Leipzig.* The Society.

Königl. Sächs. Gesellschaft der Wissenschaften. Abhandlungen (Math.-Phys. Classe). Band XIII. Nos. 8–9. Band XIV.

Transactions (*continued*).

Nos. 1–4. 8vo. *Leipzig* 1887; Abhandlungen (Philol.-Histor. Classe). Band X. Nos. 4-7. 8vo. *Leipzig* 1887; Bericht über die Verhandlungen (Philol.-Histor. Classe). 1887. Nos. 1–3. 8vo. *Leipzig*. The Society.

London:—Royal Agricultural Society of England. Journal. Vol. XXIII. Part 2. No. 46. 8vo. *London* 1887. The Society.

Society of Antiquaries. Archæologia. Vol. L. Part 2. 4to. *London* 1887. The Society.

Melbourne:—Royal Society of Victoria. Transactions and Proceedings. Vol. XIX. 8vo. *Melbourne* 1883. The Society.

New Orleans:—Academy of Sciences. Papers read 1886-87. Vol. I. 8vo. *New Orleans* 1887. The Academy.

Oxford:—Radcliffe Library. Catalogue of Transactions of Societies, Periodicals, and Memoirs, for use in the reading room. Fourth edit. 8vo. *Oxford* 1887. The Library.

Paris:—École Normale Supérieure. Annales. Sér. 3. Tome IV. Nos. 6–11. 4to. *Paris* 1887. The School.

École Polytechnique. Journal. Cahier 56. 4to. *Paris* 1886. The School.

Société Académique Indo-Chinoise de France. Bulletin. Sér. 2. Tome II. 8vo. *Paris* 1883–85. The Academy.

Société de Géographie. Bulletin. 1887. Trim. 3. 8vo. *Paris*. The Society.

Société Entomologique de France. Annales. Sér. 6. Tome VI. Trim. 1–4. 8vo. *Paris* 1886–87. The Society.

Rome:—R. Comitato Geologico d'Italia. Bullettino. 1887. Nos. 3–8. 8vo. *Roma* 1887. The Comitato.

Turin:—R. Accademia delle Scienze. Atti. Vol. XXII. Disp. 12–13. 8vo. *Torino* 1887. The Academy.

Watford:—Hertfordshire Natural History Society and Field Club. Transactions. Vol. IV. Part 6. 8vo. *London* 1887; and Vol. I. Part 6–8. Vol. II. Part 3. 1875–83. The Society.

Wellington:—New Zealand Institute. Transactions and Proceedings. Vol. XIX. 8vo. *Wellington* 1887. The Institute.

Philosophical Society. Anniversary Address. Session 1887–88. 8vo. *Wellington*. The Society.

Observations and Reports.

Berlin:—Commission für die Beobachtung des Venus-Durchgangs. Die Venus-Durchgänge 1874 und 1882. Bericht über die Deutschen Beobachtungen. Band IV. 4to. *Berlin* 1887.
The Commission.

Brisbane:—Colony of Queensland. Census, 1886. Report, with maps. Folio. *Brisbane* 1887; Statistics of the Colony. 1886. Folio. *Brisbane* 1887. The Registrar-General.

Cambridge, Mass.:—Harvard College Observatory. Annals. Vol. XVII. Vol. XVIII. Nos. 1–2. 4to. *Cambridge* 1887; Boyden Fund. Circular, No. 2. 4to. [*Cambridge*] 1887.
The Observatory.

India:—Tide Tables for the Indian Ports for 1888, and January, 1889. 12mo. *London* [1887]. The India Office.

Montreal:—McGill College and University. Calendar. 1887–88. 8vo. *Montreal* 1887. The College.

Venezuela:—La Exposicion Nacional de Venezuela en 1883. Tomo I. Texto. Folio. *Caracas* 1886. Dr. A. Ernst.

Correspondence between the Venezuelan Government and H.M. Government on the question of the Frontier, &c. Folio. *Caracas* 1887. The Venezuelan Consul.

Wellington:—Colony of New Zealand. Results of a Census. 1886. Folio. *Wellington* 1887. The Registrar-General.

Ball (Sir R. S.), F.R.S. Dynamics and Modern Geometry: a new chapter in the history of Screws. 4to. *Dublin* 1887.
The Author.

Bell (Dr. J.), F.R.S. The Chemistry of Tobacco. (*Two copies.*) 8vo. 1887. The Author.

Braithwaite (R.) The British Moss-Flora. Part 10. Large 8vo. *London* 1887. The Author.

Burggraeve (A.) La Médecine Dosimétrique contemporaine. Médecine Humaine. Sér 1. 1871–1886. Large 8vo. *Bruxelles* 1886; Livre d'Or de la Médecine Dosimétrique. Large 8vo. *Paris* 1886; Miscellanées de Médecine Dosimétrique. Sér. 1. Large 8vo. *Bruxelles* 1887. The Author.

Cauchy (A.) Œuvres complètes. Sér. 2. Tome VI. 4to. *Paris* 1887. Académie des Sciences, Paris.

Colenso (W.), F.R.S. Miscellaneous Papers, from the 'Transactions of the N.Z. Institute,' Vol. XIX, 1886. 8vo. [1887].
The Author.

Cunningham (D. O.) On the Effects sometimes following the injection of Cholera Comma-bacilli into the subcutaneous tissues of Guinea-Pigs. 4to. *Calcutta* 1887; On the phenomenon of Gaseous Evolution from the flowers of *Ottelia alismoides.* 4to. *Calcutta* 1887. The Author.

Harley (Dr. G.), F.R.S. The Recuperative Bodily Power of Man. 8vo. *London* 1887. The Author.

Helmholtz (H. von), For. Mem. R.S. Handbuch der Physiologischen Optik. Lief. 4. 8vo. *Hamburg* 1887. The Author.

Hinde (G. J.) The Organic Origin of Chert. 8vo. [*London*] 1887. The Author.

Jones (T. R.), F.R.S., and C. D. Sherborn. Further notes on the Tertiary Entomostraca of England, with special reference to those from the London Clay. 8vo. [*London*] 1887. Prof. T. R. Jones, F.R.S.

Laplace (Marquis de) Œuvres complètes. Tome VII. 4to. *Paris* 1886. Académie des Sciences, Paris.

McConnell (P.) The Agricultural Depression. 8vo. *London* 1887. The Author.

Moore (F.) The Lepidoptera of Ceylon. Part 13 (Supplementary). 4to. *London* 1887. The Government of Ceylon.

Neild (J. E.) The Medical School of the Melbourne University. An Address. 8vo. *Melbourne* 1887. The Author.

Newton (E. T.) On the remains of Fishes from the Keuper of Warwick and Nottingham, with Notes by the Rev. P. B. Brodie and E. Wilson. 8vo. [*London*] 1887. The Rev. P. B. Brodie.

Peters (C. H. F.) Flamsteed's Stars "observed, but not existing." 4to. [*Washington*] 1885; Corrigenda in various Star Catalogues. 4to. [*Washington*] 1885. The Author.

Reade (T. M.) Secular Cooling of the Earth in relation to Mountain-Building. 8vo. *London* 1887. The Author.

Sanderson (J. S. B.), F.R.S. Translations of Foreign Biological Memoirs. Edited by J. S. B. Sanderson. 8vo. *Oxford* 1887. The Delegates, Clarendon Press.

Scharff (R.) On the Intra-ovarian Egg of some Osseous Fishes. (*Two copies.*) 8vo. *London* 1887. The Author.

Thore (J.) Communications sur Une Nouvelle Force? 8vo. *Dax* 1887. The Author.

December 22, 1887.

Admiral Sir GEORGE HENRY RICHARDS, K.C.B., Vice-President, in the Chair.

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

The following Papers were read:—

I. "On the Heating Effects of Electric Currents. No. II." By WILLIAM HENRY PREECE, F.R.S. Received November 24, 1887.

On March 19th, 1884, I submitted to the Royal Society a paper on the heating effects of electric currents,* showing the strength of current necessary to fuse the fine platinum wire employed for protecting submarine cables from the ill effects of atmospheric electricity. The paper proved that the law that regulates the production of heat is one which can be expressed by the formula $C = ad^{3/2}$, "a" being a constant dependent on the metal used, and "d" the diameter of the wire. The current observed was that which heated the wire up to the point of self-luminosity (525° C.).

Since "cut-outs" of the same character as the cable lightning protector have become an essential feature of all electric lighting installations, to act as safety fuses when from accident or design an excess of current is allowed to pass through the conductor, it became most desirable to determine the current that would fuse wires of different diameters, and of different materials, so as to determine the coefficient a for all metals. The best material to use and the proper dimensions of the fusible wire to be employed for the protection of the electric light conductors would thus be easily deduced.

My source of electricity was a large secondary battery of 52 cells. I could regulate the current flowing at will by a rheostat of thick iron wire, and by varying the number of cells. The current strength was calculated by measuring the potential difference at the ends of a thick flat platinoid bar, whose resistance was $0{\cdot}1822^{\omega}$, inserted in the circuit, and so large that it would not perceptibly warm up nor have its resistance appreciably increased with any current used. The sizes of wire experimented upon were limited by the current. It is not safe

* 'Roy. Soc. Proc.,' 1884, No. 231.

to draw upon secondary cells for more than 10 ampères per negative plate of the dimensions at present followed. The packed plates disintegrate and become damaged with too great an output of current. Hence all my experiments were made with currents well within the range of the battery. I obtained samples of wire of various metals and of various diameters from 0·004 inch up to 0·040 inch. It is convenient to take these measurements in thousandths of an inch (*mils*), for all our manufacturers and electric light engineers in the United States and the United Kingdom work to this gauge. The conversion of the values thus obtained into the metrical and more scientific system is very simple. The wire to be experimented upon was clamped between two small brass binding screws fixed upon a dry wooden stand.

I pointed out in my previous paper how the cooling effects of the terminals or binding screws might vitiate the results, and how necessary it was to experiment on wires of sufficient length to prevent any error occurring from this cause. I used lengths of 6 inches to determine the constants for wires free from the cooling effect, but lengths of $1\frac{1}{4}$ inch with massive terminals to determine the constants for wires used in practice as "cut-outs."

The cooling effect of the terminals very seriously affects the efficiency of the cut-outs used in actual practice, and the larger the fusing wire and the terminals the more serious is the error introduced. On the other hand, the greater the lengths of wire used as a fuse the greater the resistance inserted, and the efficiency of the system itself may be reduced. Cut-outs, therefore, should be employed sparingly and with judgment, and the fusing wire should not be so short as to impair the fusing point.

In the following tables I have tabulated the results of the numerous experiments made.

When we consider the irregularities in drawing these fine wires to true cylinders, the difficulty in determining the current at the exact moment of fusion, and the variation in the specific resistance of the metals, I think the results must be considered very satisfactory in support of the law.

Three points of observation were taken :—

1. The melting point of a small flake of shellac placed on the wire, which may be taken at 77° C.

2. The point of self-luminosity, 525° C. This was only determined roughly in air without the dark chamber I employed previously.

3. The fusing current.

Series I. "Cut-outs."

Copper.

Diameter of wire.			Current in ampères.			Fusing current calculated from the formula $ad^{3/2}$. Ampères.	Constant "a" when d expressed in inches.
In inches.	In centi-metres.	Standard wire gauge No.	Flake of shellac melted.	Wire red hot. Visible in air.	Wire fused.		
0·004	0·010	42	2·41	3·057	3·299	3·48	13033
0·005	0·013	39	2·896	4·184	4·586	4·87	12964
0·007	0·018	37	5·631	7·000	7·724	8·07	13183
0·010	0·025	33	9·976	14·160	15·44	14·23	15433
0·012	0·030	30	11·22	16·29	19·35	18·71	14748
0·014	0·036	28	13·68	19·30	21·84	22·86	13162
0·018	0·046	26	21·71	31·43	36·40	34·37	15066
0·020	0·051	25	24·03	35·61	39·37	39·05	13699
0·026	0·066	22	30·69	44·59	54·43	57·88	12978
						Mean =	13807

Aluminium.

Diameter of wire.			Current in ampères.			Fusing current calculated from the formula $ad^{3/2}$.	Constant "*a*" when *d* expressed in inches.
In inches.	In centimetres.	Standard wire gauge No.	Flake of shellac melted.	Wire red hot. Visible in air.	Wire fused.	Ampères	
0·004	0·010	42	1·427	(*a*) 2·243 (*b*) 1·876	2·536	2·536	9023·3
0·005	0·013	39	1·851	2·614	4·023	3·549	11372·0
0·007	0·018	37	3·138	3·861	5·712	5·874	9782·6
0·010	0·025	33	4·465	5·632	10·138	10·024	10132·0
0·012	0·030	30	6·115	8·610	13·840	13·178	10523·0
0·014	0·036	28	7·722	(*a*) 13·200 (*b*) 11·176	16·412	16·606	9902·4
0·018	0·046	26	8·810	(*a*) 18·823 (*b*) 14·602	22·688	24·206	9389·4
0·020	0·051	25	13·032	(*a*) 22·445 (*b*) 17·380	28·236	28·360	9977·3
0·026	0·066	22	20·122	(*a*) 35·400 (*b*) 33·783	44·256	42·040	10551·0
0·030	0·076	21		(*a*) 44·240 (*b*) 33·790	49·88	52·100	9597·2
						Mean =	10025·0

Note.—The wire becomes red, and then immediately much brighter (a dull white), owing probably to oxidation. To reproduce faint redness without breaking the circuit, the current can be considerably reduced. "*a*" is the current which caused the first visible rays of light, and so quickly changed the wire to a brighter state of incandescence, while "*b*" is the reduced current which reproduced the first redness. If the experiment be repeated, the same effects are obtained, although the molecular structure of the wire seems to be much changed by the first heating. After fusing the wire, a white powder, alumina, is found, and sometimes a white opaque bead. A wire 18 mils diameter and 10 inches long was raised to faint red with 11·22 ampères; it glowed (dull white) on one side of loop with 11·58 ampères, and when the heat had apparently spread over the whole length uniformly, redness reappeared, and the current was found to be again 11·22 ampères. The wire was next raised to a moderately white incandescent state with 15·93 ampères, and with this current broke in two minutes.

Platinum.

Diameter of wire.			Current in ampères.			Fusing current calculated from the formula $ad^{3/2}$. Ampères.	Constant "a" when d expressed in inches.
In inches.	In centimetres.	Standard wire gauge No.	Shellac flake melted.	Wire red hot. Visible in air.	Wire fused.		
0·010	0·025	33	2·365	3·915	6·281	6·008	6278·8
0·012	0·030	30	3·060	5·424	8·076	7·90	6141·3
0·014	0·036	28	3·916	6·688	9·625	9·95	5808·4
0·018	0·046	26	4·936	8·320	13·378	14·50	5537·1
0·020	0·051	25	5·926	11·430	17·780	17·00	6282·3
0·026	0·066	22	8·808	16·316	25·778	25·20	6145·8
0·030	0·076	21	9·618	20·32	30·47	31·20	5863·6
						Mean =	6008·2

Note.—The ratio of perceptible warmth : red heat : fusing point is (roughly) as 1 : 2 : 3 in platinum.

German Silver.

Diameter of wire.			Current in ampères.			Fusing current calculated from the formula $ad^{3/2}$. Ampères.	Constant "a" when d expressed in inches.
In inches.	In centimetres.	Standard wire gauge No.	Shellac flake melted.	Wire red hot. Visible in air.	Wire fused.		
0·005	0·013	39	0·910	1·530	2·150	2·062	6078·4
0·007	0·018	37	1·406	2·150	2·812	3·415	4799·3
0·009	0·023	34	1·905	3·492	5·000	4·978	5860·7
0·012	0·030	30	2·977	5·460	7·693	7·666	5849·8
0·014	0·036	28	3·639	6·619	9·926	9·660	5989·3
0·018	0·046	26	4·757	8·894	13·235	13·560	5478·1
0·020	0·051	25	5·790	11·167	15·717	15·880	5550·5
0·026	0·066	22	9·846	15·342	23·740	23·550	5060·0
0·030	0·076	21	15·055	23·740	30·690	30·300	5901·1
						Mean =	5618·6

Platinoid.

Diameter of wire.			Current in ampères.			Fusing current calculated from the formula $ad^{3/2}$. Ampères.	Constant "*a*" when *d* expressed in inches.
In inches.	In centimetres.	Standard wire gauge, No.	Shellac flake melted.	Wire red hot. Visible in air.	Wire fused.		
0·007	0·018	37	1·609	2·092	3·218	3·223	5491·8
0·010	0·025	33	2·414	3·379	5·551	5·503	5195·1
0·012	0·030	30	2·655	4·666	6·998	6·820	5325·1
0·014	0·036	28	3·540	5·108	8·368	8·590	5045·9
0·018	0·046	26	4·626	8·042	12·230	12·530	5062·5
0·020	0·051	25	6·636	9·604	14·480	14·680	5116·6
0·027	0·069	22	9·253	13·275	23·332	23·020	5256·2
0·035	0·089	20	11·263	18·502	32·986	33·200	5035·3
						Mean =	5191·1

Iron.

Diameter of wire.			Current in ampères.			Fusing current calculated from the formula $ad^{3/2}$. Ampères.	Constant "*a*" when *d* expressed in inches.
In inches.	In centimetres.	Standard wire gauge No.	Shellac flake melted.	Wire red hot. Visible in air.	Wire fused.		
0·007	0·018	37	1·101	1·713	1·998	1·930	3410·3
0·010	0·025	33	2·121	2·896	3·466	3·435	3460·7
0·012	0·030	30	2·406	3·425	3·996	4·320	3038·8
0·014	0·036	28	3·467	4·364	5·506	5·450	3326·5
0·018	0·046	26	3·915	6·200	7·750	7·950	3208·4
0·020	0·051	25	5·028	6·758	9·012	9·310	3180·9
0·026	0·066	22	6·362	11·500	13·212	13·800	3148·4
0·030	0·076	21	8·483	14·843	17·292	17·100	3326·1
0·036	0·091	20	13·702	22·510	24·145	22·500	3533·2
						Mean =	3292·6

I was anxious to see if the shellac flake had any influence on the fusing current:—

(*a.*) Shows the effect with shellac.

(*b.*) Without shellac.

Tin.

Diameter of wire.			Current in ampères.			Fusing current calculated from the formula $ad^{3/2}$. Ampères	Constant "*a*" when *d* expressed in inches.
In inches	In centimetres.	Standard wire gauge No.	Shellac flake melted.	Wire red hot. Visible in air.	Wire fused.		
0·010	0·025	33	1·931	2·413	2·736	2·760	2730·7
0·014	0·036	28	3·181	(*b*) 4·282	(*a*) 3·630 5·058	4·570	3051·5
0·018	0·046	26	4·078	(*b*) 5·384	(*a*) 4·976 6·117	6·670	2884·8
0·020	0·051	25	4·485	(*b*) 6·812	(*a*) 6·281 7·667	7·810	2709·2
0·026	0·066	22	6·933	(*b*) 11·100	(*a*) 10·443 12·154	11·600	2897·5
0·030	0·076	21	9·300	(*b*) 13·703	(*a*) 12·725 13·950	14·350	2683·3
0·033	0·084	21	11·380	(*b*) 14·930	(*a*) 14·192 15·414	16·500	2570·1
0·036	0·091	20	11·745	(*b*) 17·620	(*a*) 15·908 17·860	18·800	2575·2
						Mean =	2762·8

Hence it appears that shellac acts as a flux and prevents oxidation. Thus tin fuses at a temperature less than that of luminosity.

Tin-Lead Alloy.

Diameter of wire.			Current in ampères.			Fusing current calculated from the formula $ad^{3/2}$. Ampères.	Constant "a" when d expressed in inches.
In inches.	In centimetres.	Standard wire gauge No.	Shellac flake melted.	Wire red hot. Visible in air.	Wire fused.		
0·010	0·025	33	. . . .	* 2·494	2·132 2·735	2·491	2731·7
0·012	0·030	30	2·333	* 2·855	2·373 3·097	3·274	2354·7
0·014	0·036	28	. . . .	* 3·942	3·298 4·586	4·127	2774·2
0·020	0·051	25	4·586 . .	* 5·792	5·309 6·838	7·046	2420·1
0·026	0·066	22	7·566 . .	* 9·331	8·206 9·978	10·220	2385·5
0·030	0·076	21	11·102 . .	* 12·070	11·102 12·070†	12·670	2321·5
0·033	0·084	21	11·102 . .	* 14·000	12·550 14·000†	14·620	2333·9
0·036	0·091	20	13·516 . .	* 14·962	14·240 14·962†	16·660	2189·7
						Mean =	2438·9

* With shellac.

† Fused immediately after faint redness was visible.

Lead.

Diameter of wire.			Current in ampères.			Fusing current calculated from the formula $ad^{3/2}$.	Constant "*a*" when *d* expressed in inches.
In inches.	In centimetres.	Standard wire gauge No.	Shellac flake melted with.	Wire red hot. Visible in air.	Wire fused.	Ampères.	
0·010	0·025	33	1·666 ..	* 1·984	1·984 2·341	1·990	2339·5
0·012	0·030	30	1·825 ..	* 2·461	2·659 2·777	2·616	2111·6
0·014	0·036	28	3·095 ..	* 3·016	3·095 3·821	3·296	2305·3
0·018	0·046	26	3·016 ..	* 3·889	4·023 4·383	4·640	1811·3
0·020	0·051	25	3·810 ..	* 4·827	4·907 4·927	5·430	1712·3
0·026	0·066	22	5·471 ..	* no redness	7·000 7·000	8·344	1668·5
0·030	0·076	21	6·838 ..	* no redness	8·366 8·850	8·980	1702·1
0·033	0·084	21	6·526	..	10·93	11·520	1828·2
0·036	0·091	20	7·831	..	12·40	13·120	1814·4
						Mean =	1921·5

* Lead wire fuses without previously emitting light when a small shellac flake touches the wire.

Series II.

The second series of experiments was made to determine the relative effect of the sudden application of powerful currents on wires of different materials such as would occur if in practice a short circuit suddenly took place. An electromotive force of 100 volts was used, and there being no appreciable resistance in the external circuit but the wire, the latter was subjected to the blow of a momentary current of immense and immeasurable strength.

Metal.	Gauge.	Remarks.
	inches.	
Tin	0·0185	Fused with a sharp report, and scattered molten particles quite 6 feet in all directions.
"	0·136	Fuse produced little more than a large splay of metal.
"	0·136 (repeated)	This wire was enclosed in a porcelain box covered by a glass plate. It fused with considerable flame. The glass was broken into fragments, and the porcelain box chipped. Some fiery particles were thrown about.
"	0·064	One inch of wire was put into an earthenware box. When fused, the particles of metal were securely imprisoned by the box to which they adhered. All the lead was resolved into globules.
Platinum - silver alloy	0·061	Molten particles were shot a distance of 9 feet.
Platinum foil......	0·001 thick	Molten particles thrown about 4 feet.
"	0·001 thick, 0·512 wide	" " "
"	0·00025 thick, ½ in. wide	This strip of foil was 1¼ in. long. Sparks thrown a few inches only.
Aluminium foil	0·001 thick	Molten particles scattered about 9 feet.
"	0·004 thick, ½ in. wide	Incandescing particles thrown upwards and around, but not more than 3 feet distant.
"	0·001 thick, ½ in. wide	Profuse particles, and some thrown 6 feet distant in a white hot state.
Silver foil	0·001 thick, $\frac{21}{32}$ in. wide.	Not so much splaying as in last experiment.
Pure silver wire....	0·017	No incandescent particles reached the ground. The wire was destroyed with a sharp report.
Zinc foil	0·003 thick, ½ in. wide	Better than silver foil, no particles being scattered.
"	0·003 thick, ½ in. wide 2 strips	A few particles were shot about 4 feet; one was of considerable size.
Copper wire.......	No. 20 B.W.G.	Large incandescent globules scattered around for a distance of 4 or 5 feet.
Brass wire	No. 18 B.W.G.	This went off with a flash, and threw to a short distance a splay of metal which remained incandescent for some moments, and burnt a hole in the table.
Hard-drawn bright steel wire	No. 18 B.W.G.	Scintillating particles flew in all directions to a great distance. This was the most dangerous break of all the experiments.
Mercury	.. ..	Considerable flame produced, and particles widely scattered.

The conclusions derived from these experiments were that the best metal to use for small diameters was platinum, and for large wires tin. Platinum fuses in a wax-like kind of way without explosion or scattering of molten particles. Platinum has great advantages over other materials; it neither tarnishes nor deteriorates. It is easily soldered.

Tin behaves very much in the same way when its dimensions are large. But it is very questionable whether large wires should ever be used for fusible cut-outs. Owing to radiation the surface keeps cool and solid, while the centre is molten and liquid. It bursts with an explosion, and the incandescent particles are forced away radially in all directions with considerable energy.

Fusible cut-outs are effective but somewhat barbarous, and from the absence of any scientific enquiry into their character and judgment in their use, they have in the majority of instances become rather a source of danger than of safety.

Series III.

The third series of experiments was made to determine the constant "*a*" when each wire was 6 inches long and therefore free from any cooling effect of the terminals.

Copper.

Diameter in inches.	Actual fusing current in ampères.	Fusing current calculated. $ad^{3/2}$.	Constant "*a*."
0·004	3·253	2·956	12888
0·005	4·444	4·130	12569
0·007	7·618	6·842	13007
0·010	13·33	11·684	13330
0·013	15·55	17·32	10491
0·014	17·14	19·35	13835
0·018	25·55	28·22	10580
0·020	27·77	33·04	9818
0·023	35·55	40·75	10192
0·030	52·69	60·71	10140
		Mean =	11684

Aluminium.

Diameter in inches.	Actual fusing current in ampères.	Fusing current calculated. $ad^{3/2}$.	Constant "*a*."
0·004	3·322	2·011	13130
0·007	5·253	4·654	8970
0·010	10·20	7·948	10200
0·012	10·51	10·45	7996
0·014	16·19	13·17	9757·3
0·018	21·01	19·20	8700
0·020	23·48	22·48	8302
0·026	28·93	33·33	6900
0·030	37·08	41·30	7133
0·033	38·93	47·65	6493
0·036	43·80	54·30	6413
0·040	52·53	63·57	6568
		Mean =	7948·4

Platinum.

Diameter in inches.	Actual fusing current in ampères.	Fusing current calculated. $ad^{3/2}$.	Constant "*a*."
0·004	1·723	..	6826·5
0·005	2·192	1·859	6200
0·007	3·211	3·080	5482·7
0·010	5·285	5·258	5285
0·012	6·734	6·910	5122·8
0·014	8·104	8·710	4884
0·018	11·28	12·700	4671
0·020	13·78	14·872	4872
0·027	22·55	23·330	5082·8
0·030	28·12	27·320	5411·8
0·033	32·75	31·520	5463·2
0·037	37·08	37·420	5209·9
0·040	43·26	42·063	5407·4
		Mean =	5258

German Silver.

Diameter in inches.	Actual fusing current in ampères.	Fusing current calculated. $ad^{3/2}$.	Constant "a."
0·004	1·825	1·317	7230·7
0·005	2·143	1·840	6061·3
0·010	5·554	5·204	5554
0·012	6·824	6·840	5191·2
0·014	9·125	8·620	5499·4
0·018	12·78	12·57	5292·2
0·020	14·40	14·72	5091·2
0·026	20·16	21·82	4808·8
0·030	27·14	27·04	5223·1
0·033	30·90	31·20	5150
0·037	36·15	37·03	5079
0·040	43·26	41·63	5407
		Mean =	5203·7

Platinoid.

Diameter in inches.	Actual fusing current in ampères.	Fusing current calculated. $ad^{3/2}$.	Constant "a."
0·007	3·675	2·846	6275
0·010	5·285	4·860	5285
0·012	6·532	6·389	4969·1
0·014	8·036	8·050	4843·1
0·018	11·670	11·74	4832·6
0·020	14·21	13·75	5024
0·016	21·38	22·77	4563·2
0·028	28·436	29·13	4744
0·035	28·82	31·82	4401·3
0·040	40·67	38·88	5084
		Mean =	4860·7

Iron.

Diameter in inches.	Actual fusing current in ampères.	Fusing current calculated. $ad^{3/2}$.	Constant "*a*."
0·007	2·10	1·869	3585·6
0·012	3·88	4·194	2951·7
0·014	5·277	5·285	3180·3
0·018	7·142	7·706	2957·4
0·020	8·888	9·026	3142·3
0·026	13·02	13·38	3105·7
0·030	15·71	16·58	3023·4
0·033	19·00	19·13	3169·0
0·036	21·90	21·80	3206·2
0·040	28·74	25·53	3592·5
		Mean =	3190·9

Tin.

Diameter in inches.	Actual fusing current in ampères.	Fusing current calculated. $ad^{3/2}$.	Constant "*a*."
0·010	2·55	1·800	2550
0·014	3·244	2·983	1959
0·018	4·095	4·348	1696
0·020	4·675	5·093	1653
0·026	6·570	7·548	1567
0·030	8·656	9·356	1666
0·033	9·430	10·800	1573
0·036	11·60	12·30	1699
0·040	13·14	14·41	1643
		Mean =	1800·6

Tin-Lead Alloy (2 parts of Lead to 1 part of Tin).

Diameter in inches.	Actual fusing current in ampères.	Fusing current calculated. $ad^{3/2}$.	Constant "a."
0·010	2·124	1·455	2124
0·0125	2·395	2·034	1714
0·014	2·472	2·411	1493
0·018	3·283	3·515	1359·5
0·020	3·515	4·117	1243
0·026	5·794	6·101	1382
0·030	6·990	7·562	1345
0·033	7·722	8·725	1289
0·036	8·961	9·941	1312
0·040	10·35	11·64	1294
		Mean =	1455·5

Lead.

Diameter in inches.	Actual fusing current in ampères.	Fusing current calculated. $ad^{3/2}$.	Constant "a."
0·010	1·893	1·512	1893
0·012	2·202	1·988	1675
0·014	2·588	2·504	1565
0·018	3·824	3·652	1584
0·020	4·171	4·277	1475
0·026	6·025	6·339	1437
0·030	7·182	7·858	1382
0·036	8·600	10·33	1259
0·040	10·74	12·10	1342·5
		Mean =	1512·27

The value of the constant "a" for the different metals is therefore :—

	Inches.	Centimetres.
Copper	11684·0	2886·0
Aluminium	7948·4	1964·0
Platinum	5258·0	1299·0
German silver	5203·7	1285·0
Platinoid	4860·7	1201·0
Iron	3190·9	788·0
Tin	1800·6	445·0
Alloys (lead and tin, 2 to 1)	1455·5	359·5
Lead	1512·27	373·5

The values in the second column are obtained from those in the first by multiplying the latter by $\frac{1}{(2{\cdot}54)^{3/2}} = 0{\cdot}247$.

Since $C = ad^{3/2}$ gives the fusing current of any wire of a given diameter d, inversely—

$$d = \left(\frac{C}{a}\right)^{2/3}$$

will give the diameter of the wire which will fuse with a given current C. Very useful tables can thus be calculated which would be of service to the electric light engineer.

[Jan. 5, 1888.—In all these experiments the results obtained on wires finer than those recorded, viz., those below 10 mils, were excluded, because it was found that they did not follow the law of the 3/2 power. In the discussion which followed the reading of the paper, Professor Ayrton pointed out that this must be so, and that it followed from Mr. Box's researches of 1868* that the current required to maintain a fine wire of a given material at a given definite excess of temperature is approximately directly proportional simply to the thickness of the wire. This has been fully developed in a paper read before the Society of Telegraph-Engineers and Electricians, November 24, 1887 ('Journal,' vol. 16, p. 539).]

* 'A Practical Treatise on Heat,' 1868.

II. "A Contribution to the Study of the Comparative Anatomy of Flowers." By Rev. G. HENSLOW, M.A., F.L.S. Communicated by Dr. B. W. RICHARDSON, F.R.S. Received December 2, 1887.

(Abstract.)

The author first drew attention to the importance of the class of observations illustrated in this paper; for by referring all the floral organs back to their vascular cords, or "axial traces," their real origins could be discovered, whenever their developmental history was incapable of showing them.

Taking the cords as "floral units," he showed how they can give rise to axes as well as all kinds of floral appendages. The two elements of which a cord is composed are tracheæ or spiral vessels and sieve-tubes, &c.. or soft bast. The significance of the relative positions of these two elements was pointed out, and M. Ph. van Tieghem's distinction between axial and foliar characters of cords, *i.e.*, in having the tracheæ on the side of the medulla in the former, and on the outside in the latter, was criticised as being by no means constant, especially as regards the floral cords; inasmuch as a more general rule is for the tracheæ of the latter to be exactly central or scattered irregularly in a groundwork of phloëm.

After describing the arrangements in peduncles and pedicels in which endogens often have their cords as regularly placed as in exogens, the author explained the different ways by which pedicels of umbells are formed in each class respectively, and how they are supplied with cords from the common peduncle.

He next pointed out the phyllotactical origin of the number of parts in floral whorls, and how the various arrangements of their members become altered in consequence of the union of their cords below, so that the proper angular divergences are not maintained, and parts often become superposed which would otherwise alternate in position.

The union, separation, reunion and fusion of cords, as well as the way in which they may shift their positions, were discussed, and the effects produced by such processes were explained.

The results of the multiplication of parts brought about by "chorisis" of a cord were illustrated; whereby a simple cord of a pedicel could give rise to any number of floral parts, such as the members of different whorls, as in the case of *Campanula medium*, in which a simple axial cord supplied a sepaline, a dorsal carpellary, a staminal and *half* a petaline cord: or when a repetition of the same kind occurs, as in double flowers.

Considerable light is thrown upon the phenomena of cohesion and adhesion by this method of investigation; and especially on *the undifferentiated state of organs when in congenital union.* This, if thoroughly understood, completely clears up the difficulties surrounding the interpretation of the "receptacular tube" and the "inferior ovary."

The investigation into the character and distribution of the vascular cords reveals the true nature of the axile and free central placentations; in the former case, it shows that with scarcely any exception the axis takes no part in the structure, all "carpophores," "stylopods," &c., being simply the coherent and hypertrophied margins of the carpels.

Similarly the free-central placenta of *Primulaceæ* received its interpretation as being coherent and ovuliferous bases of five carpels which have the upper parts of their margins cohering in a parietal manner and without ovules.

The illustrations are of about sixty genera, and nearly twenty orders.

The author proposes continuing his observations.

III. "The early Stages in the Development of *Antedon rosacea.*" By H. BURY, B.A., F.L.S., Scholar of Trinity College, Cambridge. Communicated by P. HERBERT CARPENTER, D.Sc., F.R.S., F.L.S. Received December 7, 1887.

(Abstract.)

The materials for this study were obtained from Naples in the winter of 1886–87. In the orientation of the larva, J. Barrois' suggestion ('Comptes Rendus,' November 9th, 1886) has been adopted, viz., that the stalk of the pentacrinoid represents the præoral lobe of other Echinoderm larvæ.

Development.

External Form.—Segmentation is regular, and a gastrula is formed by invagination. The blastopore closes early and the larva gradually elongates. Ciliation is at first uniform, but soon an anterior tuft of cilia and five ciliated bands become visible, and the intermediate cilia disappear. The anterior ciliated band is incomplete ventrally, and is either absent in the British form or escaped Wyville Thomson's notice. Two ciliated depressions also appear on the ventral surface. The anterior one ("pseudoproct" of W. Thomson) may be called the "præoral pit;" and the posterior one ("pseudostome") the "larval mouth." The "yellow cells" (green by transmitted light) appear

before the rupture of the vitelline membrane, and are absent from the ciliated areas.

The free larva swims with the terminal tuft of cilia directed forwards. A white patch on its left side between the third and fourth ciliated bands marks the position of the "water-pore."

Internal Anatomy.—The gastrula has at first no mesoderm, but this soon becomes budded off from the archenteron. The blastopore closes near the posterior end, but whether ventrally or dorsally could not be determined. The archenteron, which only occupies the posterior half of the larva, soon divides into two parts; the posterior of these (enterocele) assumes the form of a dumb-bell, round the constricted part of which the anterior half (mesenteron) grows till it forms a complete ring. The two swellings of the dumb-bell soon separate to form the right and left body-cavities respectively. From the anterior part of the mesenteron are budded off the hydrocele (left and ventral), and an unpaired anterior body-cavity.

By a change in position of the right and left body-cavities (incorrected described by Götte), the left body-cavity becomes posterior and ventral, while the right becomes anterior and dorsal: the latter sends a five-chambered prolongation into the præoral lobe, to form the rudiment of the "chambered organ." The hydrocele forms a ring, incomplete towards the left, on the ventral side of the mesenteron, and soon forms five ventral pouches. Shortly before fixation, the anterior body-cavity, which extends far into the præoral lobe, opens to the exterior on the left side by the "water-pore."

Underneath the anterior tuft of cilia and the præoral pit, and down the sides of the larval mouth, run fine fibres, which appear to be parts of a larval nervous system which disappears when the larva loses its freedom.

Fixation and Subsequent Changes.

After swimming freely for about twenty-four hours, the larva fixes itself by means of the præoral pit, which forms the disk of attachment. The ciliated bands then disappear, and the larval mouth invaginates to form the vestibule, which is rotated to the posterior end, as described by Barrois ('Comptes Rendus,' May 24th, 1886). At the same time all the tissues undergo histolysis, and the mesenteron becomes filled with cells budded in from the centre of the hydrocele ring.

The right and left body-cavities, which are now both dorsal, grow rapidly round to the original ventral side, being separated by a transverse mesentery, and each forms a longitudinal mesentery near the original ventral radius. The free end of the larva may be called the oral end, since the mouth now appears as a depression in the floor of the vestibule.

The anterior body-cavity is now small and lies near the oral end in the body-wall. Into it opens the water-tube or stone-canal, which runs from the water-vascular ring in the oral longitudinal mesentery, and is distinguishable from the anterior body-cavity by its higher epithelium. It is not, therefore, in direct continuity with the water-pore. The anus opens externally in the same interradius as the water-pore.

The Skeleton remains to be described. Shortly after the orals and basals have appeared, three small plates are developed at the posterior end of the stem, which resemble the basals in form but are not derived from them. They are so arranged that the most dorsal, which is smaller than the other two, lies on the right side opposite the interradius of the water-pore. These three plates are the undoubted homologues of the under-basals of the dicyclic Crinoids (*Poteriocrinus*, *Encrinus*, *&c.*). Shortly after the fixation of the larva they fuse with one another and with the top stem-joint, so as to form a large plate which has hitherto been mistaken for a simple centrodorsal. The five radial angles of this plate belong to the under-basals, and it is only at a much later period that these angles are hidden by the growth of the true centrodorsal (= top stem-joint), the angles of which become interradial when its cirri appear.

IV. "Heat Dilatation of Metals from low Temperatures." By THOMAS ANDREWS, F.R.S.E. Communicated by Professor G. G. STOKES, P.R.S. Received November 30, 1887.

It is understood that the coefficients of heat dilatation increase with rise of temperature; but Professor P. G. Tait, in his recent work on 'Heat,' p. 87, remarks that "we are not aware of any experiments made with a view of deciding whether, as is probable, these coefficients become gradually less as the temperature is lowered below zero" (0° C.).

The following experiments were made to investigate the subject in relation to metals of the iron and steel series. The varieties of modern steels manufactured by recent processes manifest properties sufficiently diverse as almost to constitute them distinct groups of metals, although for practical purposes they are conveniently grouped under the generic name of steel. Some of these modern metals have recently been so largely used for constructive purposes that the author considered it desirable to obtain an approximate quantitative estimation of their dilatation by heat through varied ranges of temperature. The rolled metals under observation in the experiments consisted of round polished bars, 3 inches diameter, and 13 inches long, planed perfectly square at each end; they were care-

Table I.

Analyses of Wrought Iron, Steels, and Cast Metals employed.

Rolled Bars.

Description.	Graphitic carbon.	Combined carbon.	Silicon.	Sulphur.	Phos- phorus.	Manganese.	Iron (by difference).	Total.	Specific gravity.
Wrought iron (Wortley best scrap) .	..	None.	0·392	0·034	0·270	0·194	99·110	100	7·590
Bessemer steel, "soft"	..	0·150	0·009	0·112	0·088	0·468	99·173	100	7·853
,, "hard"	..	0·480	0·121	0·096	0·089	0·684	98·530	100	7·838
Siemens-Martin steel, "soft"	..	0·230	0·014	0·100	0·075	0·698	98·883	100	7·856
,, "hard"	..	0·460	0·107	0·023	0·075	0·972	98·363	100	7·845
Cast steel, "soft"	..	0·450	0·016	0·027	0·048	0·086	99·373	100	7·863
,, "hard"	0·259	1·190*	0·175	0·063	0·019	0·396	97·898	100	7·805
Cast metal "best"	2·780	0·390*	2·340	0·090	0·580	0·450	93·370	100	7·206
,, "common"	2·620	0·670*	1·940	0·090	0·950	0·520	93·210	100	7·134
Hammered Forgings.									
Wrought iron (Wortley best scrap).	..	0·038	0·117	0·019	0·246	0·112	99·468	100	

* Combined carbon in these samples was determined by combustion, and in the other samples by the colour test.

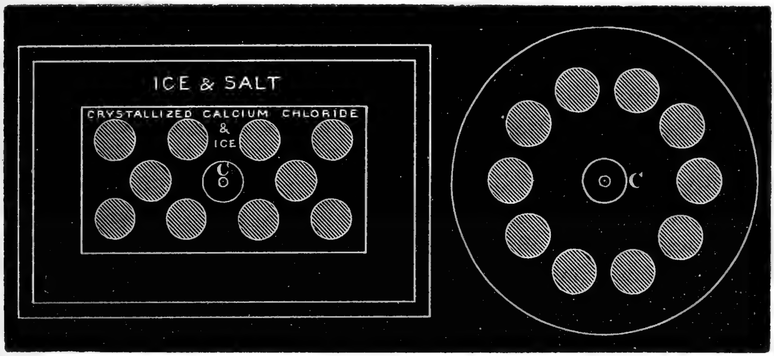
The terms "soft" and "hard" relate only to difference of percentage of combined carbon, and not to any annealing or hardening processes.
The metals were so prepared as to obtain a wide difference in the percentage of combined carbon between the "soft" and "hard" varieties.

fully manipulated during manufacture, and were selected from the author's standard samples, having the chemical composition given in Table I.

The range of temperature chosen for the observations was from —45° C. to 300° C.

The experiments were conducted as follows:—For the measurements commencing at the low temperature of —45° C., the bars (having previously been slowly reduced to the temperature of 0° C., and then gradually cooled to —18° C.) were placed upright in the bath A (see fig.), and immersed in a freezing-mixture of three parts of calcium chloride and two parts of snow, each of these ingredients previous to mixing being maintained in separate jacketed freezing

Bath A for Temperature of —45° C. Bath B for Temperature of —18° C.



Ground Plan. Ground Plan.

Scale, $\frac{3}{4}$ inch = 1 foot.

tanks at a temperature of —18° C. The vessel A, containing the bars and the calcium chloride freezing-mixture, was further surrounded by another compartment holding a quantity of a freezing-mixture of snow and salt at a temperature of —20° C. By this means and by constantly renewing the calcium chloride and snow mixture during the experiments, an uniform temperature of —45° C., as registered by an alcohol thermometer, was maintained for the experiments in the cold bath A.

Much larger cooling tanks of a snow capacity for each charge of 8 cwts. were used for the large forgings, and a large cast metal oil-bath having a capacity of about 70 gallons of oil was used for the highest temperature.

The bars remained thus immersed in the freezing bath whilst their internal temperature was regularly ascertained by another alcohol thermometer placed in a hole in the centre of the test bar C, wherein was also placed a little alcohol. When the bars had reached and remained for some time at the registered temperature of −45° C., each was in turn removed and placed on a suitable wooden frame, and its length instantly and carefully measured by telescopic readings from a delicate micro-vernier gauge (deviations of $\frac{1}{2000}$ of an inch were perceptible) also supported on a suitable rigid stand. The bars were then replaced for a short time in the freezing-mixture and again removed and their diameter then carefully measured. No perceptible alteration in the temperature of the bars occurred during the very short time occupied in taking the observations, and frequent tests were made to ascertain this. The average of about thirty measurements in each case, both longitudinal and transverse, was regarded as fairly accurate. The dimensions of the bars were taken in a similar manner for the temperature from −18° C., substituting in another cold bath, B, a freezing-mixture of snow and salt to obtain this temperature, and using powdered ice and snow for the observations at 0° C. The higher temperature observations were obtained by heating the whole of the bars in a large hot-water bath for the period necessary to insure that their temperature throughout was as required, and the oil bath was used for the temperature of 300° C. Liability to temperature errors was, as far as possible, carefully guarded against by constant reference and comparison between the bath thermometers and that in the centre of the test bar, and by keeping the bars immersed during sufficiently long periods.

The hammered metals under observation were large forgings of the different metals 7 feet 3 inches long, and 5 inches diameter, planed perfectly square at the ends and turned and polished bright. The measurements were taken on the total length of the forgings, as in the case of the rolled metals, to ensure greater accuracy, the experiments being conducted in somewhat similar manner: but owing to the greater length of the forgings, a modification of the method was made. One end of the forging was rigidly secured and the expansion ascertained by measuring the diminishing space between the other end of the forging and a fixed point situated a distance from it. The results are recorded on Table II.

General Remarks.

It is interesting to notice that the coefficients of dilatation were greater in the case of the "soft" than the "hard" steels, a circumstance which may be accounted for by a reference to Table I of the analyses, from which it will be seen that the percentage of combined carbon was much lower in the "soft" than in the "hard" steels, the percentage of pure iron was consequently also greater in the "soft" steels, this caused them to be of a greater specific gravity. The

Table II.

Rolled Bars.

Description.	Coefficients of linear dilatation for 1° C. between			1000 parts at −45° C. became at 300° C.	
	−45° and 100° C.	−18° and 100° C.	100° and 300° C.	Longitudinal.	Across the diameter.
Wrought iron (Wortley best scrap)	0·0000086	0·0000114	0·0000133	1003·638	1003·588
Bessemer steel, "soft"	0·0000093	0·0000117	0·0000159	1004·133	1003·944
,, "hard"	0·0000085	0·0000101	0·0000133	1003·746	1004·322
Siemens-Martin steel, "soft"	0·0000088	0·0000116	0·0000144	1003·807	1003·946
,, "hard"	0·0000079	0·0000100	0·0000139	1003·731	1003·570
Cast steel, "soft"	0·0000086	0·0000112	0·0000150	1003·755	1003·502
,, "hard"	0·0000084	0·0000101	0·0000130	1003·577	1003·411
Cast metal "best"	0·0000088	..	0·0000137	1003·637	1003·621
,, "common"	0·0000088	0·0000090	0·0000152	1003·784	1003·579
Large hammered Forgings.				1000 parts at 0° C. became at 300° C.	
				Longitudinal.	Across the diameter.
Wrought iron (Wortley best scrap)	0·0000096	0·0000117	0·0000131	1003·944	1003·537
Do. do.*	0·0000081	0·0000104	0·0000157	1003·790	1003·330
Bessemer steel	0·0000099	0·0000107	0·0000137	1003·829	1003·601
Siemens-Martin steel	0·0000093	0·0000113	0·0000142	1003·953	1003·641

* This was a smaller forging, only 3 inches diameter and 13 inches long.

results on Table II appear also to indicate another circumstance of metallurgical interest, viz., that the dilatation was generally rather more in the direction of the length of the metallic cylinders than when measured across the diameter, numerous repeated experiments confirmed this. The result appears more marked in the large round forgings of hammered steels and wrought iron than in the case of the rolled bars. It would therefore seem probable that the crystalline particles of the metals suffer slight permanent alteration of form in the direction of their length during the process of rolling or drawing out, sufficient to very slightly affect their relative longitudinal and transverse dilatations.

Furthermore, the observations of this memoir, conducted at these very low temperatures, experimentally confirm the suggestion of Professor Tait, inasmuch as the coefficients of dilatation were found generally to decrease with the reduced temperature below 0°C. The author also found such to be the case in his observations on the "Heat Dilatation of pure Ice from very low Temperatures." (See 'Roy. Soc. Proc.,' June, 1886, No. 245, p. 544.)

It may be remarked that many tons of the various freezing mixtures, snow, &c., were required for the experiments.

Appendix.—Received January 12, 1888.

I think it would be misleading to use the figures, given in the second column (Table II), of the dilatation from −18° C. to 100° C. for purposes of exact comparison with the other results. The coefficients for dilatation between the small margin of −45° C. and −18° C. could not be accurately inferred from the results recorded in Table B, because the series of experiments from −18° C. to 100° C. were not made consecutively with the other observations. The molecular condition of the metals in that series (−18° C. to 100° C.) I consider was probably somewhat different. Judging from the whole of the results over the wider ranges of temperature, I do not think that the coefficients for the temperature between −45° C. and −18° C., whenever specially determined, will be found to be of a comparative negative character, or vitiate the general conclusions arrived at in this paper. The whole series of observations I believe coincide in establishing the reduction of the coefficients of heat dilatation with reduced temperature. I hope to make further investigations at these low temperatures.

The Society adjourned over the Christmas Recess to Thursday, January 12th, 1888.

Presents, December 22, 1887.

Transactions.

Brussels:—Musée Royal d'Histoire Naturelle de Belgique. Annales. Tome XIII. Text and Plates. Folio and Obl. Folio. *Bruxelles* 1886. The Museum.

Coimbra:—Universidade. Annuario. 1886–87. 8vo. *Coimbra* 1887. The University.

Graz:—Naturwissenschaftlicher Verein für Steiermark. Mittheilungen. Jahrg. 1886. 8vo. *Graz* 1887. The Verein.

London:—Foreign Office. Catalogue of Printed Books in the Library 31st December, 1885. Small Folio. *London* 1886. The Foreign Office.

Quekett Microscopical Club. Journal. Vol. III. No. 20. 8vo. *London* 1887; with July 1882, and January 1883. 8vo. *London.* The Club.

Royal Institute of British Architects. Journal of Proceedings. Vol. IV. No. 3. 4to. *London* 1887. The Institute.

Madrid:—Instituto Geográfico y Estadístico. Memorias. Tomo VI. 8vo. *Madrid* 1886. The Institute.

Montreal:—Canadian Society of Civil Engineers. Transactions. Vol. I. 8vo. *Montreal* 1887; Charter, Bye-Laws, and List of Members. 8vo. *Montreal* 1887. The Society.

Santiago:—Deutscher Wissenschaftlicher Verein. Verhandlungen. Heft 4. 8vo. *Valparaiso* 1886. The Verein.

Shanghai:—Royal Asiatic Society. China Branch. Journal. Vol. XXI. Nos. 5–6. 8vo. *Shanghai* 1887. The Society.

Stockholm:—Kongl. Svenska Vetenskaps-Akademie. Bihang till Handlingar. Bandet XI. Häfte 1. 8vo. *Stockholm* 1886. Bandet XII. Häfte 1-4. 8vo. *Stockholm* 1887; Öfversigt. Årg. 44. Nos. 7–8. 8vo. *Stockholm* 1887. The Academy.

Sydney:—Linnean Society of New South Wales. Proceedings. Vol. II. Part 1. 8vo. *Sydney* 1887. The Society.

University. Calendar. 1887. 8vo. *Sydney.* The University.

Tōkyō:—College of Medicine, Imperial University of Japan. Mitteilungen. Band I. No. 1. 4to. *Tokio* 1887. The College.

College of Science, Imperial University of Japan. Journal. Vol. I. Part 3. 4to. *Tōkyō* 1887. The College.

Toulouse:—Faculté des Sciences. Annales. Tome I. Année 1887. 4to. *Paris.* The Faculty.

Observations and Reports.

Brünn:—Meteorologische Commission des Naturforschenden Vereines. Bericht. 8vo. *Brünn* 1885. The Verein.

Calcutta:—Indian Museum. A Catalogue of the Moths of India. Part I. Sphinges. 8vo. *Calcutta* 1887. The Museum.

Meteorological Observations recorded at Six Stations in India. January to May, 1887. Folio. [*Calcutta*]. The Meteorological Office, Calcutta.

Coimbra:—Observatorio. Ephemerides Astronomicas. 1888. 4to. *Coimbra* 1887. The Observatory.

Dorpat:—Sternwarte. Meteorologische Beobachtungen. 1887. Februar–Mai. 8vo. *Dorpat.* The Observatory.

Dun Echt:—Observatory. Circular. Nos. 148–151. [Sheet] *Dun Echt* 1887. The Earl of Crawford, F.R.S.

Helsingfors:—Exploration Internationale des Régions Polaires, 1882–84. Expédition Polaire Finlandaise. Tome II. 4to. *Helsingfors* 1887. The Meteorological Office.

Hobart:—Parliament of Tasmania. Report on the State Reserve at Mount Wellington. Folio. *Tasmania* 1887; Annual Report, Woods and Forests of Tasmania, 1886–87. Folio. *Tasmania.* The Lands Office, Hobart.

India:—Geological Survey. Records. Vol. XX. Part 3. 8vo. [*Calcutta*] 1887. The Survey.

Kiel:—Commission zur Untersuchung der Deutschen Meere. Fünfter Bericht. 1882–1886. 4to. *Berlin* 1887; Ergebnisse der Beobachtungsstationen. Jahrg. 1886. Heft 7–12. Obl. 4to. *Berlin* 1887. The Commission.

London:—Board of Trade. Standards Department. Report. 1887. Folio. *London.* The Department.

Luxemburg:—Observations Météorologiques, faites a Luxembourg. Vols. III–IV. 8vo. *Luxembourg* 1887. Institut Royal Grand-Ducal de Luxembourg.

Melbourne:—Mining Department. Reports of the Mining Registrars, quarters ended 30th June and 31st March, 1887. Folio. *Melbourne.* The Department.

Milan:—Reale Osservatorio di Brera. Pubblicazioni. No. XXXI. 4to. *Milano* 1887. The Observatory.

Ashburner (C. A.) The Geologic Distribution of Natural Gas in the United States. 8vo. [*Philadelphia* 1886]; The Geologic Relations of the Nanticoke Disaster. 8vo. [*Philadelphia*] 1887. The Author.

Bary (A. de), For. Mem. R.S. Lectures on Bacteria. Translation by H. E. F. Garnsey. Revised by I. B. Balfour. Second improved edition. 8vo. *Oxford* 1887. The Clarendon Press.

Bierens de Haan (D.) Bouwstoffen voor de Geschiedenis der Wis- en Natuurkundige Wetenschappen in de Nederlanden. Verzameling 2. 8vo. *Leiden* 1887. The Author.

Blomefield (Rev. L.) Further Results of Meteorological Observations made at the Bath Royal Literary and Scientific Institution. 8vo. *Bath* 1887. The Author.

Collie (N.) Ueber einige Condensationsproducte des Amidoacetessigäthers mit Salzsäure. 8vo. *Berlin* 1887; On the Action of Heat on the Salts of Triethylbenzylphosphonium. 8vo. [*London*] 1887. The Author.

—— and E. A. Letts. On a New Method for the Preparation of Tin Tetrethyl. 8vo. [*London*] 1886; On the Salts of Tetrethylphosphonium and their Decomposition by Heat. 8vo. [*London*] 1886. The Authors.

Drygalski (E. von) Die Geoiddeformationen der Eiszeit. 8vo. *Berlin* 1887. The Author.

Dufourcet (E.) Influence des Phénomènes Sismiques sur l'intensité des Courants Telluriques. 8vo. *Dax* 1887. The Author.

Fodera (F. A.) La Funzione Cromatica nei Camaleonti. 8vo. *Palermo* 1887. The Author.

Ganser (A.) Das Ende der Bewegung. 8vo. *Graz* 1888. The Author.

Gray (T.) On an Improved Form of Seismograph. 8vo. *London* 1887. The Author.

Harley (G.), F.R.S. A Blackbird's Widowhood, Wooing, and Second Wedding. (Selborne Society Letters.) 8vo. [*London*] 1887. The Author.

Hirn (G.-A.) La Thermodynamique et l'étude du Travail chez les Êtres Vivants. 4to. *Paris* 1887; Construction et Emploi du Métronome en Musique. 4to. *Paris* 1887; Théorie et Application du Pendule à deux Branches. 4to. *Paris* 1887. The Author.

India Rubber, Gutta Percha, and Telegraph Works Company, Limited. Soundings taken 1885–87. 8vo. *London* 1887. The Company.

Katzenelsohn (N.) Ueber den Einfluss der Temperatur auf die Elasticität der Metalle. 8vo. *Berlin* 1887. The Author.

Kölliker (A.), For. Mem. R.S. Der jetzige Stand der morphologischen Disciplinen mit Bezug auf allgemeine Fragen. 8vo. *Jena* 1887. The Author.

Kops (Jan) Flora Batava. Afl. 277–78. 4to. *Leiden* 1887.
The Netherlands Legation.

Lawes (Sir J. B.), F.R.S. Memoranda of Field and other Experiments conducted on the Farm and in the Laboratory at Rothamsted. 4to. *London* 1887. The Author.

Martone (M.) Dimostrazione di un celebre Teorema del Fermat. 8vo. *Catanzaro* 1887; Sopra un Problema di Analisi Indeterminata. 8vo. *Catanzaro* 1887. The Author.

Michel (Prof. J.) Über Sehnerven-Degeneration und Sehnerven-Kreuzung. Festschrift der Medizinischen Fakultät der Universität Würzburg zur Feier des LXX. Geburtstages des Herrn Professor A. von Kölliker. 4to. *Würzburg* 1887.
Prof. Kölliker, For. Mem. R.S.

Norman (J. H.) Norman's Single Grain System. 8vo. *London* 1887. The Author.

Senier (A.) Ueber Cyanursäure, ihre Isomeren und Derivate. Inaugural-Dissertation. 8vo. *Berlin* 1887. The Author.

Smith (S. P.) The Eruption of Tarawera, New Zealand. 8vo. *Wellington* 1887. Survey Office, N.Z.

January 12, 1888.

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

The Right Hon. Arthur James Balfour, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

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

The following Papers were read:—

I. "Preliminary Note on the Nephridia of *Perichaeta*." By FRANK E. BEDDARD, M.A., Prosector to the Zoological Society of London, Lecturer on Biology at Guy's Hospital. Communicated by Professor E. RAY LANKESTER, M.A., LL.D., F.R.S. Received December 21, 1887.

The following observations are the result of a study of a species of *Perichaeta*, which is probably identical with Perrier's *P. aspergillum*.* I owe a number of excellently preserved examples to the kindness of Mr. Shipley, Fellow of Christ's College, Cambridge.

In transverse sections of the anterior segments the nephridia are seen to form numerous tufts of glandular tubules closely related to the body-wall and to the septa. This appearance, which is also seen in dissections, is very different from that of most other earthworms, and has been commented upon by other observers. Perrier, in fact, expressed the opinion that these organs in *Perichaeta* are in a rudimentary condition. I shall, however, bring forward reasons for believing that they are in a very archaic condition.

The remarkable appearance of the nephridia led me to infer that I should find the external apertures in each segment to be numerous, as I showed to be the case in *Acanthodrilus*.† I may take the opportunity of stating that in the latter species (*A. multiporus*) there is apparently a very much greater number of nephridiopores in the anterior than in the posterior segments; in certain of the anterior segments I counted over one hundred. I am now able to state that this is also the case in *Perichaeta* (in all probability in other species besides *P. aspergillum*). The external pores lie between the setæ, but have no regularity in their arrangement; frequently there were three or four between two successive setæ, as often there seemed to

* 'Paris, Mus. Hist. Nat. Nouv. Archives,' 1872.

† 'Roy. Soc. Proc.,' 1885.

be only one or two. The minute structure of the terminal section of nephridia is slightly different from that of *Acanthodrilus.* I have also found that *Typhœus* and a new genus, which I propose shortly to describe under the name of *Dichogaster*, have many nephridiopores in each segment. Another point, to which I wish to direct attention in this communication, is that *in Perichaeta there is a connexion between the nephridia of successive segments.*

Quite recently Ed. Meyer* and Cunningham† have shown that in *Lanice conchilega* the nephridia of each side are connected by a continuous longitudinal duct. This discovery is in accord with the presumed origin of the Annelid from the Platyhelminth excretory system, and also with the development of *Polygordius* (Hatchek) and *Lumbricus.*‡ In *Perichaeta* the connexion between the nephridial tufts of successive segments is not brought about by a continuous longitudinal duct, one on each side of the body, but by numerous tubules which perforate the intersegmental septum. Thus it appears that *the nephridial system of Perichaeta consists of a network of tubules.* In this respect *Perichaeta* agrees with the leech *Pontobdella,*§ but differs in the presence of numerous nephridiopores in each segment.

These facts appear to lend further support to the view that it is possible to derive the annelid from the platyhelminth excretory system. Lang has pointed out that the "secondary" pores by which the excretory organ of the Platyhelminths communicates with the exterior have probably given rise to the nephridial pores in the Annelida; by a subsequent arrangement of these in a metameric fashion, and by the breaking up of the nephridial network, the paired nephridia have originated. The longitudinal canal has disappeared, except in the cases that I have already mentioned. In some Platyhelminths the longitudinal canals are, partly at least, broken up into a network; and it is this condition which has persisted in *Perichaeta* and *Pontobdella;* moreover in some Platyhelminths, where the "secondary" pores have become metamerically arranged, there are more than one pair to each "segment." For this reason it is perhaps allowable to regard the condition of the nephridia in *Perichaeta* as more archaic than *Pontobdella.* The disappearance of the connexion between the nephridia of successive segments leads to the condition which exists in *Acanthodrilus;* the reduction of the external pores, already perceptible in the posterior segments of *A. multiporus*, culminates in the disappearance of all but two in each segment. The irregularity in the position of these, which is best marked in *Plutellus*, is the last trace of the presence of multiple nephridiopores in each segment.

* Quoted by Lang, "Die Polycladen."—Naples Monographs.

† 'Nature,' June 16, 1887.

‡ Wilson, 'Journal of Morphology,' vol. 1, No. 1.

§ 'Quart. Journ. Microsc. Sci.,' 1885.

II. "Invariants, Covariants, and Quotient Derivatives associated with Linear Differential Equations." By A. R. FORSYTH, M.A., F.R.S., Fellow of Trinity College, Cambridge. Received January 7, 1888.

(Abstract.)

The present memoir deals with the covariantive forms associated with the general ordinary linear differential equation; it is strictly limited to the consideration of those forms, without any discussion of their critical character.

The most general transformation, to which such equation can be subjected without change of linearity or of order, is one whereby the dependent variable y is transformed to u by a relation

$$y = uf(x),$$

and, at the same time, the independent variable x is changed to z; and, when these transformations are effected on

$$\sum_{r=0}^{r=n} \mathrm{P}_r \frac{n!}{r!\,\overline{n-r}!} \frac{d^{n-r}y}{dx^{n-r}} = 0,$$

so that it becomes

$$\sum_{r=0}^{r=n} \mathrm{Q}_r \frac{n!}{r!\,\overline{n-r}!} \frac{d^{n-r}u}{dz^{n-r}} = 0,$$

there are r relations between the coefficients P and Q; and P_0 and Q_0 may manifestly, without loss of generality, be taken equal to unity.

It is shown that, from these relations, others can be deduced which are of the type

$$\psi(\mathrm{P}) = \left(\frac{dz}{dx}\right)^{\rho} \psi(\mathrm{Q}),$$

where $\psi(\mathrm{P})$ is an algebraical function of the coefficient P and their derivatives. Such a function is called an invariant of index ρ; and irreducible invariants are of two classes, fundamental and derived.

It is convenient to have expressions for the invariants, when the differential equation has an implicitly general canonical form. In the first place it may be supposed that P_1 and Q_1 are both zero; otherwise both equations can by substitutions of Y for $y\,e^{\int \mathrm{P}_1 dx}$ and U for $ue^{\int \mathrm{Q}_1 dz}$ be reduced to forms in which the terms involving the $(n-1)$th differential coefficients of the dependent variable do not occur. The relation between the dependent variables is now

$$y = uz'^{-\frac{1}{2}(n-1)};$$

and the expressions of the simplest invariants are

$$\Theta_3 = P_3 - \frac{3}{2}P_2',$$

$$\Theta_4 = P_4 - 2P_3' + \frac{6}{5}P_2'' - \frac{3}{5}\frac{5n+7}{n+1}P_2^2,$$

$$\Theta_5 = P_5 - \frac{5}{2}P_4' + \frac{15}{7}P_3'' - \frac{5}{7}P_2''' - \frac{10}{7}\frac{7n+13}{n+1}P_2\Theta_3,$$

similar expressions being obtained for Θ_6 and Θ_7. The expression for each of the $n-2$ invariants of this class is shown to consist of two parts, one of which is linear in the coefficients and their derivatives, the other of which is not linear but every term contains as a factor either P_2 or some derivative of P_2.

It is then proved that there is an implicitly general form of the equation for which both Q_1 and Q_2 vanish; this form, taken as the canonical form, is obtainable (as is known from earlier investigations) by the previous determination of the multiplier of the dependent variable and by the determination of the independent variable from the equation

$$\{z, x\} = \frac{6}{n+1}P_2,$$

or its equivalent,
$$\frac{d^2\theta}{dx^2} + \frac{3}{n+1}P_2\theta = 0,$$

where $z' = \theta^{-2}$.

For this canonical form of equation the expressions of the foregoing $n-2$ invariants are given in the form

$$\Theta_\sigma = Q_\sigma + \tfrac{1}{2}\sigma \sum_{r=1}^{r=\sigma-3} (-1)^r \alpha_{r,\sigma} \frac{d^r Q_{\sigma-r}}{dz^r},$$

where $\alpha_{1,\sigma}$ is unity and, for values of r greater than 1,

$$\alpha_{r,\sigma} = \frac{(\sigma-1)(\sigma-2)^2(\sigma-3)^2 \ldots (\sigma-r+1)^2(\sigma-r)}{2 \cdot 3 \ldots r(2\sigma-3)(2\sigma-4) \ldots (2\sigma-r-1)}.$$

These invariants are called *priminvariants.*

The proof of these results occupies the second section of the memoir. The first section is devoted to a short historical sketch of the growth of the subject, reference being made to the investigations of Cockle, Laguerre, Brioschi, Malet, and, especially, of Halphen, all of whom have, so far as concerns the theory of forms, discussed either seminvariants only or, with the single exception of Θ_3 for the $n$tic, invariants of the cubic and the quartic in forms which differ from the canonical form herein adopted.

In the third section *derived invariants* are obtained, all in their canonical forms; they are derived from the priminvariants by one or other of two processes called the quadriderivative and the Jacobian. The irreducible invariants are ranged in classes according to their degrees. The quadrinvariants consist of $n-2$ functions,

$$\Theta_{\sigma,1} = 2\sigma\Theta_\sigma\Theta_\sigma'' - (2\sigma+1)\Theta_\sigma'^2,$$

and of $n-3$ independent functions of the form

$$\lambda\Theta_\lambda\Theta_\mu' - \mu\Theta_\mu\Theta_\lambda';$$

and every class of invariants of degree higher than the second contains $n-2$ invariants, each in that class associated with one of the priminvariants in successive derivation according to the law

$$\Theta_{\sigma,r} = \sigma\Theta_\sigma\Theta'_{\sigma,r-1} - r(\sigma+1)\Theta_\sigma'\Theta_{\sigma,r-1}.$$

Propositions relating to the dependence of the derived invariants are proved in the section; and simpler equivalent forms are obtained later in the memoir.

In the fourth section covariants are discussed. The transformation of the dependent variable in the second section shows that, with the adopted definition of invariance, viz., reproduction save as to a power of z', the dependent variable is a covariant. A set of dependent variables, associate with the original dependent variable, is obtained by the application of a theorem due to Clebsch. Denoting these by v_2, v_3, ..., v_{n-1}, for the untransformed equation, and by t_2, t_3, ..., t_{n-1}, for the transformed equation, we have

$$v_p = t_p z'^{-\frac{1}{2}p(n-p)},$$

so that these associate variables are covariants. The variable v_p satisfies a linear differential equation of order $\frac{n\,!}{p\,!\,\overline{n-p}\,!}$; and, in particular, v_{n-1} is the variable of Lagrange's "adjoint" equation. The following inferences relating to these variables and equations are made:—

(α) The dependent variables form a complete system, that is, functional combinations of them, similar to those by which they are obtained, are expressible in terms of members of the system;

(β) The associate linear equations in variables which have the same index are mutually adjoint;

(γ) The invariants of the associate linear equations are expressible in terms of the invariants of the original equation.

In the fifth section these dependent variables are treated in the same manner as the priminvariants in the third, and give two classes of functions—*identical covariants*, which in their canonical form involve dependent variables only, and *mixed covariants*, which involve dependent variables and coefficients of the original equation. The former class includes series of covariants, each involving only one of the dependent variables; the law of successive formation is

$$V_{p,1} = p(n-p)v_p v_p'' - (np-p^2-1)v_p'^2,$$
$$V_{p,r+1} = p(n-p)v_p V'_{p,r} - r(np-p^2-2)V_{p,r}v_p',$$

for each of the associate variables. But other functions which involve more than one of the variables, *e.g.*, the Jacobian of two of them, are omitted, for they can be algebraically compounded by means of the mixed covariants. The number of independent identical covariants in the succession is one less than the order of the equation satisfied by the variable: but a modification of this number is necessary when they are considered as covariants of a differential quantic instead of being considered covariants of a differential equation. For in this case we must either retain the quantic and all derivatives from it—when there is no modification of the number of identical covariants; or the number is unlimited, and then the quantic and its derivatives are composite.

The mixed covariants which are irreducible are proved to consist only of first Jacobians of some one of the invariants and all the dependent variables in turn.

The aggregate of the concomitants is constituted by the three classes of functions thus obtained, viz., invariants, identical covariants, and mixed covariants.

In the sixth section the results previously derived are applied to equations of the second, the third, and the fourth orders; solely, however, for the sake of illustration and not for purposes of critical discussion of classes of these equations.

For the equation of the second order the only result obtained is a reproduction of Schwarz's theorem; the equation has no invariant.

For the equation of the third order, the canonical form of which is

$$u''' + \Theta_3 u = 0,$$

and which has a single priminvariant, one or two questions are solved; in particular, the differential equation satisfied by the quotient of two solutions of the cubic is obtained, and there is thence deduced a quotient-derivative, which is the analogue of Schwarz's derivative for the quadratic.

For the equation of the fourth order there are two canonical forms, viz.:—

$$u^{\text{iv}}+4Q_3u'+Q_4u = 0,$$
$$u^{\text{iv}}+6R_2u''+R_4u = 0,$$

to which the explicitly general quartic can be reduced by the solution of linear differential equations of the second and the third order respectively. The differential equation satisfied by the quotient of two solutions of the quartic is obtained; and in this connexion there arises a quartic quotient-derivative. Finally, the associate equations of the quartic are formed; and it is verified that all their invariants are expressible in terms of the invariants of the original quartic.

The seventh section is really a digression from the main subject of the paper; it is concerned with the special class of functions which occur in the preceding section and are called quotient-derivatives. The quotient-derivatives of lowest order are

$$\begin{vmatrix} s'' , & 2s' \\ s''' , & 3s'' \end{vmatrix} = [s, z]_2;$$

$$\begin{vmatrix} s''' , & 3s'' , & 3s' \\ s^{\text{iv}} , & 4s''' , & 6s'' \\ s^{\text{v}} , & 5s^{\text{iv}} , & 10s''' \end{vmatrix} = [s, z]_3;$$

and so on; in these the differential coefficient of highest order which occurs is of odd order, and thence these derivatives are said to be *of odd order*. The two most important propositions which relate to them are, first, if

$$[\sigma, s]_m = 0, \qquad [s, z]_n = 0, \qquad [z, x]_p = 0,$$

then $$[\sigma, x]_\rho = 0,$$

where $$\rho-1 = (m-1)(n-1)(p-1);$$

and second, that the law of change for homographic transformation of both variables is

$$\left[\frac{as+b}{cs+d}, \frac{ez+f}{gz+h}\right]_n = \frac{(ad-bc)^n}{(eh-fg)^{n^2}} \frac{(gz+h)^{2n^2}}{(cs+d)^{2n}} [s, z]_n.$$

There is then investigated the series of similar functions *of even order* in the form

$$\begin{vmatrix} s' , & s \\ s'' , & 2s' \end{vmatrix}, \qquad \begin{vmatrix} s'' , & 2s' , & s \\ s''' , & 3s'' , & 3s' \\ s^{\text{iv}} , & 4s''' , & 6s'' \end{vmatrix},$$

and so on; and a connexion between the two classes is given.

Up to this point the results in the memoir which relate to the derivation of covariantive forms have been synthetically obtained; the eighth (and last) section relates to their analytical derivation. It is shown that, for a homographic transformation of the independent variable applied concurrently with the proper transformation of the dependent variable, the canonical form of the differential equation is maintained. These transformations are applied to prove, by the method of infinitesimal variation, that every concomitant ϕ in its canonical form satisfied the linear partial differential equation

$$\sum_{m-1}^{m=n-1}\left\{m(n-m)u^{(m-)}\frac{d\phi}{du^{(m)}}\right\}$$

$$+\sum_{p=2}^{p=n-1}\sum_{r=1}^{r=\sigma-1}\left[r\{p(n-p)-r+1\}v_p^{(r-1)}\frac{d\phi}{dv_p^{(r)}}\right]$$

$$=\sum_{\mu=3}^{\mu=n}\sum_{s=1}\left\{s(2\mu+s-1)\Theta_\mu^{(s-1)}\frac{d\phi}{d\Theta_\mu^{(s)}}\right\},$$

where $\sigma=\dfrac{n!}{p!\,n-p!}$. This is called the *form-equation.* Such a concomitant ϕ also satisfies the equation

$$\sum_{m=0}^{m=n-1}\left[\left\{m-\tfrac{1}{2}(n-1)\right\}u^{(m)}\frac{d\phi}{du^{(m)}}\right]$$

$$+\sum_{p=2}^{p=n-1}\sum_{r=0}^{r=\sigma-1}\left[\{r-\tfrac{1}{2}p(n-p)\}v_p^{(r)}\frac{d\phi}{dv_p^{(r)}}\right]$$

$$=\lambda\phi-\sum_{\mu=3}^{\mu=n}\sum_{s=0}\left\{(s+\mu)\Theta_\mu^{(s)}\frac{d\phi}{d\Theta_\mu^{(s)}}\right\},$$

where λ is the index of the concomitant. This is called the *index equation;* and, when the form of ϕ is known, it merely determines λ, which can be written down from an inspection of the concomitant.

These equations are applied, (i) to the identical covariants in u,—(ii) to the invariants derived from Θ_3,—for each of which simplified equivalent functions are obtained for derivatives of order higher than the third,—and (iii) to verify that the Jacobian of a priminvariant and any of its derived invariants satisfies the equations. Lastly, by means of the theory of partial differential equations, it is proved that the aggregate of concomitants obtained in the earlier part of the memoir is complete, that is, that any concomitant can be expressed as an algebraical function of the members of that aggregate.

Presents, January 12, 1888.

Observations and Reports.

Bombay:—Colába Observatory. Report, 1887. Folio. *Bombay.* The Director.

Lisbon:—Commission des Travaux Géologiques du Portugal. La Faune Crétacique du Portugal. Vol. II. Fasc. 1. 4to. *Lisbonne* 1887. The Commission.

London:—Local Government Board, 16th Annual Report, 1886–87. Supplement containing the Report of the Medical Officer for 1886. 8vo. *London* 1887. The Medical Department.

Royal Gardens, Kew. Bulletin of Miscellaneous Information, Nos. 6–7, 9–12. 8vo. *London* 1887. The Director.

Melbourne:—Department of Mines and Water Supply. Annual Report, 1886. Folio. *Melbourne* 1887. The Department.

Milan:—Reale Osservatorio di Brera. Pubblicazioni. No. VII. Parte 2. 4to. *Milano* 1885. No. XXIX. 4to. *Milano* 1887. The Observatory.

Paris:—Observatoire. Annales—Mémoires. Tome XVIII. 4to. *Paris* 1885; Observations, 1882. 4to. *Paris* 1887. The Observatory.

Prague:—K. K. Sternwarte. Magnetische und Meteorologische Beobachtungen. Jahrgang 47. 1886. 4to. *Prag* 1887. The Observatory.

Rome:—Pontificia Università Gregoriana. Bullettino Meteorologico. Vol. XXVI. Num. 1–7. 4to. *Roma* 1887. The University.

St. Petersburg:—Comité Géologique. Bulletin. Tome VI. No. 6–10. Supplément au Tome VI. 8vo. *St. Pétersbourg* 1887; Mémoires. Tome II. No. 4–5. Tome III. No. 3. Tome IV. No. 1. 4to. *St. Pétersbourg* 1887. The Comité.

Physikalisches Central-Observatorium. Annalen. Jahrgang 1886. Theil 1. 4to. *St. Pétersbourg* 1887. The Observatory.

Tiflis:—Physikalisches Observatorium. Magnetische Beobachtungen. 1884–85. 8vo. *Tiflis* 1887. The Observatory.

Turin:—Osservatorio. Bollettino. Anno XXI; 1886. Oblong 4to. *Torino* 1887; Effemeridi del Sole, della Luna e dei Principali Pianeti per l'Anno 1888. A. Charrier. 8vo. *Torino* 1887. With four Excerpts in 8vo. The Observatory.

Washington:—Bureau of Education. Circulars of Information. Nos. 1–2. 8vo. *Washington* 1887. The Bureau.

U. S. Naval Observatory. Report. 1887. 8vo. *Washington.* The Observatory.

Journals.

American Chemical Journal. Vol. IX. Nos. 1–2. 8vo. *Baltimore* 1887. The Editor.

American Journal of Philology. Vol. VIII. No. 31. 8vo. *Baltimore.* 1887. The Editor.

American Journal of Science. Vol. XXXIV. July to December, 1887. 8vo. *New Haven.* The Editors.

Analyst (The) July to December, 1887. 8vo. *London.* The Editor.

Annalen der Physik und Chemie. 1887. Nos. 7–12. 8vo. *Leipzig;* Beiblätter 1887. Nos. 6–12. 8vo. *Leipzig.* The Editor.

Astronomie (L') Juillet—Décembre, 1887. 8vo. *Paris.* The Editor.

Athenæum (The) July to December, 1887. 4to. *London.* The Editor.

Builder (The) July to December, 1887. Folio. *London.* The Editor.

Chamber of Commerce Journal. Vol. VI. Nos. 63–69. 4to. *London* 1887. The Chamber.

Chemical News (The) July to December, 1887. 4to. *London.* Mr. W. Crookes, F.R.S.

Cosmos. Juillet—Décembre, 1887. 8vo. *Paris.* M. l'Abbé Valette.

Educational Times (The) July to December, 1887. 4to. *London.* The College of Preceptors.

Electrical Review (The) July to December, 1887. Folio. *London.* The Editor.

Industries. July to December, 1887. Folio. *London.* The Editor.

Meteorologische Zeitschrift. Juli—Dezember, 1887. Sm. folio. *Berlin.* Oesterreichische Gesellschaft für Meteorologie.

Morskoi Sbornik, 1887. Nos. 1–12. 8vo. *St. Petersburg.* The Compass Observatory, Cronstadt.

Nature. July to December, 1887. Roy. 8vo. *London.* The Editor.

New York Medical Journal. July to December, 1887. 4to. *New York.* The Editor.

Notes and Queries. July to December, 1887. 4to. *London.* The Editor.

Observatory (The) July to December, 1887. 8vo. *London.* The Editors.

Practical Engineer. March to July, 1887. 4to. *London.* The Editor.

Repertorium für Meteorologie. Band X. 4to. *St. Pétersbourg* 1887. The Academy of Sciences, St. Petersburg.

Journals (*continued*).

Revue Internationale de l'Électricité. Juillet—Décembre. 8vo. *Paris.* The Editor.

Symons's Monthly Meteorological Magazine. July to December, 1887. 8vo. *London.* Mr. Symons, F.R.S.

Zeitschrift für Biologie. Band XXIV. Hefte 1–2. 8vo. *München* 1887. The Editors.

Barboza Rodrigues (J.) O Tamakoaré, Especies novas da Ordem das Ternstroemiaceas. Folio. *Manáos* 1887. The Author.

Ebert (H.) Ueber die Abhängigkeit der Wellenlänge des Lichtes von seiner Intensität. 8vo. *Leipzig* 1887. The Author.

Gegenbaur (C.), For. Mem. R.S. Zur Kenntniss der Mammarorgane der Monotremen. 4to. *Leipzig* 1886; Ueber die Occipitalregion der Fische. 4to. *Leipzig* 1887. The Author.

Jervis (W.) Delle Cause dei Movimenti Tellurici. 8vo. *Torino* 1887. The Author.

Le Chatelier (H.) Recherches Expérimentales sur la Constitution des Mortiers Hydrauliques. 8vo. *Paris* 1887. The Author.

Lindelöf (L.) Trajectoire d'un Corps sur la Surface Terrestre. 4to. *Helsingfors* 1887. The Author.

Mueller (Baron von), F.R.S. Iconography of Australian Species of Acacia. Decades 1–4. 4to. *Melbourne* 1887. The Government of Victoria.

Schübeler (F. C.) Norges Vaextrige, et Bidrag till Nord-Europas Natur- og Culturhistorie. Bind I. Hefte 2. Bind II. Hefte 1. 4to. *Christiania* 1886. Dr. Schübeler.

Thompson (E.E.) List of the Mammals of Manitoba. 8vo. *Toronto* 1886. The Author.

Whitehouse (C.) The Raïan Mœris; or Storage Reservoir of Middle Egypt. 8vo. *London* 1887; The Caves of Staffa. 8vo. 1887. The Author.

A copy, in silver, of the Jubilee Medal of the Numismatic Society. Mr. John Evans, Treas. R.S.

January 19, 1888.

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

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

The following Papers were read:—

I. "Notes on the Spectrum of the Aurora." By J. NORMAN LOCKYER, F.R.S. Received January 9, 1888.

I exhibited to the Society on November 17, 1887, a tabular statement showing the bright lines seen in the spectra of various celestial bodies, and I also gave those recorded in the spectrum of the Aurora showing many remarkable coincidences.

I now find that the connexion is closest between the auroral spectrum and that of stars 3*a*, and in anticipation of a subsequent communication of details I send on the accompanying table, showing the origin of Dunér's bands, so far as I have at present made them out, and their connexion with the spectrum in question.

The individual observations which I have used in the table are those collected by Mr. Capron and Mr. Backhouse ('Nature,' vol. 7, pages 182, 463).

Table of Wave-lengths of Auroral Lines.

Barker	431	470	482	502	517				562	623
Smyth									558	635
Zöllner										628
A. Clerke			485				532			
Herschel							531			
Backhouse	430			501	516·5		532			606
Lord Crawford			more ref. than F			523				
Proctor (R. H.)			"							
Vogel		469								
Ellery										635
O. Struve									554	
Ångström		472				521			556	
Lemström	426	469				525				
German N. P. Ex.									557	
Respighi									557	
Peirce	431	464	486			520	531	545	557	
Probable origin	CH	C hot	C cold	Mg	C hot	Mg	*	Zn† (1)‡	Mn (1)	Fe (1)
Wave-lengths of probable origin	431	474	483	500	516·5	520·1		546	558	615
Dunér's bands		460–474 (10)	477–485 (9)	495–503 (8)	516–521 (7)			550–545 (5)	564–559 (4)	616–627 (2)

* Coronal line. † Another próbable origin for this in the aurora is 540 Mn. ‡ This means brightest fluting.

Addendum.—Received January 19, 1888.

The following table shows the above figures in another form and includes the bright lines recorded in γ-Cassiopeiæ :—

Aurora.	Dunér's bands.	Bright lines in γ-Cassiopeiæ.	Probable origin.	Wave-length of probable origin.
431	..	..	CH	431
474	460—474 (10)	..	C (hot)	474
..	..	462·3	Sr	460·7
483	477—485 (9)	..	C (cool)	483
500	495—503 (8)	499	Mg	500
516·5	516—521 } (7)	516·7	C (hot)	516·5
520·1	.. } (7)	..	Mg	520·1
531	..	531	Coronal line	
..	..	542·2	Mn	540
545	545—550 (5)	..	Zn (1)	546
558	559—564 (4)	555·7	Mn (1)	558
..	585—595 (3)	586	Mn (2)	586
615	616—627 (2)	616	Fe (1)	615
635	..	635·6	*	..

II. "On the Secondary Carpals, Metacarpals, and Digital Rays in the Wings of existing Carinate Birds." By W. K. Parker, F.R.S. Received January 11, 1888.

In a paper "On the Morphology of Birds," already sent in to the Royal Society, but not yet published, I have described certain additional parts in the wings of Gallinaceous birds.

One of these lies on the radial side of the first metacarpal; the other two are on the ulnar side of the second and third metacarpals.

These parts, which at first caused me considerable surprise, being wholly unexpected by me, are only part of what I have since found in other families.

During the past year I have worked out the development of the skeleton in the Duck tribe ("Anatidæ"), in the Auk tribe ("Alcidæ"), and in the Gull tribe ("Laridæ"), and to some degree in some other families. The subject appears to me to be of great interest, and I have, through various English and American friends, obtained many scores of embryos and young birds, &c., that I may be able to trace

* This line is seen as a pretty bright line in the spectrum of the Limerick meteorite, but its origin has not yet been determined, although comparisons have been made with most of the common elements. So far, it has not been observed in any other meteorite.

these parts in every main group of the Class. Normally, both the existing Carinatæ and Ratitæ, and such extinct forms as have been worked out—*Archæopteryx*, *Hesperornis*, *Ichthyornis*—show that the primary form of the bird's wing is simply *tri-digitate.* In this I agree with Baur, who has helped me greatly in this matter, both by his valuable papers and also by personal discussion with me.

The normal "manus" of a carinate bird contains two permanently distinct carpals: three carpals that lose their independence by ankylosis with the metacarpals, and three digital rays extending from the three fused metacarpals.

In some birds, *e.g.*, the Passerinæ, the *pollex* of the first digit has only one phalanx attached to its short metacarpal, the second only two, and the third only one, phalanx. In others, Plovers, Gulls, Cormorants, &c., an additional or *ungual* phalanx is found on the first and second digit; and in some birds, *e.g.*, *Numenius*, during their embryonic state, a small nucleus arm is seen on the end of the aborted phalanx of the third digit.

In my as yet unpublished paper I have mentioned a sub-distinct tract of very solid fibro-cartilage, which evidently corresponds with what has been called "præ-pollex" by Kehrer and others.*

I am satisfied, now, that this very notable part is the remnant of the skeleton of the *spur*, so remarkably developed in the Palamedidæ, certain Geese, Plovers, and Jacanas.

This part therefore need not interfere with the consideration of the *true secondary digital* parts.

Among the last communications received by me from Dr. Baur, I find in print what I had already learned from him orally.

In some "General Notes" published in the 'American Naturalist,' September, 1887, p. 839, I find the following paragraph: "The oldest Ichthyopterygia had few phalanges and not more than five digits; [the] radius and ulna were longer than broad, and separated by a space. Later, through the adaptation to the water, more phalanges were developed, more digits appeared, mostly by division of the former, or by new formation on the ulnar side. I have never found a new digit developed on the radial side."

These are most important facts, some of which, namely, the bifurcation of the digital rays, I had received some light upon, before, both from Dr. Gadow and from Professor D'Arcy W. Thompson.†

I find that the *carpus*, *metacarpus*, and digital rays are all apt to increase in number beyond what is normal.

* "Beiträge zur Kentniss des Carpus und Tarsus der Amphibien, Reptilien, und Säuger," 'Berichte der Naturforschenden Gesellschaft zu Freiburg i. B.,' vol. 1, 1886 (Heft 4 and Taf. 4).

† See his paper on the hind limb of Ichthyosaurus, &c., 'Journ. Anat. Physiol.,' vol. 20, 1886, pp. 532–535.

Long ago I found, in one of the Palamedidæ, *Chauna chavaria*, *two ulnar carpals*, apparently an "ulnare" proper, and "centrale." More recently in the embryo of a more normal Chenomorph—the Falkland Island Goose (*Chloëphaga policephala*) I found the ulnare nearly divided into two segments.

On the other side of the carpus in an embryo Kestrel (*Falco tinnunculus*) and in a young Sparrow-hawk (*Accipiter nisus*), I found a "radiale" in two pieces, the outer of which in the latter was degenerating into the large "os prominens" which is found in the tendon of the "tensor patagii" muscle of rapacious birds.

In the embryos of Gulls, Auks, Guillemots, &c., the large "distal carpal" of the index or second digit sends forward a long wedge of cartilage towards an additional metacarpal nucleus. Evidently this is the rudiment of another carpal seeking to be attached to its own intercalary metacarpal.

Further on, on the large second digit, the flat dilated part of the proximal phalanx, on its ulnar side, also, is developed from a distinct tract of true cartilage, but soon loses its independence; it forms the plate on which some of the *primary quills* are fixed.

Further on, on the ulnar side, near the small well-developed ungual phalanx of the embryo, but later, after hatching, a small oval cartilage appears, and *is ossified independently*.

A similar tract of cartilage is formed on the pollex or first digit, also, but is somewhat smaller than that on the second; it is on the ulnar side and near the ungual phalanx.

In the feeble third digit I only find a rudimentary secondary metacarpal, on the ulnar side; this is very constant throughout the *Carinatæ;* and sometimes, as I have already mentioned, there is a small rudiment of a second phalanx on that digit which, in the Lizard, has *four* phalanges.*

In seeking for evidence of the manner in which these high and noble hot-blooded feathered forms arose from among the Archaic Reptilia, I think that something has been gained in what I have stated above.

The skull brings evidence of the same sort, during its development, and it is to ancient long-beaked forms, and not to modern short-faced types of Reptilia, that we must look for any near relationship of the Reptiles in the Birds.

In the Guillemot (*Uria troile*) I have satisfied myself that there has been a considerable amount of *secular* shortening of the beak (rostrum and fore part of mandibles), and if we look at Dr. Marsh's figures of *Hesperornis* and *Ichthyornis* we shall see what long bills these toothed birds possessed.

* The figures of these parts, and also of the rest of the developing skeleton in these birds—Ducks, Auks, Guillemots, &c.—are ready for publication.

But there is no part of a developing bird's skeleton that is not rich with suggestive facts of this kind, as I propose to show in due time.

Presents, January 19, 1888.

Transactions.

Brussels:—Académie Royale de Médecine. Bulletin. Sér. 4. Tome I. Nos. 5–10. 8vo. *Bruxelles* 1887. The Academy.

Académie Royale des Sciences. Bulletin. Sér 3. Tomes XIII–XIV. Nos. 5–11. 8vo. *Bruxelles* 1887. The Academy.

Hobart:—Royal Society of Tasmania. Abstracts of Proceedings. April 19, and May 10, 1887. 8vo. [*Hobart*]. The Society.

London:—Chemical Society. Abstracts of the Proceedings. Nos. 41–45. 8vo. *London* 1887; Journal. July to December, 1887. 8vo. *London.* The Society.

Geological Society. Abstracts of the Proceedings. Nos. 508–513. 8vo. *London* 1887. The Society.

Institution of Civil Engineers. Abstracts of the Proceedings. Nos. 1–3. 8vo. [*London*] 1887. The Institution.

Linnean Society. Journal (Botany). Vol. XXIII. Nos. 152–154. Vol. XXIV. Nos. 159–161. 8vo. *London* 1887; Ditto (Zoology). Vol. XX. Nos. 117–118. Vol. XXI. No. 129. Vol. XXII. No. 136. 8vo. *London* 1887. The Society.

Pharmaceutical Society of Great Britain. Journal and Transactions. July to December, 1887. 8vo. *London.* The Society.

Royal Astronomical Society. Monthly Notices. July to December, 1887. 8vo. *London.* The Society.

Royal Geographical Society. Proceedings. July to December, 1887. 8vo. *London.* The Society.

Royal Institution of Great Britain. Reports of the Weekly Meetings. 1887. 8vo. The Institution.

Society of Arts. Journal. July to December, 1887. 8vo. *London.* The Society.

Society of Chemical Industry. Journal. Vol. VI. Nos. 5–11. Sm. folio. *London* 1887. The Society.

Paris:—Académie des Sciences. Comptes Rendus. Juillet—Décembre, 1887. 4to. *Paris.* The Academy.

Société de Biologie. Comptes Rendus. Juillet—Décembre, 1887. 8vo. *Paris.* The Society.

Société d'Encouragement pour l'Industrie Nationale. Bulletin. Juillet—Décembre, 1887. 4to. *Paris;* Compte Rendu des Séances. Juin—Décembre, 1887; Annuaire pour l'Année 1887. 12mo. *Paris.* The Society.

Transactions (*continued*).

Société de Géographie. Compte Rendu. 1887. Nos. 12–16. 8vo. *Paris.* The Society.

Société Française de Physique. Résumé des Communications. Juillet—Décembre, 1887. 8vo. *Paris.* The Society.

Philadelphia:—Academy of Natural Sciences. Proceedings. 1887. 8vo. [*Philadelphia.*] The Academy.

Franklin Institute. Journal. July to December, 1887. 8vo. *Philadelphia.* The Institute.

Rome:—Accademia Pontificia de' Nuovi Lincei. Processi-Verbali. Sessione 7–8. 12mo. [*Roma*] 1887. The Academy.

Reale Accademia dei Lincei. Atti. Ser. 4. Vol. III. Fasc. 9–13 (Semestre 1). Fasc. 1–5 (Semestre 2). 8vo. *Roma* 1887. The Academy.

Sydney:—Linnean Society of N.S.W. Abstracts of Proceedings. April to September, 1887. 8vo. *Sydney.* The Society.

Corfield (W. H.) The Treatment and Utilisation of Sewage. Third Edition, revised and enlarged by the author, and L. C. Parkes, M.D. 8vo. *London* 1887. The Authors.

Dawson (Sir J. W.), F.R.S. Note on Fossil Woods and other Plant Remains, from the Cretaceous and Laramie Formations of the Western Territories of Canada. 4to. [*Montreal*] 1887. The Author.

Hooker (Sir J. D.), F.R.S. The Flora of British India. Part XIV. 8vo. *London* 1887. The Author.

Liversidge (A.), F.R.S. On the Composition of some Pumice and Lava from the Pacific. 8vo. *Sydney* 1886; with three other Excerpts. The Author.

Marriott (W.) Results of Thermometrical Observations at Boston, Lincolnshire, 1882–86. 8vo. [*London*] 1887. The Author.

Nobre (A.) Sur la Faune Malacologique Marine des Possessions Portugaises de l'Afrique Occidentale. 8vo. [*Lisbonne*] 1887. The Author.

Roscoe (Sir H. E.), F.R.S., and C. Schorlemmer, F.R.S. A Treatise on Chemistry. Vol. III. Part 4. 8vo. *London* 1888. The Authors.

Ursini-Scuderi (A. S.) Il Fattore Personale della Specie Umana. Vols. I-II. 8vo. *Catania* 1887. The Author.

January 26, 1888.

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

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

The following Papers were read :—

I. "The Emigration of Amœboid Corpuscles in the Starfish." By HERBERT E. DURHAM, B.A., lately Vintner Exhibitioner, King's College, Cambridge. Communicated by P. HERBERT CARPENTER, D.Sc., F.R.S., F.L.S. Received January 5, 1888.

[PLATE 3.]

Through the kindness of Professor M'Intosh, to whom my very best thanks are due, I spent some time last summer at the Marine Laboratory at Saint Andrews.

The results given below arose from what were intended to be control experiments in some observations which aimed at determining, if possible, what organ or system of organs is definitely excretory in function in the Echinoderms.

The common starfish (*Asterias rubens*) was the form used, being convenient both from its size and from its abundance.

Indian ink or a precipitated aniline blue was injected into the cœlomic cavity by means of a fine pipette or a hypodermic syringe. It was found best to insert the instrument into an arm close to the disk, for then the arm is far more rarely thrown off than if the puncture is made near its distal end. The specimen was next held in different positions so as to distribute the injected fluid.

The granules that are introduced are ingested by the amœboid corpuscles which float in the cœlomic fluid, as can readily be demonstrated by microscopic examination of that fluid. The granule-laden phagocytes can be seen very plainly, owing to the particles they have ingested, in the dermal branchiæ of a living specimen. The cilia of the cœlomic epithelium cause them to dance up and down in the branchia, and to be thrown against its wall. Every now and again a corpuscle will adhere, and by repetition of this process a small clump may be formed; this occurs at or near the apex of the branchia.

The corpuscles after their adhesion to the wall of the branchia creep by their amœboid movement through the cœlomic epithelium, the connective tissue layer, and the epidermis to the exterior (fig. 1). Thus a clump may be formed on the outer side of the branchia, and the animal is freed from some of the irritating particles.

In these clumps the corpuscles retain their individuality (fig. 2), they do not fuse to form plasmodia such as Geddes* describes in the so-called clotting of the perivisceral fluid of urchins; indeed if such a coalescence did take place the facility for their migration through tissues would be considerably diminished.

In cases where the emigration is proceeding exceedingly actively, besides the isolated phagocytes that are seen at different depths in the branchial wall on their outward journey, the apices of some of the branchiæ appear to be perforated by an aperture, which is entirely filled up by a plug of phagocytes (fig. 3, *p*). It is clear that such a result might be due either to a stretching of the wall by a simultaneous entrance of several phagocytes at a certain point and subsequent intrusions of others between them, or to an actual rupture or carrying away of part of the wall by the energy and magnitude of the emigration round one patch. So far as can be made out from serial sections the former of these alternatives holds good; there seems, however, to be no reason why the latter might not also take place.

Since in preparing Echinoderms for sections it is usual to distend them with the fixing fluid, I should mention that here such treatment has been avoided. The specimens were anæsthetisized with chloral hydrate, and the gills could then be removed in a distended state, while moreover they remained distended after removal.

To return to the subject: after their arrival at the exterior the corpuscles retain their irregular amœboid shape for a time. They then become spherical and swell up and later they disintegrate, the granules they contained being scattered free.

It was found that besides the corpuscles containing Indian ink particles in the extruded material, there occurred amœboid cells loaded with refringent granules (fig. 2, *b*); moreover it is not only in the injected specimens that such corpuscles emigrate; for if a starfish is kept in a vessel (into which fresh sea-water is constantly dripping) it throws off from its surface a certain amount of a dirty brownish slime. This slime contains large corpuscles with refringent granules (fig. 4) which are apparently identical with those mentioned above, and with those peculiar cells which occur here and there in different parts of the animal, especially perhaps in the so-called "heart;" they are called "Plasma-Wanderzellen" by the Germans: I propose to refer to them as "sphæruliferous" corpuscles.

* 'Archives de Zoologie Expérimentale,' vol. 8, p. 483.

In the slime these sphæruliferous corpuscles are seen in various stages of disintegration, held together by a material of slimy consistency which is, at any rate in part, derived from the swollen-up stromata of the corpuscles, some doubtless having origin in the scattered mucous gland cells of the epidermis. Besides these elements a holotrichous infusorian occurred, frequently in considerable numbers, swimming about and feeding on the freely scattered granules. In connexion with this I might also note that on a large percentage of the specimens of *Asterias rubens* observed at Saint Andrews there crawled a species of *Caprella*. These Caprellæ feed on the above-mentioned slime; and those which lived on specimens treated with aniline blue presented particularly gay alimentary canals.

As regards the emigration of these sphæruliferous cells, it is interesting to find that Hamann* has recently described and figured the presence of such corpuscles in the wall of the ambulacral gills of Echinids; these are doubtless on their outward journey. I might also note here that when the dermal branchiæ of *Asterina gibbosa* are slightly, not rigidly, distended, they move round and round, more or less circularly, so that their apices rub against the neighbouring ossicles. This movement might be interpreted as the expression of attempts to remove emigrated corpuscles from their surface; the branchiæ when removed showed sphæruliferous cells in their wall.

I hope to make further observations to help to elucidate the meaning of this out-wandering of sphæruliferous cells, about which at present it is impossible to draw up any definite conclusions. I desire now merely to note its occurrence.

It seems evident, however, that the starfish has the power of removing minute foreign particles introduced into its system; and it is conceivable that in nature such particles might gain admittance to the cœlomic cavity when an arm is thrown off.

It does not seem clear what becomes of insoluble foreign granules when they are introduced into other animals, except in the case of mammals; at any rate I have been unable to find any account of an actual transportation to the exterior such as has been described above.

Over and above any respiratory function that the dermal branchiæ may have, they form from their structure convenient places for the out-passage of scavenging amœboid cells. Hamann† notes that their nerve supply is very scanty; the well-being of a fine nerve plexus would obviously not be added to by amœboid cells traversing it.

To summarise in a few words—minute foreign bodies introduced into the body-cavity of the starfish are removed to the exterior by

* 'Jenaische Zeitschrift,' vol. 21, p. 159, and Taf. VI, fig. 12.

† 'Die Asteriden,' Jena, 1885 (p. 11).

phagocytes which pass out through the dermal branchiæ. In conclusion, I should state that clumps of corpuscles occur, here and there, in the pore canals of the madreporite both of *Asterias rubens* and *Cribrella ocellata* as seen in sections. The madreporites and neighbouring structures were removed from full-grown specimens and then placed in hardening fluids: this being so, I think it not improbable that these corpuscles came from the cut end of the "heart," and arrived at their position by the outward ciliary current, recently described by Dr. Hartog.* It is difficult to conceive that such an outflow of corpuscles should take place normally; for then there must be a continual loss of *ordinary* as well as of *sphæruliferous corpuscles.*

EXPLANATION OF PLATE 3.

FIG. 1.—Section through a dermal branchia of *Asterias rubens,* after Indian ink injection. *c. e.,* cœlomic epithelium; *c. t.,* connective tissue; *e,* epidermis; *cut.,* cuticle.

FIG. 2.—Corpuscles containing granules of Indian ink, taken off a branchia. *b,* sphæruliferous corpuscle.

FIG. 3.—Section through terminal portion of dermal branchia. Note the plug of corpuscles (*p*) and crowding of epiderm nuclei at its sides. The other letters as in fig. 1.

FIG. 4.—Sphæruliferous cells from slime. *l,* liberated sphærules.

II. "Note on the Madreporite of *Cribrella ocellata.*" By HERBERT E. DURHAM, B.A., lately Vintner Exhibitioner, King's College, Cambridge. Communicated by P. HERBERT CARPENTER, D.Sc., F.R.S., F.L.S. Received January 5, 1888.

I have a series of vertical longitudinal (radial) sections carried through the madreporite, &c., of a full-grown specimen of *Cribrella ocellata:* in this series the madreporic canals have a peculiar relation to the stone canal or water-tube.

Most of the pore canals pass into collecting canals which open into the stone canal directly: some few, however, lead into the space below the madreporite, which is the upper extremity of the "schlauchförmiger Kanal." The stone canal dilates laterally on each side into an "*ampulla,*" and one of these lateral lobes of the ampulla has an aperture into the "schlauchförmiger Kanal." Now the "schlauchförmiger Kanal" is derived from the enterocœle (Hamann),† so that in the specimen described there is a permanent connexion between the hydrocœle cavity and the enterocœle cavity.

* M. M. Hartog, 'Ann. Mag. Nat. Hist.,' Nov. 1887.

† O. Hamann, 'Die Asteriden,' p. 51, Jena, 1885.

Fig 1 (x 540.)

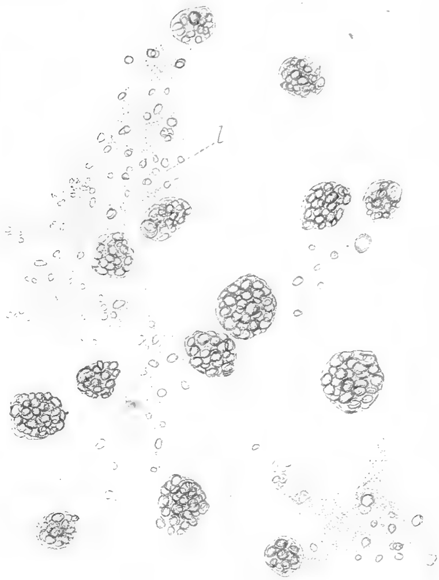


Fig 4 (x 690.)

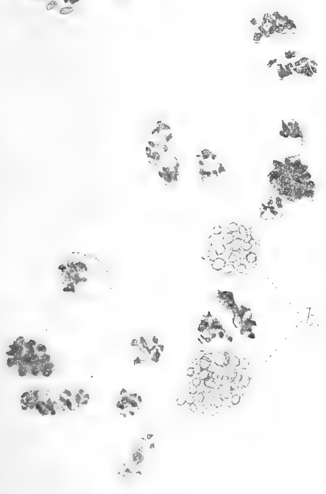


Fig 3 (x 190.)

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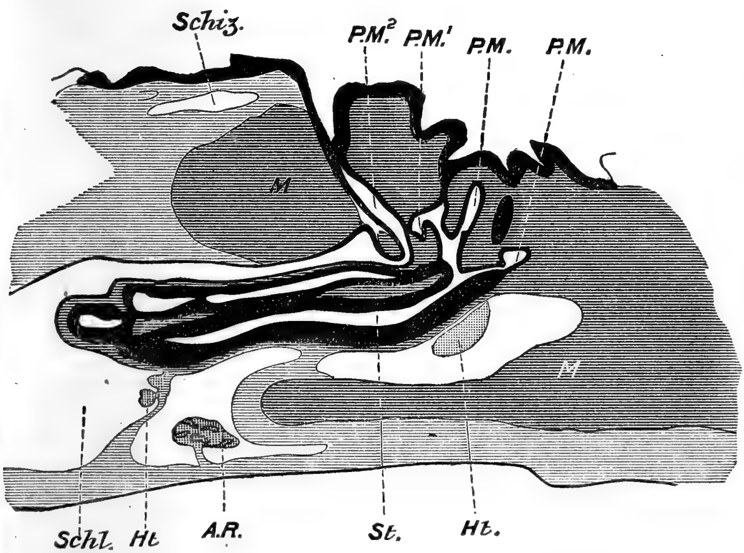
Ludwig* states that he was unable to find any such connexion (a connexion which would explain the injection results obtained by many observers) in the forms investigated by him, and I can confirm his statement for *Asterias rubens.* Neither in *A. rubens* nor in *Cribrella ocellata* have I detected any connexion between the water vascular and "blood vascular" systems in this region of the body.

Section 1 (fig. 5) passes along the upper extremity of the stone canal between the dilatations, and through one of the abnormal pore canals (PM²); by examination of neighbouring sections it is seen that the lumen of the pore canal is continuous from the exterior to the "schlauchförmiger Kanal" (Schl) and also that there is a communication between the canals (PM¹) and (PM²). Two other canals (PM) are seen opening into the stone canal (St).

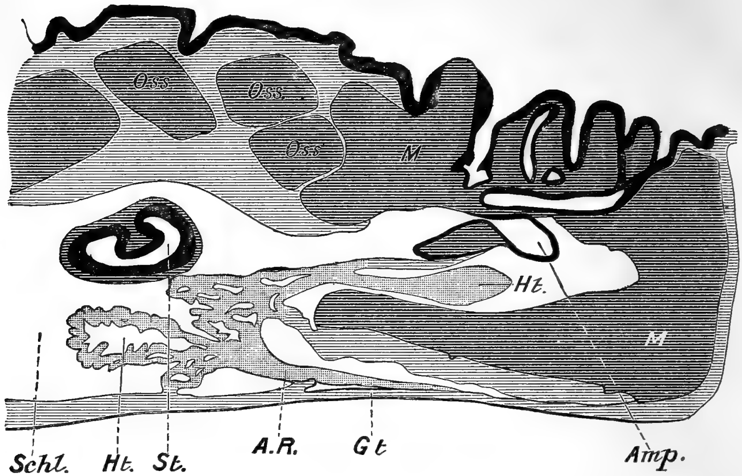
Section 2 (fig. 6) passes through the aperture of communication between the *ampulla* and the "schlauchförmiger Kanal." In this section the continuity of the "*heart*" or dorsal organ (Ht) and anal "blood" ring (AR) is seen; also a gut vessel (Gt) from the latter.

FIG. 5.



* H. Ludwig, 'Zeitschr. Wiss. Zool.,' vol. 30, 1878, pp. 103, 104.

Fig. 6.



I am not in a position to state that such is the usual arrangement in *Cribrella:* but that such a connexion should exist even as an abnormality is not without interest. Mere closure of the internal aperture of the ampulla would not lead to the common asterid arrangement, because of the canals (PM^2 and PM^1) and others with similar relations, which are some distance from the aperture of the ampulla.

EXPLANATION OF LETTERS.

Amp.	Ampulla.
AR.	Anal "blood vascular" ring.
Gt.	Tract of "blood-vessels" to gut.
Ht.	"Heart" or dorsal organ.
M.	Madreporic ossicle.
Oss.	Ossicles in body wall.
PM, PM^1, PM^2	Pore canals of madreporite.
Schl.	"Schlauchförmiger Kanal."
St.	Stone canal.

SHADING.

Light.	Connective tissue and muscle.
Dark.	Ossicles.
Black.	Epithelium of stone canal, &c.
Cross.	"Blood vascular" tracts.

III. "Report on Hygrometric Methods. First Part, including the Saturation Method and the Chemical Method, and Dew-point Instruments." By W. N. SHAW, M.A. Communicated by R. H. SCOTT, F.R.S., Secretary to the Meteorological Council. Received January 17, 1888.

(Abstract.)

With the exception of certain "absolute hygrometers," the behaviour of which has not yet been sufficiently tested, the determination of the pressure of water-vapour in the air is indirect and requires a formula of reduction. The formulæ in use are based upon assumptions which are at present not so completely verified by experiment that any hygrometric method can be relied upon to give measures of the pressure of aqueous vapour trustworthy to within 0·1 mm. of mercury. The authority for these statements is given in detail in an account of the hygrometric work done since 1830. This account is appended to the report as Note A.

In the report, the chemical hygrometric method is provisionally regarded as a standard. The formula of reduction applicable in this case is

$$e = \frac{760(1+\alpha t)}{\Delta d} f,$$

where e is the pressure of aqueous vapour in millimetres; f, the number of grammes of moisture per cubic metre in the air at temperature t° C.; α the coefficient of expansion of air per degree C.; Δ the density of dry air at 0° and 760 mm., *i.e.*, 1293 grammes per cubic metre; and d is the specific gravity of the moisture referred to air at the same temperature and pressure.

The assumptions upon which the formula is based are—(1) That it is possible to absorb the whole of the moisture from air by passing it over desiccating substances; and (2) that a numerical value can be assigned to d. The first assumption has been discussed by Regnault and others, and is sufficiently nearly accurate for all hygrometric calculations. With regard to the second, Regnault's direct observations upon steam (free from air) and other evidences point to the value 0·622. The assumption can, moreover, be tested, by applying the chemical method to air saturated at a known temperature, assuming the value 0·622 for d, and comparing the results with the table of saturation pressures *in vacuo*. This, however, *assumes Dalton's law to be strictly accurate*, an open question, upon which opinion is reserved until further experimental investigation is concluded. Regnault made the comparison in sixty-eight experiments, in fifty-nine of which the air was practically saturated when it entered the drying tubes. For these he found that

the value 0·622 gave results which were less than the tabulated pressures, the errors being always of the same sign, but so small in amount that he neglected them in his subsequent work.

The ultimate object of the experiments described in the report was to examine the behaviour of dew-point instruments in air of known state, and for this purpose air was saturated at a known temperature and drawn by an aspirator through vessels in which the dew-point instrument could be placed when required, and subsequently through drying tubes of special pattern. The vapour-pressure was thus obtained at the two extremities of the train of apparatus and the results compared.

The following questions are raised and discussed :—

i. Were the drying tubes used as efficient as Regnault's?

ii. Does the pressure of vapour in the air become changed by passing through the apparatus designed to contain the dew-point instruments, or by the mere presence of those instruments themselves?

iii. Do the results of the chemical method agree with the tabulated vapour-pressures *in vacuo* when the air is more or less heated after being saturated?

iv. Can the observed differences between the results be obviated by assuming a value for *d* (other than 0·622), which is compatible with values obtained by other methods?

v. Can any reason be assigned for the differences observed by Regnault in the case of saturated air?

(i.) The answer to the first question is given in an account of a series of twelve experiments practically repeating Regnault's observations with saturated air. The tabulated results show divergences in the same direction and of the same order of magnitude as those in Regnault's paper. Some incidental points are also discussed namely, the comparative efficiency of phosphoric anhydride, sulphuric acid, and calcium chloride, and the effect of india-rubber and glass connexions between drying tubes. It is shown that the sulphuric acid and phosphoric anhydride tubes are efficient, that as a rule one tube is all that is strictly necessary, but that two should be used to provide for the case of exhaustion of the first tube or too rapid flow of air, and further, that the glass and mercury connexions between the tubes employed in the second series of experiments cannot be regarded as producing any effect.

(ii and iii.) The answers to the second and third questions are furnished by the results of eighty-two experiments with the chemical method upon air saturated at known temperatures by a specially designed "saturater" in a water bath. The temperatures of saturation lay between 1° C. and 21° C., and, with one exception, were below the temperature of the surrounding air. Each experiment involved upwards of thirty readings of weight, pressure, and temperature. The temperature readings were corrected by means of a special comparison at Kew. Of the eighty-two observations thirty-two are retained as being free from any known disturbing causes, and from them it appears that, with d equal to 0·622, the pressure deduced by the chemical method is on the average greater by 0·03 mm. than that given in Regnault's table of vacuum pressures, as recalculated in Landolt and Börnstein's tables. This difference is very small compared with the discrepancies from Dalton's Law observed by Regnault in the case of water vapour.

(iv.) With regard to the fourth question; if the observations be employed to determine the value which must be substituted for d, the specific gravity of saturated steam referred to air at the same temperature and pressure, the mean value of d so obtained is 0·6245, which agrees very closely with 0·6240, the mean value for the same range of temperature deduced from Clausius's calculations based on thermodynamical reasoning. The value 0·622 is probably correct if the air is not nearly saturated; in that case the measure of the pressure of vapour in the air is 2/622 greater than it would be if the same air were reduced in temperature (at constant pressure), until it was saturated.

(v.) The one observation of the second series with saturated air gives a result 0·18 mm. smaller than the tabulated pressure, and thus with the twelve experiments of the first series confirms the results of Regnault's observations. To account for this, it is suggested that air which is very nearly or quite saturated, would deposit some of its moisture on the glass tubes used to conduct it from one vessel to another. This behaviour of nearly saturated air has been already noticed, and it is confirmed by the observations on dew-point instruments, and moreover, by experiments directly intended for the purpose, quoted in a note.

Details are given of observations with Regnault's hygrometer and Dines's hygrometer when exposed in glass vessels between the saturater and the drying tube. The two instruments are separately discussed. With Regnault's instrument, after some practice, two different observers obtained practically identical results. In ordinary observations, the observed temperatures of the dew-point were below the temperature of saturation, but seldom by more than 0·1° C. A considerable amount of uncertainty was shown to be attached to the readings, and by very close inspection readings of the dew-point were obtained above the temperature of saturation, in one case by as much as 0·7° C.

From the experiments with Dines's hygrometer, it appears that the instrument is likely to give very easy determinations of the dew-point that are within small limits of error; but that if the instrument be observed with the closest attention, the result will be considerably too high in consequence of the formation of a dew deposit at a temperature above the true dew-point, and it may possibly be erroneous in consequence of variations in temperature of the different parts of the box containing the thermometer.

An account is given of Alluard's modification of Regnault's hygrometer, and of Bogen's hygrometer.

A second note, B, is appended to the report, showing the tables used in various countries for the reduction of wet and dry bulb observations.

Presents, January 26, 1888.

Transactions.

Albany:—Albany Institute. Transactions. Vol. XI. 8vo. *Albany* 1887. The Institute.

Baltimore:—Johns Hopkins University. Circulars. Vol. VII. Nos. 60–61. 4to. *Baltimore* 1887; Studies in Historical and Political Science. Fifth Series. Nos. 10–11. 8vo. *Baltimore* 1887. The University.

Basel:—Naturforschende Gesellschaft. Verhandlungen. Theil VIII. Heft 2. 8vo. *Basel* 1887. The Society.

Batavia:—Bataviaasch Genootschap van Kunsten en Wetenschappen. Notulen. Deel XXV. Aflev. 3. 8vo. *Batavia* 1887; Nederlandsch-Indisch Plakaatboek, 1602–1811. Deel IV. 8vo. *Batavia* 1887. The Society.

Koninklijke Natuurkundige Vereeniging. Natuurkundig Tijdschrift. Deel XLVI. 8vo. *Batavia* 1887. The Association.

Bergen:—Museum. Aarsberetning, 1886. 8vo. *Bergen* 1887. The Museum.

Birmingham:—Philosophical Society. Proceedings. Vol. V. Part 2. 8vo. *Birmingham* [1887]. The Society.

Brussels:—Académie Royale de Belgique. Annuaire. 1888. 12mo. *Bruxelles.* The Academy.

Halle:—Verein für Erdkunde. Mittheilungen. 1887. 8vo. *Halle.* The Verein.

Helsingfors:—Finska Vetenskaps-Societet. Bidrag till Kännedom af Finlands Natur och Folk. Häftet 44. 8vo. *Helsingfors* 1887. The Society.

Innsbruck:—Ferdinandeum für Tirol und Vorarlberg. Zeitschrift. Folge III. Heft. 31. 8vo. *Innsbruck* 1887. The Ferdinandeum.

Naturwissenschaftlich-Medizinischer Verein. Berichte. Jahrgang XVI. 8vo. *Innsbruck* 1887. The Verein.

Jena:—Medicinisch-Naturwissenschaftliche Gesellschaft. Jenaische Zeitschrift für Naturwissenschaft. Band XXI. 8vo. *Jena* 1887. The Society.

Transactions (*continued*).

Leeds:—Philosophical and Literary Society. Annual Report, 1886–7. 8vo. *Leeds* 1887. The Society.

Leipzig:—Verein für Erdkunde. Mittheilungen. 1886. Hefte 1–3. 8vo. *Leipzig* 1887. The Verein.

Liverpool:—Astronomical Society. Journal. Vol. VI. Parts 2–3. Catalogue of the Library. 8vo. *Liverpool* 1887–8. The Society.

London:—Geological Society. Quarterly Journal. Vol. XLIII. No. 172. 8vo. *London* 1887; List of Fellows, 1887. 8vo. *London.* The Society.

Institution of Mechanical Engineers. Library Catalogue, 1887 8vo. *London.* The Institution.

Iron and Steel Institute. Journal. 1887. 8vo. *London.* The Institute.

Mathematical Society. Proceedings. Vol. XVIII. Nos. 301–304. 8vo. *London* [1887]; List of Members, 1887. 8vo. *London.* The Society.

Odontological Society of Great Britain. Transactions. Vol. XX. Nos. 1–2. 8vo. *London* 1887. The Society.

Pathological Society. Transactions. Vol. XXXVIII. 8vo. *London* 1887; General Index to Vols. XXVI–XXXVII. 8vo. *London* 1887. The Society.

Pharmaceutical Conference. Year-Book of Pharmacy and Transactions of the Conference. 1887. 8vo. *London.* The Conference.

Photographic Society of Great Britain. Journal and Transactions. Vol. XII. No. 2. 8vo. *London* 1887. The Society.

Royal Institute of British Architects. Journal of Proceedings. Vol. IV. Nos. 4–6. 4to. *London* 1887–8. The Institute.

Royal Institution of Great Britain. Proceedings. Vol. XII. Part 1. No. 81. 8vo. *London* 1887; List of Members, &c. 1887. 8vo. *London.* The Institution.

Royal Meteorological Society. Quarterly Journal. Vol. XIII. No. 64. 8vo. *London* 1887. The Society.

Royal Microscopical Society. Journal. 1887. Part 6. 8vo. *London.* The Society.

Sanitary Institute of Great Britain. Transactions. Vol. VIII. 8vo. *London* 1887. The Institute.

Society of Biblical Archæology. Proceedings. Vol. IX. Nos. 1–7. Vol. X. Nos. 1–2. 8vo. *London* 1887. The Society.

Victoria Institute. Journal of Transactions., Vol. XXI. No. 83. 8vo. *London* [1887]. The Institute.

Manchester:—Free Library. Thirty-fifth Annual Report. 1886–7. 8vo. *Manchester.* The Library.

Transactions (*continued*).

Geological Society. Transactions. Vol. XIX. Part 11–12. 8vo. *Manchester* 1887. The Society.

Moscow:—Société Impériale des Naturalistes. Bulletin. Année 1887. No. 4. 8vo. *Moscou;* Meteorologische Beobachtungen. Jahrg. 1887. Erste Hälfte. Beilage zum Bulletin. Obl. 4to. *Moskau* 1887. The Society.

Munich:—K. B. Akademie der Wissenschaften. Sitzungsberichte (Math.-Phys. Classe). 1887. Heft 2. 8vo. *München;* Sitzungsberichte (Philos.-Philol. Classe). 1887. Hefte 1–3. 8vo. *Munchen.* The Academy.

Münster:—Königl. Theologische und Philosophische Akademie. Bericht der Facultäten. 4to. *Münster* 1887; Chronik der Akademie, 1886–7. 8vo. *Münster* 1887; Inaugural-Dissertationen, &c. 8vo. *Münster* 1886–7. The Academy.

Paris:—École Normale Supérieure. Annales. Année 1887. No. 12, and Supplément. 4to. *Paris.* The School.

Société Française de Physique. Séances. Janvier–Juillet, 1887. 8vo. *Paris.* The Society.

Rome:—Accademia Pontificia de' Nuovi Lincei. Anno XXXVII. Sessione 6–8. 4to. *Roma* 1884. Anno XXXVIII. Sessione 1–4. 4to. *Roma* 1886. The Academy.

Reale Accademia dei Lincei. Atti (Classe di Scienze Morali, Storiche e Filologiche). Gennaio–Dicembre, 1886, with Indice Topografico per l'anno 1886. 4to. *Roma.* The Academy.

Trieste:—Società Adriatica di Scienze Naturali. Bollettino. Vol. X. 8vo. *Trieste* 1887. The Society.

Upsala:—Société Royale des Sciences. Nova Acta. Vol. XIII. Fasc. 11. *Upsal* 1887. The Society.

Venice:—R. Istituto Veneto. Atti. Ser. 6. Tomo V. Disp. 2–9. 8vo. *Venezia* 1886–7; Memorie. Vol. XXII. Parte 3. 4to. *Venezia* 1887. The Institute.

Observations and Reports.

Christiania:—Norwegische Commission der Europäischen Gradmessung. Geodätische Arbeiten. Heft 5. 4to. *Christiania* 1887; Vandstandsobservationer. Heft 4. 4to. *Christiania* 1887. The Commission.

Cordova:—Observatorio Nacional Argentino. Resultados. Vol. IX. 4to. *Buenos Aires* 1887. The Observatory.

Dublin:—General Register Office. Weekly Returns of Births and Deaths in Dublin, and Quarterly Returns of Marriages, Births, and Deaths in Ireland. January to December, 1887. 8vo. *Dublin.* The Registrar-General.

Greenwich:—Royal Observatory. Observations. 1885. 4to. *London*

Observations, &c. (*continued*).

1887; Astronomical Results. 1885. Spectroscopic and Photographic Results. 1885. Magnetical and Meteorological Observations. 1885. 4to. *London* 1887; Report to the Board of Visitors, 1887. 4to. [*London*]. The Observatory.

London:—Middlesex Hospital. Reports for 1886. 8vo. *London* 1887. The Hospital.

Nautical Almanac Office. The Nautical Almanac for 1891. 8vo. *London* 1887. The Office.

Virginia:—Leander McCormick Observatory of the University of Virginia. Report, year ending June 1st, 1887. 8vo. 1887. The Observatory.

Vizagapatam:—G. V. Juggarow Observatory. Results of Observations, 1886; with an Introduction by A. V. Nursingrow. 8vo. *Calcutta* 1887. Mr. Nursingrow

Washington:—Bureau of Navigation. American Ephemeris and Nautical Almanac for 1890. 8vo. *Washington* 1887; Report of the Superintendent of the Nautical Almanac, 1887. 8vo. *Washington.* The Bureau.

U. S. Commission of Fish and Fisheries. Report. 1885. 8vo. *Washington* 1887. The Commission.

Wellington:—New Zealand University. Calendar. 1887–8. 8vo. *Wellington.* Sir J. Hector, F.R.S.

Journals.

Annales des Mines. Sér. 8. Tome XI. Livr. 3. 8vo. *Paris* 1887. École des Mines.

Asclepiad (The) Vol. IV. No. 16. 8vo. *London* 1887. Dr. Richardson, F.R.S.

Ateneo Veneto (L'.) Rivista Mensile di Scienze, Lettere ed Arti. Ser. 10. Vol. II. Num. 5–6. Ser. 11. Vol. I. Num. 1–6. Vol. II. Num. 1–2. 8vo. *Venezia* 1886–7. The R. Istituto Veneto.

Bullettino di Bibliografia e di Storia delle Scienze. Matematiche e Fisiche. Tomo XX. Gennaio–Aprile, 1887. 4to. *Roma.* The Prince Boncompagni.

Horological Journal. Vol. XXX. Nos. 352–3. 8vo. *London* 1887–8. The Horological Institute.

Naturalist (The) No. 150. 8vo. *London* 1888. The Editors.

Naturalists' Monthly (The) Vol. I. Nos. 1–5. 4to. *London* 1887–8. The Editor.

Revista do Observatorio. Anno II. Num. 10–11. 8vo. *Rio de Janeiro* 1887. The Imperial Observatory, Rio de Janeiro.

Scientific News. December, 1887. 4to. *London.* The Editor.

Zeitschrift für Naturwissenschaften. 1886. Hefte 2–6; 1887. Hefte 1–2. 8vo. *Halle.* Naturwissenschaftlicher Verein, Halle.

February 2, 1888.

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

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

The following Papers were read:—

I. "On Tidal Currents in the Ocean." By J. Y. BUCHANAN, M.A., F.R.S.E. Communicated by the late Sir FREDERICK EVANS, K.C.B., R.N., Hydrographer to the Admiralty. Received March 24, 1884. Received after revision January 23, 1888.

It is frequently asserted and commonly believed that tidal currents do not exist in the open ocean or in waters remote from land. Oceanic currents, that is, streams which set more or less constantly in one general direction, are well-known and, from their importance to navigation, have been the objects of much study. Chief among these may be mentioned the Gulf Stream and the Equatorial Currents in both oceans. The data on which almost all our information connected with these streams rests are furnished by the logs of ships traversing them. When the position of the ship is determined from day to day by good astronomical observations on the one hand, and the courses and distances sailed are carefully observed and noted on the other, it is usual, after allowing for known perturbing causes, to ascribe the differences between the positions as ascertained by "observation" and by "dead reckoning" to the effect of a current. As in the ordinary routine of a sea-going ship, the positions are made up from noon to noon, the strength and direction of the current so deduced is the integral resultant "set" of the previous twenty-four hours. The direction and strength of the current may have changed in any way during that time, and it would be nearly impossible to detect such changes. The period of twenty-four hours corresponds closely with that of the tidal wave, consequently in the time elapsing between two successive noons, whatever effect may have been due to a tidal cause will have completely reproduced itself twice over and a very little more. The resultant current due to the tide during the two complete periods will be nothing, and the only resultant current affecting the day's reckoning will be that due to the difference between the solar and lunar day. From the nature of the observations the effect due to this would not be easily detected. It is evident then that the ordinary method of observing oceanic currents is such as completely to cloak any tidal effect which may exist. The proximate source of energy for the production of tidal currents is the tidal wave.

Storm waves are confined to the surface of the ocean, and break only in comparatively shallow water. The tidal wave affects the deepest oceans to the bottom. It might, therefore, be reasonably expected that, in passing over many of even the deeper ridges which traverse the ocean bed, its character as an undulation would be modified with the production of a true tidal current. We know that in the shallow water surrounding the land and in the bays and inlets which indent its coasts, a portion of the tidal energy is dissipated by the partial transformation of the wave into currents.

In littoral waters these currents are necessarily exaggerated by the confinement produced by the neighbouring land; but the presence of a shoal alone, without any dry land in the vicinity, ought to be sufficient to produce well-marked and regular tidal currents.

Considerations of this nature determined me to take the first opportunity which might offer of putting the matter to the test of observation.

Thanks to the hospitable invitation of the India Rubber, Gutta Percha, and Telegraph Works Company, of Silvertown, I had the good fortune to spend the months of October and November, of 1883, on board the s.s. "Dacia," one of their excellently equipped cable ships, which, along with the s.s. "International," was sent out to connect Cadiz with the principal islands of the Canary Group by means of a telegraph cable. The whole expedition was under the command of the Company's Telegraphic Engineer-in-chief, Mr. Robert Kaye Gray, to whom I am particularly indebted for the facilities which were afforded me in carrying out this and many other investigations, and I beg publicly to tender him my best thanks.

In the course of the sounding operations carried out with a view of gaining a thorough acquaintance with the depth and nature of the sea bottom, over which it was proposed to lay the cable, many remarkable inequalities were met with. Perhaps the most striking was one which was called the "Dacia Bank," after the ship on which it was discovered. This bank, which occupies a surface of 50 square miles with less than 100 fathoms of water on it, rises rapidly from the prevailing depth of 1800 or 1900 fathoms to within 500 fathoms of the surface, whence the slope is very abrupt and in many places precipitous to within 100 fathoms of the surface. As the bank lay close to the proposed route of the cable, two days were spent in surveying it carefully. In order to have a fixed point to refer the soundings to, a mark-buoy was anchored in 175 fathoms, just outside the precipitous edge of the bank.

On the afternoon of the 21st October, 1883, I spent several hours in one of the ship's boats made fast to this buoy, and during that time I made frequent observations of the rate and direction of the surface current as well as of the general direction of the under current. (See Table I.)

It had been observed during the previous day and night that at times the current set strongly to the southward, at other times became nearly slack and even ran to the northward. While the boat was being lowered and got away the ship drifted very slowly to the northward past the buoy and against a light northerly air blowing at the time. When the boat was made fast to the buoy the current was found setting to the northward, against the wind and sea, and measures were immediately taken for determining its direction and velocity at frequent intervals. For this purpose an ordinary life-buoy was attached to the end of a line which was marked at every fifth fathom with a piece of wood, which also served the purpose of keeping the line afloat and of showing whether it was going out straight or not. Although the wind was only barely perceptible, it was found to retard the life-buoy. An arrangement of canvas was accordingly weighted and hung down in the axis of the buoy. This greatly increased its hold on the water and made its movements dependent only on the current. The direction of the current was observed with a pocket azimuth compass for use on land. Although there was hardly any wind, there was a considerable swell coming up from the north, but it did not produce any motion sufficiently violent to interfere with the use of the compass. In order, however, to remove any uncertainty, which might have existed with regard to the correctness of the bearings so observed, I always took a bearing of the sun at the same time, as an index of the trustworthiness of the current observations.

No accurate measurements were made of the under current, but while the surface current was being observed a tow-net lashed to a sounding line was lowered to 35 fathoms for one hour, and to 70 fathoms also for an hour. The directon taken by the sounding line showed that down to 75 fathoms the direction inclined slightly more to the eastward than the surface current, and its strength seemed to be slightly greater.

The observed bearings of the sun give for the local variation 17° W., 17° W., 21° W., 21° W. According to the chart the variation is 19° W. The bearings therefore as determined in the boat may be depended on to a quarter of a point.

Time was taken by a watch set to local time. In letting the current log run out care was taken to put no strain on the line, so

Table I.—Observations of Surface Current on the Dacia Bank, lat. 31° 10′ N., long. 13° 14′ W. 21st October, 1883.

Time P.M. (local)		1 hr. 55 m.	2 hr. 15 m.			2 hr. 40 m.			3 hr. 30 m.			4 hr. 6 m.		
Sun's bearing (magnetic)......		N. 235° E.	N. 240° E.			N. 250° E.			N. 260° E.					
„ „ (true)		N. 218° E.	N. 223° E.			N. 229° E.			N. 239° E.					
„ variation (apparent)		17° W.	17° W.			21° W.			21° W.					
Direction of current (magnetic)		N. 30° E.	N. 30° E.			N. 60° E.			N. 75° E.			N. 120° E.		
„ „ (true)		N. 11° E.	N. 11° E.			N. 41° E.			N. 56° E.			N. 101° E.		
	Fathoms.		min.	sec.	Diff. sec.	min.	sec.	Diff. sec.	min.	sec.	Diff. sec.	min.	sec.	Diff. sec.
Time when each mark on the line passed into the water.	0	.. ..	0	0	—	0	0	—	0	0	—	0	0	—
	5	.. ..	0	40	40	0	55	55	1	30	90	0	50	50
	10	.. ..	1	20	40	2	0	65	2	5	35	2	0	70
	15	.. ..	1	55	35	3	5	65	3	15	70	3	0	60
	20	.. ..	2	25	30	4	10	65	4	40	85	4	2	62
	25	.. ..	2	55	30	5	15	65	5	50	70	5	6	64
	30	.. ..	3	25	30	6	15	60	6	55	65	5	57	51
	35	.. ..	4	5	40	7	5	50	7	55	50	6	51	54
	40	.. ..	4	50	40	7	55	50	9	10	75	7	45	54
	45	.. ..	5	30	40	8	45	50	10	12	62	8	35	50
	50	.. ..	6	5	35	9	45	60	11	20	68			
	55	.. ..	6	50	45									
Line taut	50	.. ..	..		..	10	0	..	11	30	..	9	45	
	55	.. ..	7	5										
Current (knots per hour)		.. ..	0·47			0·30			0·26			0·30		

that the intervals at which successive marks passed out of the boat must not be compared too rigidly. When the whole length of line, 50 or 55 fathoms, as the case might be, was paid out, it was allowed to tauten itself, and the time observed when it became taut. The bearing of the float was then taken, and that of the sun immediately afterwards. The results of the observations are summarised in the following table.

Table II.—Summary of Current Observations made on the Afternoon of the 21st of October, 1883, on the edge of the Dacia Bank, lat. 31° 10′ N., long. 13° 34′ W., depth 175 fathoms.

Time (P.M.)	1 hr. 55 m.	2 hr. 15 m.	2 hr. 40 m.	3 hr. 30 m.	4 hr. 6 m.
Direction (true)....	N. 11° E.	N. 11° E.	N. 41° E.	N. 56° E.	N. 101° E.
Rate in knots per hr.	..	0·47	0·30	0·26	0·30

"It will be seen from these observations that, in two hours, the current had shifted its direction through 90°, and had passed through a minimum velocity of 0′·26 per hour without there having been any period of 'slack water.' The observations are too few in number to make it worth while submitting them to analysis, but a little study of them will show that they indicate a current which is the resultant of a constant current and a periodic one. A constant current running S.E. by E., combined with a tidal current running N.N.W. and S.S.E., the maximum velocity of which, in either direction, is twice that of the permanent current, would give a resultant current agreeing fairly with that observed."*

In these circumstances, during a complete tidal interval the water flows along an S-like path, and in the twenty-four hours it describes two such figures, and moves on in a zig-zag course. It is apparent that the integral drift in the twenty-four hours is that due to the constant current alone independently of the tidal current. The same holds for twelve hours. Hence, if observations are carried on at frequent and regular intervals for twelve hours, we are able to determine both the constant current and the tidal current superposed on it, and it is to be hoped that when they can be made they will not be neglected by surveying ships and telegraph ships.

Many banks are already known in the North Atlantic on which such observations could conveniently be made, and there are probably many more scattered about the ocean, for instance, on the ridge called after the U.S.S. "Dolphin," which extends along a somewhat crooked line from the Azores to Tristan da Cunha. It is of course important to have fine weather; then the boat in which the observations are made

* 'Edinburgh, Roy. Soc. Proc.,' vol. 13, 1886, p. 437.

should, if possible, be moored with two lines to prevent swaying. In cable ships large mushroom anchors with heavy chain bridles are in common use. If one of them is dropped, a ship's boat may be anchored to it with no more line than is required to reach the bottom, and be in no danger of swaying. Similar observations should be made also from a boat anchored in very deep water.

No measurements were made of the under current, but by sinking a tow-net made fast to a sounding line, it was seen to be running in the same direction as the surface current, and apparently with much the same velocity. In the channels between the Canary Islands where even on the shallowest ridges the water is over 1000 fathoms in depth, the tidal current reaches to the very bottom, and its scouring action is shown by the nature of the bottom. To seaward in 1800 to 2000 fathoms the bottom is a fine *Globigerina* ooze, which gets coarser and sandier as the water shoals in the channels, till on the summit ridge there is generally no deposit at all, and the bottom is rock or coral coated with black oxide of manganese. Round the western end of Tenerife the tide runs violently causing rips and overfalls. Much rocky ground is met with in the North Atlantic in depths of even 1300 and 1400 fathoms, especially on the ridge which extends through the whole length of that ocean. It is not unlikely that the summit edge of this ridge is swept clean through the greater part of its length, and it must be remembered that the removal of sediment from one part of the ocean bottom means its deposit in greater abundance in others, and especially in hollows in the neighbourhood of the ridge. Hence a sounding in "ooze or clay" in one position furnishes no argument against the trustworthiness of another sounding in the vicinity and in equally deep water on "rock" or "hard ground." Such ridges are great enemies to telegraph cables, for while the tidal currents keep the rock-surface clean, they also tend to give the cable an oscillating or surging motion, which is apt to bring it in rubbing contact with the rock-surface and so to wear it through. On the other hand these currents, in sweeping clean the rocky eminences at the bottom of the ocean, prepare a lodging place for deep-sea corals, and bring food to them when settled, thus enabling them to build up their pillar-like banks, a very fine example of which was discovered and surveyed by the "Dacia" on the 12th October, 1883. It lies in lat. 34° 57′ N., long. 13° 57′ W., and the shoalest sounding was 435 fathoms. The surface of the bank was locally very rough, and sloped gradually to the edge in about 550 fathoms, when it terminated in an actual precipice, dropping to 835 fathoms in one place.

The coral on this bank was living and growing in the greatest luxuriance, and many specimens which were brought up showed that the living corals were growing on a mass of dead ones. There can therefore be little doubt that in this case we have a submarine bank which is in vigorous growth towards the surface, and which has been in existence long enough to have risen through a height of about 300 fathoms or 1800 feet. I have little doubt that in a large number of the coral islands of the Pacific, the intermediate platform between the tropical reef-building coral and the volcanic peak, plateau, and ridge, which most probably form the foundation, is formed by these deep-sea corals largely assisted by annelids, especially Serpulæ, which secrete calcareous tubes. The tidal currents assist their growth both by bringing the animals nourishment and by removing light *débris* which might choke them.

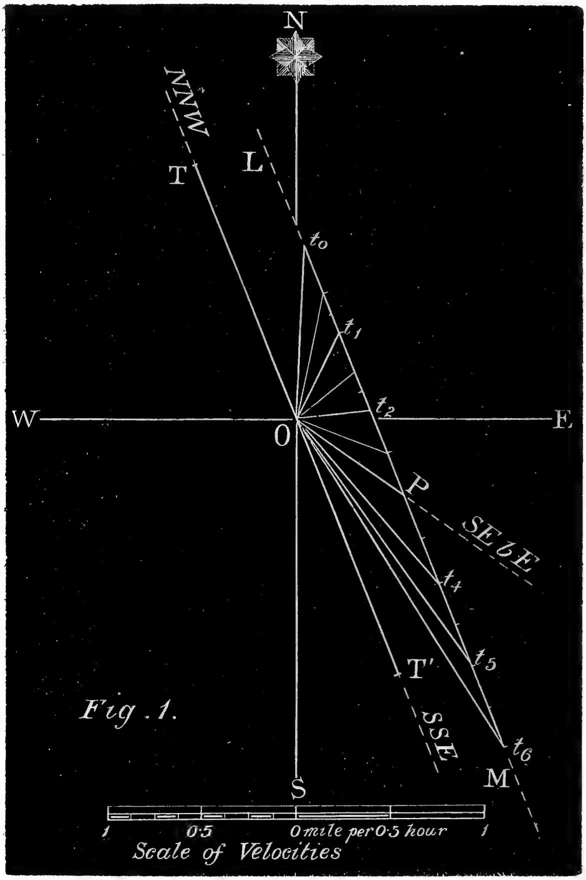


Diagram showing the resultant currents due to the composition of a constant current, OP, with a tidal current, OT, OT.′

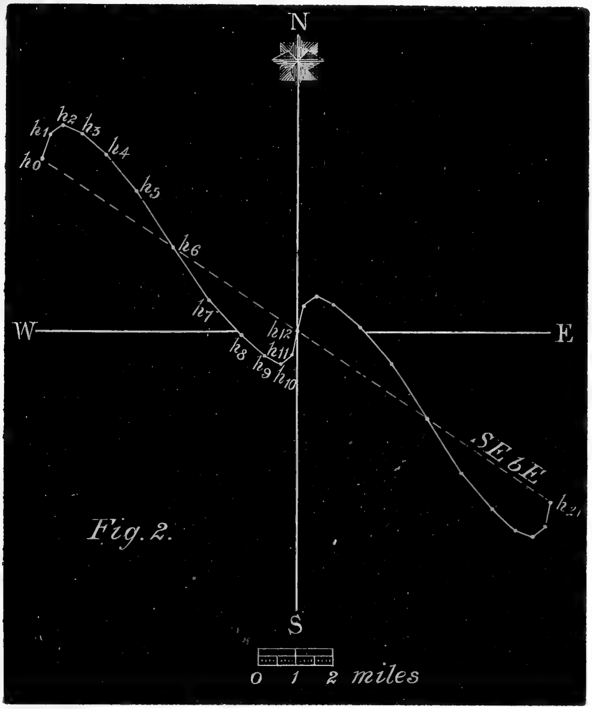


Diagram showing the path described in twenty-four hours by a particle under the influence of the tidal and constant currents of fig. 1.

II. "On the Spectrum of the Oxyhydrogen Flame." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received January 18, 1888.

(Abstract.)

In a former communication the authors described simultaneously with Dr. Huggins the strongest portion of the spectrum of water, subsequently they described a second less strong but more refrangible section of the same spectrum. M. Deslandres has noticed a third still more refrangible section. The authors now find that the spectrum extends, with diminishing intensity, into the visible region on the one hand, and far into the ultra-violet on the other. These faint parts of the spectrum they have photographed, using the dispersion of a single calcite prism and a lengthened exposure; and in the present communication they give a map of the whole extent observed, and a list of wave-lengths of upwards of 780 lines.

The spectrum exhibits the appearance of a series of rhythmical groups more or less overlapping one another, and the arrangement of the lines in these groups is shown to follow, in many cases, the law that the distances between the lines, as measured in wave-lengths, are in an arithmetic progression. M. Deslandres had previously announced that the succession of lines in A, B, and *α* follow this law when their distances are measured in reciprocals of wave-lengths, and he has stated that the groups A, B, and *α* have counterparts in the spectrum of water. The authors find a striking resemblance between those groups and certain parts of the water spectrum, but no exact correspondence.

Dr. Grünwald, of Prague, predicted on theoretical grounds that certain lines would appear in the spectrum of water, and the authors have found a considerable number of lines which tally closely with Dr. Grünwald's predictions, some of them, in the extremities of the spectrum, being the strongest lines observed in those regions.

III. "On the Voltaic Circles producible by the mutual Neutralisation of Acid and Alkaline Fluids, and on various related Forms of Electromotors." By C. R. ALDER WRIGHT, D.Sc., F.R.S., Lecturer on Chemistry and Physics, and C. THOMPSON, F.I.C., F.C.S., Demonstrator of Chemistry, in St. Mary's Hospital Medical School. Received January 18, 1888.

[Publication deferred.]

Presents, February 2, 1888.

Transactions.

Bern:—Naturforschende Gesellschaft. Mittheilungen. Jahrgang 1885. Heft 3. 8vo. *Bern* 1886. The Society.

Brünn:—Naturforschender Verein. Verhandlungen. Band XXIV. Hefte 1–2. 8vo. *Brünn* 1886; Bericht der Meteorologischen Commission für 1884. 8vo. *Brünn* 1886. The Verein.

Brunswick:—Verein für Naturwissenschaft. Jahresbericht 3–4. 8vo. *Braunschweig* 1883, 1887. The Verein.

Cambridge, Mass.:—Harvard College. Museum of Comparative Zoölogy. Annual Report. 1886–7. 8vo. *Cambridge* 1887. The Museum.

Harvard University. Bulletin. October, 1887. 8vo. [*Cambridge*]. The University.

Catania:—Accademia Gioenia di Scienze Naturali. Processi Verbali. 1887. Nos. 1–2. 4to. [*Catania*]. The Academy.

Transactions (*continued*).

Copenhagen:—K. Danske Videnskabernes Selskab. Oversigt. 1887. No. 2. 8vo. *Kjøbenhavn;* Skrifter. Bd. IV. Afd. 4–5. 4to. *Kjøbenhavn* 1887. The Academy.

Cordova:—Academia Nacional de Ciencias. Actas. Tomo V. Entrega 3. 4to. *Buenos Aires* 1886. The Academy.

Delft:—École Polytechnique. Annales. Tome III. Livr. 3. 4to. *Leide* 1887. The School.

Dublin:—Royal Geological Society of Ireland. Journal. Vol. VIII. Part 2. 8vo. *Dublin* 1887. The Society.

Eastbourne:—Natural History Society. Transactions. Vol. I. Part 11. Vol. II. Part 1. 8vo. *Eastbourne* [1887]. The Society.

Essex Naturalists' Field Club:—The Essex Naturalist. 1887. 8vo. Nos. 10–11. *Buckhurst Hill.* The Essex Field Club.

Florence:—Biblioteca Nazionale Centrale. Bollettino. Gennaio—Dicembre, 1887. 8vo. *Firenze;* Indici e Cataloghi. Vol. I. Fasc. 6. 8vo. *Roma* 1887. The Library.

Frankfurt-am-Main:—Senckenbergische Naturforschende Gesellschaft. Bericht. 1886. 8vo. *Frankfurt* 1886. The Society.

Frankfurt-am-Oder:—Naturwissenschaftlicher Verein. Monatliche Mittheilungen aus dem Gesammtgebiete der Naturwissenschaften. Jahrg. V. Nr. 3–8. 8vo. [*Frankfurt*] 1887; Societatum Litterae. 1887. Nos. 3–10. 8vo. *Frankfurt.* The Verein.

Haarlem:—Musée Teyler. Archives. Sér. 2. Vol. III. Partie 1. Lar. 8vo. *Haarlem* 1887; Catalogue de la Bibliothèque. Livr. 5–6. Lar. 8vo. *Haarlem* 1886. The Museum.

Société Hollandaise des Sciences. Archives Néerlandaises des Sciences Exactes et Naturelles. Tome XXII. Livr. 2–3. 8vo. *Haarlem* 1887; Natuurkundige Verhandelingen. Verzameling 3. Deel IV. Stuk 4. Deel V. Stuk 1. 4to. *Haarlem* 1887. The Society.

Hamburg:—Naturhistorisches Museum. Bericht. 1886. 8vo. *Hamburg* 1887. The Museum.

Naturwissenschaftlicher Verein. Abhandlungen aus dem Gebiete der Naturwissenschaften. Band X. 4to. *Hamburg* 1887. The Verein.

Heidelberg:—Universitäts-Bibliothek. Katalog der Handschriften. Bd. I. Die Altdeutschen Handschriften. Karl Bartsch. 4to. *Heidelberg* 1887. The Library.

International:—Congrès International d'Anthropologie et d'Archéologie Préhistoriques. Compte-Rendu. Session 8. Vol. II. Partie 1–2. 8vo. *Budapest* 1878, 1886. The Congress.

Lisbon:—Academia Real das Sciencias. Memorias. (Classe de

Transactions (*continued*).

Sciencias Moraes, Politicas e Bellas-Lettras.) Tomo V. Partie 2. Tomo VI. Partie 1. 4to. *Lisboa* 1882, 1885; Cartas de Affonso de Albuquerque. Tomo I. 4to. *Lisboa* 1884; Documentos Remettidos da India ou Livros das Monções. Tomos II–III. 4to. *Lisboa* 1884–5; Corpo Diplomatico Portuguez. Relações com a Curia Romana. Tomos VI–IX. 4to. *Lisboa* 1884–6; Historia dos Estabelecimentos Scientificos Litterarios e Artisticos de Portugal, por J. S. Ribeiro. Tomos X–XIV. 8vo. *Lisboa* 1882–5; Jornal de Sciencias Mathematicas Physicas e Naturaes. Num. 30–44. 8vo. *Lisboa* 1881–7; Estudos sobre as Provincias Ultramarinas, por J. de Andrade Corvo. Vols. I–III. 8vo. *Lisboa* 1883–5; Curso de Silvicultura, por A. Coutinho. Tomo 1. 8vo. *Lisboa* 1886, &c., &c. The Academy.

Paris:—Société Mathématique de France. Bulletin. Tome XV. No. 7. 8vo. *Paris* 1887. The Society.

Société Philomathique. Bulletin. Tome XI. No. 4. 8vo. *Paris* 1887. The Society.

Pesth:—K. Ungar. Geologische Anstalt. Jahresbericht für 1885. 8vo. *Budapest* 1887; Mittheilungen. Bd. VII. Heft 6. Bd. VIII. Heft 5. 8vo. *Budapest* 1887; Földtani Közlöny. Kötet XVII. Füzet 1–6. 8vo. *Budapest* 1887. The Institute.

Philadelphia:—Academy of Natural Sciences. Proceedings. 1887. Part 2. 8vo. *Philadelphia.* The Academy.

Wagner Free Institute of Science. Transactions. Vol. I. 8vo. *Philadelphia* 1887. The Institute.

St. Petersburg:—Académie Impériale des Sciences. Bulletin. Tome XXXI. No. 4. Tome XXXII. No. 1. 4to. *St. Pétersbourg;* Mémoires. Tome XXXV. Nos. 2–7. 4to. *St. Pétersbourg* 1887; Supplementband V. zum Repertorium für Meteorologie. 4to. *St. Petersburg* 1887. Atlas. Folio. *St. Petersburg* 1887. The Academy.

Switzerland:—Société Helvétique des Sciences Naturelles. Compte Rendu des Travaux présentés à la 68e Session réunie au Locle. 8vo. *Genève* 1885; Actes. 8vo. *Neuchâtel* 1886. The Society.

Sydney:—Linnean Society of New South Wales. Proceedings. Vol. II. Part 2. 8vo. *Sydney* 1887. The Society.

Royal Society of New South Wales. Journal and Proceedings. Vol. XX. 1886. 8vo. *Sydney* 1887. The Society.

Vienna:—K. Akademie der Wissenschaften. Denkschriften (Math.-Naturw. Classe). Band LI–LII. 4to. *Wien* 1886–7; Register zu den Bänden XV–XXXV. 4to. *Wien* 1887; Sitzungsberichte (Math.-Naturw. Classe). Abth. 1. Bd. XCIII. Hefte 4–5. Bd. XCIV. Hefte 1–5; Ditto, Abth. 2. Bd. XCIII. Hefte

Transactions (*continued*).
3–5. Bd. XCIV. Hefte 1–5. Bd. XCV. Hefte 1–2; Ditto, Abth. 3. Bd. XCIII. Hefte 1–5. Bd. XCIV. Hefte 1–5. 8vo. *Wien* 1886–7; Sitzungsberichte (Phil.-Hist. Classe). Bd. CXII. Hefte 1–2. Bd. CXIII. Heft 1–2. Bd. CXIV. Heft 1. 8vo. *Wien* 1887. The Academy.

Buelna (E.) Peregrinacion de los Aztecas y Nombres Geográficos Indígenas de Sinaloa. 8vo. *México* 1887. The Mexican Minister.

Burmeister (H.) Atlas de la Description Physique de la République Argentine. Section II. Mammifères. Livr. 3. Folio. *Buenos Aires* 1886; with accompanying Text in 4to. Dr. H. Burmeister.

Macadam (W. J.) Notes on the Ancient Iron Industry of Scotland. Sm. 4to. [*Edinburgh*] 1886. The Author.

McCoy (F.), F.R.S. Prodromus of the Zoology of Victoria. Decade 1–15. 8vo. *Melbourne* 1878–87. The Victorian Government.

Mirinny (L.) Aperçu Élémentaire abrégé de l'Héliogénèse ou de la Formation des Systèmes Solaires. 12mo. *Paris* 1887. The Author.

Sanna Solaro (G. M.) I Terremoti: Ricerche sulle Cause che li Producono. 8vo. *Prato* 1887. The Author.

Scacchi (A.) Catalogo dei Minerali Vesuviani. 4to. *Napoli* 1887; La Regione Vulcanica Fluorifera della Campania. 4to. *Napoli* 1887. The Author.

Schultz (J. F. H.) Zur Sonnenphysik. 4to. *Kiel* 1887. The Author.

Steven (J. L.) Lectures on Fibroid Degeneration and allied Lesions of the Heart. [Reprint] 8vo. *London* 1887. The Author.

February 9, 1888.

Dr. E. FRANKLAND, Vice-President, in the Chair.

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

The following Papers were read:—

I. "The Small Free Vibrations and Deformation of a Thin Elastic Shell." By A. E. H. LOVE, B.A., Fellow of St. John's College, Cambridge. Received January 19, 1888.

(Abstract.)

In this paper the method employed by Kirchhoff and Clebsch for the treatment of a thin plane plate is applied to the case of a thin shell, or plate of finite curvature. The form of the potential-energy-function for the strain in an element of the shell is the same as that obtained by Kirchhoff for a plate, the quantities depending on the curvature of the surface being replaced by the difference of their values in the strained and unstrained states. It is proved that only for an inextensible spherical surface is this function the same function of the changes of principal curvature as for a plane plate. The general equations of equilibrium and small motion under the action of any system of forces are formed. It is shown that in general the shell cannot vibrate in such a manner that no line on the middle-surface is altered in length, because this condition makes it impossible to satisfy the boundary conditions which hold at a free edge. It then appears that approximate equations of motion may be taken, in which the terms of the potential energy depending on the bending may be neglected, and only those depending on the stretching need be retained. It is shown that surfaces of uniform curvature with no bounding edges are the only ones which admit of purely normal vibrations, and that vibrations in which the displacement is purely tangential are possible on all shells whose middle-surfaces are surfaces of revolution bounded by small circles. The cases of the spherical and cylindrical shell receive special discussion. The equations of motion can always be solved, but solutions of the frequency equations could only be obtained in case the displacement was symmetrical about the axis. The application of the general equations to problems of equilibrium is illustrated in the case of spherical shells, for which

the equations can be solved; some special examples are given. For the sake of greater intelligibility, I have included an historical account of previous theories of plane plates and shells, a description of the method of the present paper, and a summary recapitulating the chief physical results.

II. "True Teeth in the young *Ornithorhynchus paradoxus*." By EDWARD B. POULTON, M.A., F.L.S., of Jesus and Keble Colleges, Oxford. Communicated by W. K. PARKER, F.R.S. Received January 26, 1888.

For the purpose of continuing some recent work upon various epidermic structures in Ornithorhynchus, Dr. Parker very kindly placed his most valuable material at my disposal. Among other things was a series of consecutive vertical transverse sections through the head of a young individual, about 8·3 decimetres long, when in the curled-up attitude in which it had been received, and which was fixed by the spirit. In this specimen only the larger hairs had appeared above the surface of the skin.

The sections had been prepared for Dr. Parker by his son, Professor W. Newton Parker, of Cardiff, and although intended for the investigation of morphological points in connexion with the development and structure of the skull, many of them were in every way adapted for minute histological investigation. Examining these sections I found that large and apparently typical mammalian teeth were developing in the subepithelial tissues on each side of the roof of the mouth. I at once communicated with Dr. Parker, telling him of the discovery, and enquiring whether he had any objection to the publication of the fact. Dr. Parker replied, and urged me to at once communicate the discovery to the Royal Society, at the same time offering me material in the most free and generous manner for the further investigation of the dental structures in Ornithorhynchus and in Echidna (if present in the latter). When it is remembered that Dr. Parker had put the sections aside for a time in consequence of the press of other work, intending soon to make use of them for the investigation of the skull, it will be seen at once that my association with this discovery is purely accidental, and that I have been treated in an extremely generous spirit.

As the lower jaw was not included in the sections, I cannot yet state that teeth are present in it, but there is little doubt that this is the case.* Teeth were present in the upper jaw, in thirty sections through the head, and of these all, except the nine anterior sections, included some part of the eye. The teeth probably represent some

* I have since found that teeth are present in the lower jaw.—Feb. 6, 1888.

part of the molar series in the higher mammals. Examining the sections from the front backwards, the first tooth appeared a little behind the anterior margin of the epithelial elevation, which appears to represent the developing horny plate which in the adult is the functional representative of true teeth. The teeth seem to form a tolerably straight line, extending internally to the horny plates, and passing considerably further backwards than the latter.* Owing to imperfections in this part of some of the sections, I could not determine the exact number of teeth with accuracy, but they appear to be five or six in number on each side.† The most anterior of these is of a different character from the others, and is apparently separated from them by an interval which is longer than in other cases. This anterior tooth is the most developed, and its apex extends so far towards the surface that it nearly touches the epithelium. It is a pointed cylindrical tooth, directed vertically downwards. The four† or five posterior teeth are of uniform shape. Their structure, appearance, and relation to the surface are shown in fig. 1 (× 40). The two chief cusps of each of these broad teeth arise from the inner side of the surface.‡

The structure of the enamel-cap is entirely normal, except that capillaries are certainly present in the middle membrane, intruding from without. The inner layer of long enamel cells is very distinct (see figure). No enamel is formed from them at this stage, except probably in the case of the anterior tooth.§ The dentine is quite normal in appearance and formation in the posterior teeth, except that the striation due to dentinal tubules can only be made out beneath the apex, but this may be due to the condition of the specimen or to method of preparation.|| The inner part of the dentine stains faintly in carmine, and shows the striation; the outer part does not stain, and appears homogeneous. The dentine of the anterior teeth is much thicker, and is not of normal character|| in its inner part, its outer part resembling that of the other teeth.

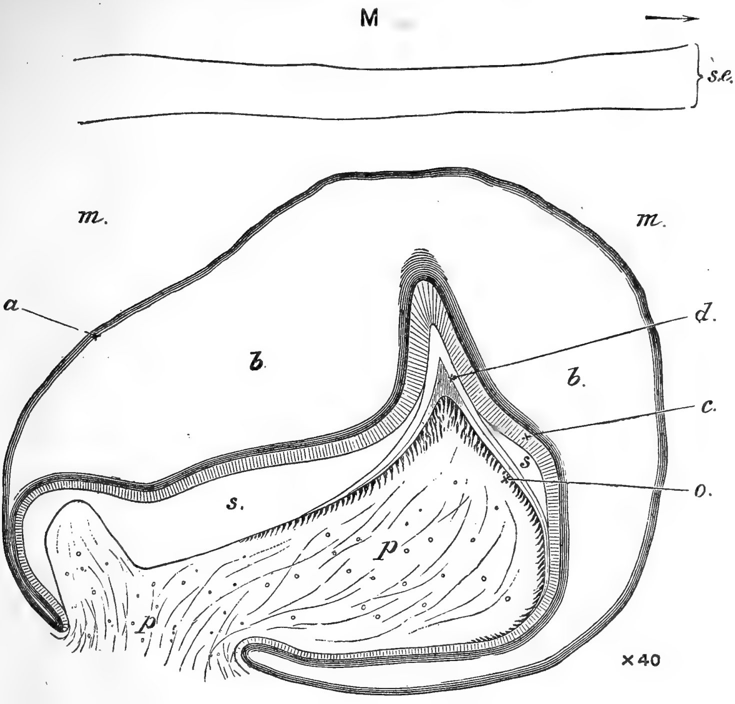
* In the lower jaw the teeth appear to lie exactly beneath the developing plate. This may be the case in the upper jaw also, for the epithelium was in a damaged condition, and I may have been mistaken in my first identification of the undeveloped horny plate. Comparison with a skull of the same age strongly supports this conclusion.—Feb. 6, 1888.

† I have since found that the teeth are fewer in number, probably three being present upon each side of each jaw. The two posterior teeth have many cusps, and the two largest of these looked like separate teeth in sections. The true shape has been shown by a dissected preparation of the lower jaw.—Feb. 6, 1888.

‡ In the lower jaw the two chief cusps arise from the *outer* side of the teeth.—Feb. 6, 1888.

§ Recently prepared sections, made in order to decide this point, have shown that enamel is certainly present.—Feb. 6, 1888.

|| Recently prepared sections have shown that the dentine is of the usual struc-



There can be no doubt that these structures are characteristic Mammalian teeth, and their appearance harmonises well with the results of Hertwig's researches on the structure and development of Placoid scales. His researches indicate that the Mammalian teeth are probably in a more ancestral condition than any other organ possessed by the adult. They must have been derived at one time from Prototherian ancestors, and yet existing Prototheria were not known to possess them. Their occurrence in Ornithorhynchus, therefore, supplies the step just where it is wanted, and the fact that they are practically identical with the teeth of higher mammals is a further indication of the ancestral nature of these structures, for other higher mammalian features represented in the Prototheria are profoundly modified in the latter.

Dr. Parker has very kindly placed his material at my disposal, so that I propose to at once investigate, and shall shortly publish a paper upon, the nature of the teeth in the lower jaw of Ornithorhynchus and in Echidna (for it is in every way probable that they will be found in this genus also). I also intend to work at the mature organism,

ture, although in some sections it has been rendered apparently homogeneous, probably by some method of preparation.—Feb. 6, 1888.

for it seems probable that such large tooth-rudiments may be traceable in later stages.

In this investigation, the few points of uncertainty as to the minute structure of some of the tissues will in all probability be settled satisfactorily.*

DESCRIPTION OF FIGURE. ×40.

The arrow points towards the middle line of the mouth.

M. Mouth.
s, e. Superficial epithelium of oral cavity.
m. Sub-epithelial tissues forming tooth-sac immediately outside enamel-cap.
a. Outer membrane of enamel-cap.
b. Middle membrane of enamel-cap.
c. Inner membrane of enamel-cap; the enamel cells. The dark layer just external to the enamel cells represents the stratum intermedium of Hannover.
d. The dentine, of which the inner part stains faintly and shows the delicate dentinal tubes.
s. Space caused by shrinkage between enamel cells and the outer part of the tooth rudiment.
p. Tooth papilla.
o. The odontoblasts forming the superficial layer of the papilla.

III. "On the Relative Densities of Hydrogen and Oxygen. Preliminary Notice." By Lord RAYLEIGH, Sec. R.S., Professor of Natural Philosophy in the Royal Institution. Received February 2, 1888.

The appearance of Professor Cooke's important memoir upon the atomic weights of hydrogen and oxygen† induces me to communicate to the Royal Society a notice of the results that I have obtained with respect to the relative *densities* of these gases. My motive for undertaking this investigation, planned in 1882,‡ was the same as that which animated Professor Cooke, namely, the desire to examine whether the relative atomic weights of the two bodies really deviated from the simple ratio 1 : 16, demanded by Prout's Law. For this purpose a knowledge of the densities is not of itself sufficient; but it appeared to me that the other factor involved, viz., the relative atomic *volumes* of the two gases, could be measured with great accuracy by eudiometric methods, and I was aware that Mr. Scott had in view a redetermination of this number, since in great part carried out.§ If both investigations are

* This appears to have been already the case.—Feb. 6, 1888.

† "The Relative Values of the Atomic Weights of Hydrogen and Oxygen," by J. P. Cooke and T. W. Richards, 'Amer. Acad. Proc.,' vol. 23, 1887.

‡ Address to Section A, British Association 'Report,' 1882.

§ "On the Composition of Water by Volume," by A. Scott, 'Roy. Soc. Proc.,' June 16, 1887 (vol. 42, p. 396).

conducted with gases under the normal atmospheric conditions as to temperature and pressure, any small departures from the laws of Boyle and Charles will be practically without influence upon the final number representing the ratio of atomic weights.

In weighing the gas the procedure of Regnault was adopted, the working globe being compensated by a similar closed globe of the same external volume, made of the same kind of glass, and of nearly the same weight. In this way the weighings are rendered independent of the atmospheric conditions, and only small weights are required. The weight of the globe used in the experiments here to be described was about 200 grams, and the contents were about 1800 c.c.

The balance is by Oertling, and readings with successive releasements of the beam and pans, but without removal of the globes, usually agreed to $\frac{1}{10}$th mgrm. Each recorded weighing is the mean of the results of several releasements.

The balance was situated in a cellar, where temperature was very constant, but at certain times the air currents, described by Professor Cooke, were very plainly noticeable. The beam left swinging over night would be found still in motion when the weighings were commenced on the following morning. At other times these currents were absent, and the beam would settle down to almost absolute rest. This difference of behaviour was found to depend upon the distribution of temperature at various levels in the room. A delicate thermopile with reflecting cones was arranged so that one cone pointed towards the ceiling and the other to the floor. When the galvanometer indicated that the ceiling was the warmer, the balance behaved well, and *vice versâ.* The reason is of course that air is stable when the temperature increases upwards, and unstable when heat is communicated below. During the winter months the ground was usually warmer than the rest of the room, and air currents developed themselves in the weighing closet. During the summer the air cooled by contact with the ground remained as a layer below, and the balance was undisturbed.

The principal difference to be noted between my arrangements and those of Professor Cooke is that in my case no desiccators were used within the weighing closet. The general air of the room was prevented from getting too damp by means of a large blanket, occasionally removed and dried before a fire.*

In Regnault's experiments the globe was filled with gas to the atmospheric pressure (determined by an independent barometer), and the temperature was maintained at zero by a bath of ice. The use of ice is no doubt to be recommended in the case of the heavier gases; but it involves a cleaning of the globe, and therefore diminishes some-

* I can strongly recommend this method. In twenty-four hours the blanket will frequently absorb two pounds of moisture.

what the comparability of the weighings, vacuous and full, on which everything depends. Hydrogen is so light that, except perhaps in the mean of a long series, the error of weighing is likely to be more serious than the uncertainty of temperature. I have therefore contented myself with enclosing the body of the globe during the process of filling in a wooden box, into which passed the bulbs of two thermometers, reading to tenths of a degree centigrade. It seems probable that the mean of the readings represents the temperature of the gas to about $\frac{1}{10}$th degree, or at any rate that the differences of temperature on various occasions and with various gases will be given to at least this degree of accuracy. Indeed the results obtained with oxygen exclude a greater uncertainty.

Under these conditions the alternate full and empty weighings can be effected with the minimum of interference with the surface of the globe. The stalk and tap were only touched with a glove, and the body of the globe was scarcely touched at all. To make the symmetry as complete as possible, the counterpoising globe was provided with a similar case, and was carried backwards and forwards between the balance room and the laboratory exactly as was necessary for the working globe.

In my earliest experiments (1885) hydrogen and oxygen were prepared simultaneously in a U-shaped voltameter containing dilute sulphuric acid. Since the same quantity of acid can be used indefinitely, I hoped in this way to eliminate all extraneous impurity, and to obtain hydrogen contaminated only by small quantities of oxygen, and *vice versâ*. The final purification of the gases was to be effected by passing them through red-hot tubes, and subsequent desiccation with phosphoric anhydride. In a few trials I did not succeed in obtaining good hydrogen, a result which I was inclined to attribute to the inadequacy of a red heat to effect the combination of the small residue of oxygen.* Meeting this difficulty, I abandoned the method for a time, purposing to recur to it after I had obtained experience with the more usual methods of preparing the gases. In this part of the investigation my experience runs nearly parallel with that of Professor Cooke. The difficulty of getting quit of the dissolved air when, as in the ordinary preparation of hydrogen, the acid is fed in slowly at the time of working, induced me to design an apparatus whose action can be suspended by breaking an external electrical contact. It may be regarded as a Smee cell thoroughly enclosed. Two points of difference may be noted between this apparatus and that of Professor Cooke. In my manner of working it was necessary that the generator should

* From Professor Cooke's experience it appears not improbable that the impurity may have been sulphurous acid. Is it certain that in his combustions no hydrogen (towards the close largely diluted with nitrogen) escapes the action of the cupric oxide?

stand an internal vacuum. To guard more thoroughly against the penetration of external air, every cemented joint was completely covered with vaseline, and the vaseline again with water. Again, the zincs were in the form of solid sheets, closely surrounding the platinised plate on which the hydrogen was liberated, and standing in mercury. It was found far better to work these cells by their own electromotive force, without stimulation by an external battery. If the plates are close, and the contact wires thick, the evolution of gas may be made more rapid than is necessary, or indeed desirable.

Tubes, closed by drowned stopcocks, are provided, in order to allow the acid to be renewed without breaking joints; but one charge is sufficient for a set of experiments (three to five fillings), and during the whole of the time occupied (10 to 14 days) there is no access of atmospheric air. The removal of dissolved air (and other volatile impurity) proved, however, not to be so easy as had been expected, even when assisted by repeated exhaustions, with intermittent evolution of hydrogen; and the results often showed a progressive improvement in the hydrogen, even after a somewhat prolonged preliminary treatment. In subsequent experiments greater precautions will be taken.* Experience showed that good hydrogen could not thus be obtained from zinc and ordinary "pure" sulphuric acid, or phosphoric acid, without the aid of purifying agents. The best results so far have been from sulphuric and hydrochloric acid, when the gas is passed in succession over liquid potash, through powdered corrosive sublimate, and then through powdered caustic potash. All the joints of the purifying tubes are connected by fusion, and a tap separates the damp from the dry side of the apparatus. The latter includes a large and long tube charged with phosphoric anhydride, a cotton wool filter, a blow-off tube sealed with mercury until the filling is completed, besides the globe itself and the Töppler pump. A detailed description is postponed until the experiments are complete. It may be sufficient to mention that there is but one india-rubber connexion, —that between the globe and the rest of the apparatus, and that the leakage through this was usually measured by the Töppler before commencing a filling or an evacuation.

The object of giving a considerable capacity to the phosphoric tube was to provide against the danger of a too rapid passage of gas through the purifying tubes at the commencement of a filling. Suppose the gas to be blowing off, all the apparatus except the globe (and the Töppler) being at a pressure somewhat above the atmospheric. The tap between the damp and dry sides is then closed, and that into the globe is opened. The gas which now enters some-

* Spectrum analysis appears to be incapable of indicating the presence of comparatively large quantities of nitrogen.

what rapidly is thoroughly dry, having been in good contact with the phosphoric anhydride. In this way the pressure on the dry side is reduced to about 2 inches of mercury, but this residue is sufficient to allow the damp side of the apparatus to be exhausted to a still lower pressure before the tap between the two sides of the apparatus is reopened. When this is done, the first movement of the gas is retrograde; and there is no danger at any stage of imperfect purification. The generator is then re-started until the gas (after from two of five hours) begins to blow off again.

In closing the globe some precaution is required to secure that the pressure therein shall really be that measured by the barometer. The mercury seal is at some distance from, and at a lower level than, the rest of the apparatus. After removal of the mercury the flow of gas is continued for about one minute, and then the tap between the dry and damp sides is closed. From three to five minutes more were usually allowed for the complete establishment of equilibrium before the tap of the globe was turned off. Experiments on oxygen appeared to show that two minutes was sufficient. For measuring the atmospheric pressure *two* standard mercury barometers were employed.

The evacuations were effected by the Töppler to at least $\frac{1}{20000}$, so that the residual gas (at any rate after one filling with hydrogen) could be neglected.

I will now give some examples of actual results. Those in the following tables relate to gas prepared from *sulphuric* acid, with subsequent purification, as already described:—

Globe (14), empty.

Date.	Left.	Right.	Balance reading.
1887.			
Oct. 27—Nov. 5........	$G_{14}+0\cdot394$	G_{11}	22·66
Nov. 7—Nov. 8..	..	..	22·89
Nov. 9—Nov. 10.......	..	..	23·00
Nov. 11—Nov. 12......	..	..	21·72

Globe (14), full.

Date.	Left.	Right.	Balance reading.	Barometer.	Temperature.
1887.				in.	C.
Nov. 5—7..	$G_{14}+0\cdot2400$	G_{11}	20·52	29·416	14·7°
Nov. 8—9..	$G_{14}+0\cdot2364$	G_{11}	19·77	29·830	12·3
Nov. 10—11..	$G_{14}+0\cdot2360$	G_{11}	19·18	22·807	11·2
Nov. 12—14..	$G_{14}+0\cdot2340$	G_{11}	19·51	30·135	10·3

The second column shows that globe (14) and certain platinum weights were suspended from the left end of the beam, and the third column that (in this series) only the counterpoising globe (11) was hung from the right end. The fourth column gives the mean balance reading in divisions of the scale, each of which (at the time of the above experiments) represented 0·000187 gram. The degree of agreement of these numbers in the first part of the table gives an idea of the errors due to the balance, and to uncertainties in the condition of the exteriors of the globes. A minute and unsystematic correction depending upon imperfect compensation of volumes (to the extent of about 2 c.c.) need not here be regarded.

The weight of the hydrogen at each filling is deduced, whenever possible, by comparison of the "full" reading with the mean of the immediately preceding and following "empty" readings. The difference, interpreted in grams, is taken provisionally as the weight of the gas. Thus for the filling of Nov. 5—

$$H = 0{\cdot}154 - 2{\cdot}25 \times 0{\cdot}000187 = 0{\cdot}15358.$$

The weights thus obtained depend of course upon the temperature and pressure at the time of filling. Reduced to correspond with a temperature of 12°, and to a barometric height of 30 inches (but without a minute correction for varying temperature of the mercury) they stand thus—

November 5..........	0·15811
„ 8..........	0·15807
„ 10..........	0·15798
„ 12..........	0·15792
Mean......	0·15802

The hydrogen obtained hitherto with similar apparatus and purifying tubes from hydrochloric acid is not quite so light, the mean of two accordant series being 0·15812.

The weighing of oxygen is of course a much easier operation than in the case of hydrogen. The gas was prepared from chlorate of potash, and from a mixture of the chlorates of potash and soda. The discrepancies between the individual weighings were no more than might fairly be attributed to thermometric and manometric errors. The result reduced so as to correspond in all respects with the numbers for hydrogen is 2·5186.*

But before these numbers can be compared with the object of obtaining the relative densities, a correction of some importance is required, which appears to have been overlooked by Professor Cooke,

* An examination of the weights revealed no error worth taking into account at present.

as it was by Regnault. The weight of the gas is *not* to be found by merely taking the difference of the full and empty weighings, unless indeed the weighings are conducted *in vacuo*. The external volume of the globe is larger when it is full than when it is empty, and the weight of the air corresponding to this difference of volume must be *added* to the apparent weight of the gas.

By filling the globe with carefully boiled water, it is not difficult to determine experimentally the expansion per atmosphere. In the case of globe (14) it appears that under normal atmospheric conditions the quantity to be added to the apparent weights of the hydrogen and oxygen is 0·00056 gram.

The actually observed alteration of volume (regard being had to the compressibility of water) agrees very nearly with an *à priori* estimate, founded upon the theory of thin spherical elastic shells and the known properties of glass. The proportional value of the required correction, in my case about $\frac{4}{1000}$ of the weight of the hydrogen, will be for spherical globes proportional to a/t, where a is the radius of the globe, and t the thickness of the shell, or to V/W, if V be the contents, and W the weight of the glass. This ratio is nearly the same for Professor Cooke's globe and for mine; but the much greater departure of his globe from the spherical form may increase the amount of the correction which ought to be introduced.

In the estimates now to be given, which must be regarded as provisional, the apparent weight of the hydrogen is taken at 0·15804, so that the real weight is 0·15860. The weight of the same volume of oxygen under the same conditions is 2·5186 + 0·0006 = 2·5192. The ratio of these numbers is 15·884.

The ratio of densities found by Regnault was 15·964, but the greater part of the difference may well be accounted for by the omission of the correction just now considered.

In order to interpret our result as a ratio of atomic weights, we need to know accurately the ratio of atomic volumes. The number given as most probable by Mr. Scott in May, 1887,* was 1·994, but he informs me that more recent experiments under improved conditions give 1·9965. Combining this with the ratio of densities, we obtain as the ratio of atomic weights—

$$\frac{2 \times 15 \cdot 884}{1 \cdot 9965} = 15 \cdot 912.$$

It is not improbable that experiments conducted on the same lines, but with still greater precautions, may raise the final number by one or even two thousandths of its value.

The ratio obtained by Professor Cooke is 15·953; but the difference

* *Loc. cit.*

between this number and that above obtained may be more than accounted for, if I am right in my suggestion that his gas weighings require correction for the diminished buoyancy of the globe when the internal pressure is removed.

Presents, February 9, 1888.

Transactions.

Hertford:—Hertfordshire Natural History Society. Transactions. Vol. IV. Part 7. 8vo. *London* 1887. The Society.

Leipzig:—Königl. Sächsische Gesellschaft der Wissenschaften. Abhandlungen. Band XIV. Hefte 5–6. 8vo. *Leipzig* 1887. The Society.

London:—Entomological Society. Transactions. 1887. Part 4. 8vo. *London.* The Society.

Institution of Mechanical Engineers. Proceedings. 1887. No. 3. 8vo. *London.* The Institution.

Photographic Society of Great Britain. Journal and Transactions. Vol. XII. No. 3–4. 8vo. *London* 1887. The Society.

Royal Asiatic Society. Journal. Vol. XX. Part 1. 8vo. *London* 1888. The Society.

Melbourne:—Royal Geographical Society of Australasia (Victorian Branch). Proceedings. Vol. V. Part 2. 8vo. *Melbourne* 1887. The Society.

Naples:—Accademia delle Scienze Fisiche e Matematiche. Rendiconto. Anno XXV. Fasc. 4–12. 4to. *Napoli* 1886. The Academy.

Newcastle:—Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne. Natural History Transactions of N., D., and N. Vol. IX. Part 1. 8vo. *London* 1887. The Society.

North of England Institute of Mining and Mechanical Engineers. Transactions. Vol. XXXVII. Part 1. 8vo. *Newcastle-upon-Tyne* 1887. The Institute.

New York:—Academy of Sciences. Annals. Vol. IV. Nos. 1–2. 8vo. *New York* 1887; Transactions. Vol. IV. 8vo. *New York* 1887. The Academy.

Paris:—École des Hautes Études. Bibliothèque. Sciences Philologiques et Historiques. Fasc. 69–74. 8vo. *Paris* 1887. The School.

Société Astronomique de France. Statuts. 8vo. [*Paris* 1887]. The Society.

Société de Géographie. Bulletin. 1882. Trim. 2. 8vo. *Paris.* The Society.

Transactions (*continued*).

Pisa:—Società Toscana di Scienze Naturali. Processi Verbali. Vol. VI. 8vo. *Pisa* 1887–9. The Society.

Presburg:—Verein für Natur- und Heilkunde. Verhandlungen. Jahrg. 1881–6. 8vo. *Presburg* 1884, 1887. The Verein.

Rome:—R. Comitato Geologico d'Italia. Bollettino. 1887. Nos. 9–10. 8vo. *Roma* 1887. The Committee.

Rotterdam:—Bataafsch Genootschap der Proefondervindelijke Wijsbegeerte. Gedenkrede. 1787–1887. 4to. *Rotterdam* 1887. The Society.

Salem, Mass.:—Essex Institute. Bulletin. Nos. 1–12. 8vo. *Salem, Mass.* 1887; The Morse Collection of Japanese Pottery. 4to. *Salem* 1887. The Institute.

San Francisco:—California Academy of Sciences. Bulletin. Vol. II. No. 7. 8vo. *San Francisco* 1887. The Academy.

Tōkyō:—College of Science, Imperial University, Japan. Journal. Vol. I. Part 4. Sm. 4to. *Tōkyō* 1887. The College.

Seismological Society of Japan. Transactions. Vol. XI. 1887. 8vo. *Yokohama.* The Society.

Turin:—R. Accademia delle Scienze. Atti. Vol. XXII. Disp. 14–15. Vol. XXIII. Disp. 1. 8vo. *Torino* 1887. The Academy.

Utrecht:—Laboratorium der Utrechtsche Hoogeschool. Onderzoekingen gedaan in het Physiologisch Laboratorium. Reeks 3. Deel. X. Stuk 2. 8vo. *Utrecht* 1887. The University.

Vienna:—Anthropologische Gesellschaft. Mittheilungen. Bd. XV. Heft 4. 4to. *Wien* 1885. Bd. XVII. Hefte 2–4. 4to. *Wien* 1887. The Society.

K. Akademie der Wissenschaften. Anzeiger. Jahrg. 1887. Nr. 15–25. 8vo. [*Wien*]. The Academy.

K. K. Geologische Reichsanstalt. Jahrbuch. Jahrg. 1887. Heft 1. 8vo. *Wien;* Verhandlungen. 1887. Nos. 2–8. 8vo. *Wien.* The Institute.

K. K. Historhistorisches Hofmuseum. Annalen. Bd. II. Nr. 4. 8vo. *Wien* 1887. The Museum.

K. K. Zoologisch-Botanische Gesellschaft. Verhandlungen. Jahrg. 1887. Hefte 3–4. 8vo. *Wien.* The Society.

Warwick:—Warwickshire Naturalists' and Archæologists' Club. Proceedings. 1886. 8vo. *Warwick.* The Club.

Würzburg:—Physikalisch-Medicinische Gesellschaft. Verhandlungen. Bd. XX. 8vo. *Würzburg* 1887. The Society.

Yokohama:—Asiatic Society of Japan. Transactions. Vol. XV. Part 1. 8vo. *Yokohama* 1887. The Society.

Zürich:—Naturforschende Gesellschaft. Vierteljahrschrift. Jahrg.

Transactions (*continued*).

31. Hefte 3-4. Jahrg. 32. Heft 1. 8vo. *Zürich* 1886-7; Neujahrsblatt. Jahrg. 1880-81, 1887. Sm. 4to. *Zürich.*
The Society.

Observations and Reports.

Adelaide:—Public Library, Museum, and Art Gallery of South Australia. Report 1886-7. Folio. *Adelaide* 1887.
The Trustees.

Brussels:—Observatoire. Annuaire. 1888. 12mo. *Bruxelles* 1887.
The Observatory.

Buenos Ayres:—Primer Censo de la Provincia de Santa-Fé. Folio. *Buenos Aires* 1887.
Oficina Nacional de Publicaciones.

India:—Meteorological Observations recorded at Six Stations in India. 1887. June—July. Folio. [*Calcutta.*]
The Meteorological Office, India.

Trigonometrical Branch, Survey of India. Spirit-Levelled Heights. No. 2. Madras Presidency. Season 1885-86. 8vo. *Dehra Dun* 1887. The Survey.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. No. 13. 8vo. *London* 1888. The Director.

Melbourne:—Monthly Record taken at the Melbourne Observatory. July—August, 1887. 8vo. *Melbourne.* The Observatory.

Public Library, Museums, and National Gallery of Victoria. Report, 1886. 8vo. *Melbourne* 1887. The Trustees.

Milan:—Reale Osservatorio di Brera. Pubblicazioni. No. XXVII. 4to. *Milano* 1885. The Observatory.

New South Wales:—Department of Mines. Geology of the Vegetable Creek Tin-Mining Field. 4to. *Sydney* 1887; Annual Report of the Department. 1886. Folio. *Sydney* 1887.
The Department.

Legislative Assembly. Report on Technical Education. Folio. *Sydney* 1887. Board of Technical Education, N.S.W.

Paris:—Bureau des Longitudes. Annuaire pour l'an 1885. 12mo. *Paris.* Société de Géographie, Paris.

Rome:—Pontificia Università Gregoriana. Continuazione del Bullettino Meteorologico. Vol. XXVI. Num. 8. 4to. *Roma* 1887. The University.

St. Petersburg:—(Meteorological Observations made in Ships of the Russian Fleet.) Nos. 52-54. 8vo. *St. Petersburg* 1887.
The Meteorological Office, London.

Salford:—Museum, &c. Report 1886-7. 8vo. *Salford.*
The Committee.

Observations, &c. (*continued*).

Wellington:—Reports on the Mining Industries of New Zealand. Folio. *Wellington* 1887; Statistics of the Colony of New Zealand. Folio. *Wellington* 1887.

The Government of New Zealand.

Babinet (M.) The Diamond and other Precious Stones. 8vo. *Washington* 1872. The Smithsonian Institution.

Henshaw (S.) The Entomological Writings of Dr. A. S. Packard. 8vo. *Washington* 1887. Dr. Packard.

Langley (S. P.), C. A. Young, and E. C. Pickering. Pritchard's Wedge Photometer. 4to. [1887.] The Authors.

Packard (A. S.) On the Syncarida. 4to. [1887]; On the Carboniferous Xiphosurous Fauna of North America. 4to. [1887.] The Author.

Pickering (E. C.) Observations of Variable Stars in 1886. 8vo. [*Philadelphia* 1887.] The Author.

Wolf (R.) Astronomische Mittheilungen. No. 70. 8vo. *Zürich* 1887. Dr. Wolf.

February 16, 1888.

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

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

The following Papers were read:—

I. "Note on the Changes effected by Digestion on Fibrinogen and Fibrin." By L. C. WOOLDRIDGE, M.D., D.Sc., Assistant Physician to Guy's Hospital. Communicated by Professor VICTOR HORSLEY, F.R.S. (From the Laboratory of the Brown Institution.) Received January 27, 1888.

Fibrinogen is a name conveniently given to a group of proteïd substances which can all be converted under certain conditions into fibrin. They exist in blood plasma (in traces in certain kinds of blood serum), and they can be prepared from almost all animal tissues (thymus, testis, brain, liver, kidney, stroma of red corpuscles, &c.).

They differ slightly in their behaviour towards various reagents, but they have a number of characters in common. They are all extremely easily changed by precipitation. In their normal condition they are easily soluble in water ("soluble" as caseïn is dissolved in milk). They are readily precipitated by acetic acid in excess, and dilute mineral acids in slight excess. If more dilute acid be added they are re-dissolved.*

On adding pepsin to the acid solution and maintaining the mixture at a temperature of 37° for some hours, a very marked precipitate makes its appearance. This precipitate is not dissolved if the artificial digestion be continued for many days. Freshly formed, it is easily soluble in dilute alkalis, but not in dilute acids. It dissolves in strong nitric acid, giving a yellow or yellowish-green solution, which gives a marked xanthoproteic reaction on warming and adding ammonia. On incinerating it leaves a markedly acid ash. If it be burnt with a little soda and saltpetre the ash is found to be extremely rich in phosphoric acid.

The phosphorus is present in the form of lecithin.

The alcoholic extract of even very small quantities of the precipitate contains relatively very large quantities of lecithin.

After complete extraction with alcohol there is either no phosphorus

* It is to be noted that fibrinogens are easily changed in this respect by precipitation.

at all in the ash, or a very dubious trace, due probably to imperfect extraction.

The ash always contains iron, and the iron is not removed from the precipitate by extraction with alcohol containing hydrochloric acid. This description applies equally to the fibrinogen obtained from the tissues and the fibrinogens present in blood.*

Under appropriate conditions these fibrinogens can be entirely converted into fibrin. The fibrin always contains lecithin; but fibrin differs from the fibrinogens from which it is formed by being absolutely and entirely soluble in artificial gastric juice. This remarkable difference in the behaviour of the two classes of substances towards artificial gastric juice is considered by the author as strong evidence that the relation between the lecithin and the proteïd which both bodies contain must be different in the two cases.

That lecithin was a very important factor in coagulation was shown by the author many years ago, and this fact has been fully confirmed by pupils of Alexander Schmidt (Nauck, Samson-Himmelstjerna, Krüger).

Ordinary fibrin obtained by whipping blood always leaves an undigested residue, due partly to the presence of admixed white corpuscles (Hammarsten), partly, however, to its containing unchanged fibrinogen. Fibrin obtained from pure fibrinogen fluids by artificially induced coagulation is always completely digestible if care be taken that it contains no unchanged fibrinogen. The fibrin obtained by the action of ferment on fibrinogen is always completely digestible (Hammarsten).

II. "A new Method for determining the Number of Micro-organisms in Air." By Professor CARNELLEY, D.Sc., and THOS. WILSON, University College, Dundee. Communicated by Sir HENRY ROSCOE, F.R.S. Received February 3, 1888.

(Abstract.)

This is a modification of Hesse's well-known process. It consists essentially in the substitution of a flat-bottomed conical flask for a Hesse's tube. Its chief advantages are:—(1.) Much smaller cost of flask and fittings as compared with Hesse's tubes; (2.) Very many fewer breakages during sterilisation; (3.) Great economy in jelly; (4.) Freedom from leakage during sterilisation; (5.) Results not vitiated by aërial currents.

* The presence of iron in an organic form in blood plasma was described by the author in the Arris and Gale Lectures, delivered before the Royal College of Surgeons in 1886. Pamphlet, 1886.

III. "Note on the Number of Micro-organisms in Moorland Air." By Professor CARNELLEY, D.Sc., and THOS. WILSON, University College, Dundee. Communicated by Sir HENRY ROSCOE, F.R.S. Received February 3, 1888.

As no determinations appear to have been made of the number of micro-organisms in moorland air, the following results obtained last summer may be of interest as forming a small contribution to our knowledge of the distribution of micro-organisms in the air of different localities.

Our determinations were made "on the heather" in the neighbourhood of Midtown, in the parish of Tanadice, in Forfarshire. This forms a part of the Clova district, so well known to botanists as the habitat of many rare mountain and moorland plants. Midtown is situated at a height of about 1000 feet above the level of the sea, and is far removed from towns and other sources of contamination, as is evidenced by the fact that the nearest railway is about six miles away.

We also made simultaneous estimations of the carbonic acid by Pettenkofer's method. The process employed for the determination of the micro-organisms was the "flask method," which we have already described in a previous paper. The samples of air were taken at a height of about 3 feet from the ground. The results obtained are represented in the following table. 10 litres of air were aspirated in each case. More determinations would have been made, but owing to an accident the remainder of the flasks were unfortunately spoilt.

No.	Date.	Time.	Weather.	Wind.	Temperature.	Carbonic acid vols. per 10,000.	Micro-organisms per 10 litres of air.		
							Bacteria.	Moulds.	Total.
	1887.								
1	Aug. 3rd	5 p.m. ...	Bright sunshine, no clouds	Moderate, S.E.	63° F.	4·2	0	8	8
2	,, 5th	9.30 a.m.	Bright sunshine, cloudy	Strong, S.........	70	4·0	0	2	2
3	,, 8th	9.20 a.m.	Cloudy	Gale, W.	70	4·0	0	4	4
4	,, 9th	2 p.m. ...	,, ...	Strong, W.	65	3·3	0	0	0
5	,, 15th	10 a.m....	Bright sunshine, few clouds	Gentle, W.	58	3·6	0	0	0
6	,, 19th	9.30 a.m.	Cloudy	Moderate, N.W.	58	4·3	0	7	7

The weather had been fine and dry for a long time previous to August 9th, but between that and the 15th there were several days of rain.

It will thus be seen that *not a single sample contained bacteria, and that all the micro-organisms obtained consisted of moulds, amounting on the average to* 3·5 *per litre.*

Now Miquel and Dr. P. Frankland have each shown that the air is much richer in micro-organisms during the summer than during the winter, there being a minimum about midwinter and a maximum about July and August, thus :—

	Miquel.		P. Frankland.
	Montsouris.	Paris.	South Kensington.
Winter (Dec., Jan., Feb.)	2·0	21·3	12
Spring (March, April, May) ..	5·0	47·9	29
Summer (June, July, Aug.) ..	6·4	50·5	74
Autumn (Sept., Oct., Nov.) ..	4·8	37·0	30

It hence follows that the number of moulds we found in moorland air was probably a maximum, since the determinations were made in August, and that bacteria are absent all the year round in pure air from moors and hills away from towns.

In order to give an idea of the number of micro-organisms in moorland air as compared with air from other localities, the following table is appended, more especially as many of these data are not generally accessible to chemists :—

Place.	Season of year.	Micro-organisms per 10 litres of air. Bacteria.	Moulds.	Total.	Authority.
Sea air (Atlantic Ocean)	—	0	0	0	Dennis.
High mountains	?	..	..	·01	Miquel.
Moorland	August	0	3·5	3·5	Carnelley and Wilson.
Country	Annual average	..	..	4·6	Miquel.
	Autumn	..	..	6·8	
,,	August	..	..	14·0	P. Frankland.
Dundee, suburbs	Winter	0	1	1·0	Carnelley and Haldane.
,, town, open places, night	,,	1·5	0	1·5	,, ,,
,, ,, close places, ,,	,,	5·0	0·5	5·5	,, ,,
,, ,, open places, day	,,	7	5	12	,, ,,
,, ,, ,, ,,	Spring	12·8	0	13	Carnelley and Wilson.
Norwich, Cathedral Close	?	..	..	18	P. Frankland.
Paris, Rue de Rivoli	Annual average	..	..	39	Miquel.
	August	..	..	44	
London, open places	?	..	..	24	P. Frankland.
,, roof of Science Schools, South Kensington	Annual average	..	..	36	,,
	August	..	..	105	
London, St. Paul's Churchyard	?	..	..	70	,,
Sewers (Paris)	?	..	..	9	Miquel.
,, (Bristol)	Summer	..	..	20*	Haldane.
,, (Westminster and Dundee)	Spring	79	10	89*	Carnelley and Haldane.
Royal Infirmary, Dundee	Winter	42	17	59	,, ,,
Hospital for Consumption, Brompton	?	..	..	72	P. Frankland.
Hôpital de la Pitié, Paris	Annual average	..	..	96	Miquel.
Bedrooms at night: Three and more roomed houses, Dundee	Winter	85	5	90	Carnelley and Haldane.
Bedrooms at night: Two ,, ,, ,,	,,	434	26	460	,, ,,
Bedrooms at night: One ,, ,, ,,	,,	585	15	600	,, ,,
Naturally ventilated schools, Dundee	,,	160	6	166	,, ,,
Mechanically ,, ,, ,,	,,	1510	10	1520	,, ,,
Jute Mills, Dundee	,,	1140	460	1600	,, ,,

* Outside air at same time in streets above the sewers and at a height of 3 feet from the ground gave 130 at Bristol and 159 per 10 litres at Westminster and Dundee. The better a sewer is ventilated the larger is the number of micro-organisms present in the air of the sewer, since all, or by far the greater part of them, come from the outside air (compare Carnelley and Haldane, 'Roy. Soc. Proc.,' vol. 42, p. 501).

N.B.—With the exception of those of Miquel, all the determinations given in the table were made with a solid cultivating medium, viz., Koch's gelatine-peptone.

IV. "On the possibly Dual Origin of the Mammalia." By St. George Mivart, M.D., F.R.S. Received February 14, 1888.

The recent discovery by Mr. Edward B. Poulton of non-functional teeth hidden beneath the bony plates of the jaws of the young Ornithorhynchus is not only most interesting in itself, but taken in connexion with another recent discovery as to the anatomy of that animal, exceedingly suggestive. It is, of course, easy to assign too great a value to the forms of teeth, and everyone knows how Cuvier was thus led to associate the marsupial Carnivora with the placental Carnivores. There is an evident temptation also to exaggerate the significance of dental structure, both on account of the obvious nature of such characters and also because they are so exceptionally well preserved in fossil remains. But no zoologist can deny that the value of dental characters is often exceedingly great, and when, as in the case of *Ornithorhynchus*, we have them in the form of living fossils, as it were, entombed within the jaws, we may fairly presume that they show us what their shape was when they were last in actual use, and so must possess a greater or less taxonomic value. The most valuable evidences of affinity are commonly afforded by structures less distinctly related to habits of life. Thus, for example, the course taken by the internal carotid artery has often a more profound significance than has either the structure of the teeth or shape of the limbs; while the possession by any two animals of a prehensile tail —in spite of the niceties of structure which concur to produce it— cannot alone be accepted as a test that they belong even to the same order.

The shape of the teeth, having a manifest direct relation to conditions of life, requires, then, a very careful criticism before any evidence it may seem to afford can be relied on as a test of affinity.

The *Ornithodelphia* (*Ornithorhynchus* and *Echidna*) have long been known to possess characters resembling the *Sauropsida* and especially the Lacertilian Reptilia. Nevertheless, no less distinguished an anatomist than Professor Huxley has, so late as 1880, regarded them as descendants (through imaginary creatures called *Hypotheria*) from amphibians and not from any of the *Sauropsida*;* a view which I myself have also held.

The most interesting discovery by Mr. Caldwell of the eggs of *Ornithodelphia*, the announcement of which startled the meeting of the British Association in Canada, greatly strengthened the evidence previously relied on by certain naturalists, that the Ornithodelphia descended from some Reptilian form, and this view seems to have

* 'Zool. Soc. Proc.,' 1880, p. 662.

met with general acceptance, and it is similarly supposed that all other mammals must have followed the same route and must therefore also be descendants of some early reptile-like creature.

The question, therefore, of this resemblance or non-resemblance of the Ornithorhynchus teeth with any known reptilian teeth becomes a question of much interest. The author of the recent communication, Mr. Poulton, affirmed that the teeth were distinctly mammalian teeth.*

I have long believed that no such teeth were to be found in any of the Sauropsida, and the conviction I previously entertained has been confirmed by a recent re-examination (*ad hoc*) of the dentition of Reptiles extant and extinct, preserved in the National Collection; and I here desire to express my warm thanks to Mr. G. A. Boulenger and Mr. Lydekker for the very kind and ready help I have received from them.

The results of my examination may be summed up as follows:—

The Sauropsidan tooth, from the lowest reptiles to *Hesperornis*, may be described in general terms as a subconical structure in which subsidiary additions or modifications may arise, which, however, never cause it to resemble a mammalian molar—except, of course, such exceptional mammalian molars as are themselves mere dental cones—or to resemble the mandibular tooth described by Mr. Poulton as existing in *Ornithorhynchus*. That tooth was said to present the following characters:—Towards its outer edge were two large cusps, one in front of the other, and opposite to them were four or five very small ones extending from behind forwards along the inner edge of the tooth. The tooth above it was said to be conversely constructed, so that the two interlocked, the greater prominence of the upper tooth being towards its internal edge.

Nothing of this kind exists in any reptile. In reptiles the dental cone may be laterally compressed and serrated at its margin, as in *Megalosaurus;* it may be less laterally compressed but serrated and furnished with vertical prominences, as in *Iguanodon*. From this we find transitions to the tricuspid tooth of *Cyclura*, and the summit is subdivided into two or three cusps in a multitude of existing lizards, while it may assume the form of a *fleur-de-lys* as in *Amblyrhynchus*. Very rarely (only in *Teius* and *Dicrodon*) there may be a supplementary prominence on one side, which may attain to within a short distance of the height of the main cone and thus present the appearance of a single cone with a deep antero-posteriorly directed groove. Finally, as in *Empedias*† there may be a central prominence

* "The teeth probably represent some part of the molar series in the higher mammals."

† The *Empedocles molaris* of Cope (see 'Amer. Phil. Soc. Proc.,' vol. 19, p. 47). The specimens in our national collection are also thus labelled.

(which appears to become much worn down by use) with a small accessory prominence both on the inner and the outer side of the central one. As every one knows, reptilian teeth may become obtuse rounded structures as in *Cyclodus* and *Ada*, or almost quite flattened as in the curious extinct reptiles *Lepidotus* and *Placodus*. The *Theriodontia** offer examples of teeth more or less like the incisors and canines of mammals, but exhibit no grinding molar, the subdivisions of the summits of their molar teeth sometimes, however, reminding us of the tricuspid molars so common in existing Lacertilians.

Such being the negative evidence with respect to the molar teeth of the Sauropsida, I availed myself of the kind assistance of Mr. Oldfield Thomas, F.Z.S., in an endeavour to find amongst mammals teeth like those described as existing in the Ornithorhynchus. Although various forms were seen to present slight resemblances, we failed to obtain any which could be said to bear an unquestionable likeness to them.

The ancestors of the Ornithorhynchus which had functional teeth, must, according to the ordinarily received doctrine of evolution, have had a general bodily organisation at least as Sauropsidan as that of the existing *Ornithodelphia*. How far back in geological time that tooth structure existed, we have as yet no evidence; but we have abundant evidence that a dentition much like that of some existing Marsupials already existed during the deposition of the Oolite strata. Professor Huxley has expressed† his expectation that generalised ancestors of the Monotremes may be found amongst the remains "of the terrestrial Vertebrates of the later Palæozoic epochs."

The toothed ancestor of the Ornithorhynchus, however, could I think hardly have been extant at so extremely distant an epoch; for then its resemblance in other respects to the *Lacertilia* would make it probable that it had a pretty close connexion with the stem of the Sauropsidan tribe. But a connexion so low down seems unlikely, now that we are acquainted with its tooth-structure; since amongst the multitude of numerous Sauropsidan species living and extinct, there is not one which has inherited a tooth at all like that of the Ornithorhynchus, but the teeth of every one such species is, as above stated, formed upon a fundamentally different type; this could hardly be the case if the Ornithorhynchus tooth was derived from some archaic form whence the Sauropsida, or any considerable section of them, were also derived. But this tooth if not derived from a non-mammalian animal, must either have been derived from some one amongst the

* See Owen's 'Descriptive and Illustrated Catalogue of the Fossil Reptilia of South Africa in the British Museum,' 1876, p. 15.

† 'Zool. Soc. Proc.,' 1880, p. 658.

earliest mammals which first had teeth of the mammalian type, or have arisen independently.

Let us first briefly consider the former alternative ; such a mammalian ancestor must, on the generally received doctrine of evolution, have had its general organisation like that of an existing Monotreme, or have been formed on a yet lower type. In either case if all mammals furnished with grinding teeth have also proceeded from such early root form, it is remarkable that none of its descendants save the Monotremes have inherited those skeletal, cerebral and genito-urinary peculiarities which characterise the *Ornithodelphia*, and which, on this hypothesis, must also have been possessed by the various ancestors of the different orders of non-monotrematous mammals. In that case, the creatures which came to form all these orders must have simultaneously and persistently varied in a single direction, resulting in that one very definite form of organisation which is common to the placental and marsupial mammals. But this will probably be considered an all but utterly inadmissible supposition.

If, however, the Ornithorhynchus tooth arose in some much less primitive mammal, one which was previously edentulous or had but Sauropsidan teeth, and therefore was not also the progenitor of all the other mammals with grinding teeth, then such teeth must have twice arisen independently, and there seems, on this view, no reason to repudiate the other alternative, namely, that the Ornithorhynchus teeth might have arisen independently, in relatively modern times, in what may have been no very remote ancestor of the Ornithorhynchus itself. In that case, however, the wonder remains that the Monotremes should have retained so many Sauropsida-like features which all other mammals have entirely lost.

The question then presents itself, is it possible that the Monotremes may be instances of degradation ; that they inherit their teeth from early but ordinary toothed mammals, while their shoulder-structure, rudimentary corpus callosum, and genito-urinary peculiarities are due to degradation and reversion ? It is now considered by some naturalists that the *Amphioxus* and the *Tunicates* are extremely degraded Vertebrates.

When we recall to mind such instances amongst the Invertebrata as *Lerneocera* and *Sacculina*, any amount of degradation seems possible. As to the corpus callosum, considerable differences exist amongst the Placentalia, and it is difficult to see why it might not sometimes shrink as well as augment, and we must admit that the optic chiasma has disappeared in Teleostean fishes, if they had, as would be generally admitted, either Ganoid-like or Elasmobranch-like ancestors. A cloaca is absent in mammals which are not Monotremes, yet such a structure, though very shallow, has reappeared in Rodents and Edentates (Beaver and Sloth). The penis is strangely modified, but the production of the mouth of the cloaca of the female eft, *Euproctus*, into an intromittent organ is also startling, and even amongst mammals, the female of the spotted hyæna with its enormous clitoris, perforated by the urethra, is wonderfully different from that of the striped hyæna, otherwise so nearly resembling it in structure. The disconnexion of the ureters with the bladder is a very important difference, certainly, but even in placental mammals those ducts shift their position greatly, as may be seen if we compare *Sorex* with *Hyrax*.

Moreover, it must be admitted that if the Monotremes had remote Sauropsidan ancestors (as can hardly, I think, now be questioned) then more or less of epicoracoids, interclavicles, &c., must have been "in their blood," so that reversion is conceivable. Nevertheless, I am far from believing that such a reversion has actually taken place. Granted that degradation frequently occurs, yet it would hardly, I think, get so completely on the old lines again. There is, however, I venture to believe, another less improbable hypothesis which I will now venture to suggest. It is the hypothesis that the Monotremes come from a radically distinct stock from that whence all other mammals proceeded; that the Monotremes are an example of hypothetical higher mammals in the making, the future evolution of which may probably be hindered by man's presence, but which, did they appear, would produce mammalian forms more or less parallel to but, of course, radically distinct from, the placental and marsupial series of mammals. The latter series of mammals—the superior mammals—may still be supposed to have arisen from Amphibia-like root forms, according to the position defended by Professor Huxley, for which I think there is a great deal to be said. The Monotremes, or inferior mammals, on the other hand, must, I think, be supposed to be derived from Sauropsidan ancestors, and according to this view the resemblances which exist between these higher and lower kinds of mammals, including tooth structure, will be induced resemblances—the two groups having grown alike through the independent origin of similar structures.

What evidence is there that the *Amphioxus* is a degraded animal? What principle of evolution need hinder us from regarding it as a possible parent of another line of Vertebrates profoundly different from the Vertebrates which have come into being? Each of these suppositions is alike hypothetical, and a number of similar dilemmas may be suggested in cases more or less parallel.

With regard to the Monotremes, however, we have a very solid reason for regarding them as mammals which have arisen from another root from the higher (placental and marsupial) Mammalia, namely, the fundamental difference which, according to Professor Gegenbaur, exists between their mammary glands and the mammary glands of other mammals,* the one being formed from modified sweat glands, and the other from sebaceous follicles. If this distinction is found to hold good throughout the class, it seems to me difficult to think that the Mammalia had not this dual origin—an hypothesis which harmonises so well with the differences, skeletal, genito-urinary, and developmental, which divide these two groups of mammals.

On this view, the teeth of the toothed Ornithorhynchus ancestor must have arisen for the first time in a form more reptilian than is the form of our living Monotremes, yet sufficiently divergent from the Sauropsidan main stem to explain the non-existence of teeth of the kind in any known Sauropsidan, living or fossil.

To this hypothesis it will probably be at once objected, that Mr. Caldwell's† studies of the mammalian ova show a noteworthy resemblance between those of the Marsupials and Monotremes. But if the Marsupials are an offshoot from the placental mammals, then such resemblances as exist between them and Monotremes in this respect must be induced resemblances. Moreover, certain very noteworthy resemblances exist between the ova of those exceptional Amphibians, the *Ophiomorpha*, and Sauropsidan ova.‡ It may be objected in the second place that the dual hypothesis implies the independent origin of too many similar structures. But the independent origin of similar structures is a doctrine for which I have combated ever since the year 1869. I say "similar," not "identical." No two leaves in a forest are *absolutely* alike; how then could absolute resemblance be thought possible between two structures of different origin? Yet the closeness of resemblances between parts which must have arisen diversely is often remarkable. The Marsupials are now regarded as having diverged from the mammalian stem by some single remote ancestor. Yet amongst its descendants have arisen animals some of the teeth of which strikingly resemble some of the teeth of beasts of the placental series. Some teeth of *Perameles* and *Urotrichus*, of *Macropus* and *Macroscelides*, of *Thylacinus* and of *Canis*, may be cited as examples; and though the histological difference of the extension of dentinal tubes into the enamel generally obtains in the Marsupials, yet it is more marked in the Kangaroos, which are the most differentiated forms, while such tubes almost or quite vanish in the *Dasyuridæ*, which more nearly resemble ordinary mammals. But the most striking similarity of tooth structure is that between *Orycteropus* and *Myliobates*—a similarity which extends over the microscopic characters. Again, it would be difficult to find a more curious practical resemblance than that between the hinge teeth of *Lophius*, the

* See his 'Zur Kenntniss der Mammarorgane der Monotremen,' 1886.

† 'Phil. Trans.,' B, vol. 178 (1887) p. 463.

‡ See the account of the ova of *Ichthyophis glutinosus* in C. and P. Sarasin's 'Ergebnisse Naturwiss. Forschungen auf Ceylon,' vol. 2, 1887, p. 11.

Pike, and certain fishes yet undescribed. The poison fangs of Serpents have also arisen independently, as is certain when we compare the fang of *Atractaspis* with that of *Vipera*; quite independently also have arisen the poison teeth of *Heloderma*. The scrotum of placentals and the singularly placed scrotum of marsupials (so difficult to explain either by "natural" or "sexual" selection) must also have had a dual origin, as the prehensile pes of *Didelphys* and of the Apes has also doubtless had. For my own part I am still disposed to maintain the probability, which I long ago asserted, of the independent origin of the *Simiadæ* and the *Cebidæ*, and now Professor Cope brings forward* noteworthy reasons for believing that the Horse of America and the Horse of Europe have had a widely distinct ancestry, and have grown alike from two distinct lines of descent. Finally I would refer to the similar forms of placenta, both umbilical and allantoic, which seem to have arisen independently, as also have the mammary glands of Monotremes and other mammals. Any one who is disposed to think incredible the independent origin of a mammalian molar in a diverging offshoot from the Sauropsidan tree, I would ask to bear in mind the multitude of origins which we must regard as independent, and often as quite geologically modern. Among them I would enumerate the dentition of *Desmodus*, *Diphylla*, and *Cheiromys*, and especially the very remarkable multicuspidate canines of a Pteropine bat (*Pteralopex atrata*) recently described† by Mr. Oldfield Thomas. What again can be more singular than the wonderful dental divergence between the Narwhall and the Beluga, otherwise so extremely alike in structure? The poison teeth and, as we shall soon learn, the poison gland and ducts of *Heloderma*, before referred to, are also most noteworthy. Again, what is more startling than to find the respiratory tail of the young *Hylodes* and the respiratory ventral folds of *Rana opisthodon*?‡ The tip of the snout of the young of this animal reminds us of the beak of the unhatched chick, though there can be doubt but that these structures have arisen independently. The development of this Batrachian recalls to mind the similarity of condition of the Axolotl, the larvæ of *Triton alpestris*, and the so-called Perennibranchiate Batrachians, all of which seem to have acquired a normal or permanent condition of life resembling that of immature stages in the existence of their several ancestors.

Mr. Boulenger has been kind enough to inform me of another case of the sudden origin of a new character—probably a reversion—which he has noticed in a Lizard, a species of *Gymnophthalmus*. Here normally the tail is clothed with scales, quincuncially disposed, as in the

* See 'American Naturalist' for December, 1887.

† See 'Ann. Mag. Nat. Hist.,' vol. 1, 1888, p. 155.

‡ See Mr. Boulenger's paper on the reptiles and batrachians of the Solomon Islands, 'Zool. Soc. Trans.,' vol. 12, p. 51.

Scincs. When the tail has been broken, however, it is reproduced with an investment of scales arranged in a verticillate manner—a change which shows how small is the real value of a difference which has been deemed by morphologists to be so important a taxonomic character. And here I would venture to make another observation bearing upon taxonomy. The study of the processes of individual development are of course of great importance in determining the nature of the adult animal. Nevertheless that importance may be exaggerated. *Rana opisthodon* is no less a *Rana* because it is never a Tadpole. The outcome of the process of development is surely as important as the process itself. Similarly with respect to the evolution of species, the lines of descent are of the highest interest, but if Professor Cope is right as to the diverse ancestry of the oriental and occidental *Equus*, then surely its importance may be exaggerated also. The genus *Equus* is no less one genus for having arrived at maturity along two distinct routes. It seems to me probable that various other natural groups, which are commonly regarded, and I think truly regarded, as natural unities, have become one from various sources. Should this view become generally recognised, it seems to me that the idea of the tree of life will not serve as a basis of a really satisfactory system of classification. Certainly no system could be regarded as satisfactory or natural which placed in widely different groups the two kinds of Horse referred to.

In concluding, I beg leave to repeat my assertion, that all the teeth of the Ornithorhynchus are unlike any known Sauropsidan teeth, while nevertheless the totality of the structure of Monotremes, and especially the nature of their mammary gland, lend support to the hypothesis that they have become mammals along a different road from that which the higher Mammalia have travelled, and that they gained their teeth by the way, after they had separated off from the main Reptilian stem. This difference of origin nevertheless constitutes in my eyes no reason whatever for not regarding Monotremes and higher Mammals as being all true members of the one class Mammalia.

Presents, February 16, 1888.

Transactions.

Bologna:—Reale Accademia delle Scienze. Memorie. Ser. 4. Tomo VII. 4to. *Bologna* 1886. — The Academy.

Heidelberg:—Universität. Inaugural-Dissertationen, &c., 1887. 8vo. and 4to. *Heidelberg, &c.* — The University.

Kiel:—Universität. Inaugural-Dissertationen, &c., 1887. 8vo. and 4to. *Kiel.* — The University.

London:—Institution of Mechanical Engineers. Proceedings. 1887. No. 4. 8vo. *London.* — The Institution.

Transactions (*continued*).

London Mathematical Society. Proceedings. Vol. XIX. Nos. 305–7. 8vo. *London* 1887. The Society.

Odontological Society of Great Britain. Transactions. Vol. XX. No. 3. 8vo. *London* 1888. The Society.

Royal Institute of British Architects. Journal of Proceedings. Vol. IV. No. 7. 4to. *London* 1887. The Institute.

Society of Antiquaries. Proceedings. Vol. XI. No. 4. 8vo. *London* 1887. The Society.

Louvain:—Université Catholique. Annuaire, 1888. 12mo. *Louvain;* Thèses de la Faculté de Théologie, 1886–87. 8vo. *Louvain.* The University.

Montpellier:—Académie des Sciences et Lettres. Mémoires. (Section des Lettres.) Tome VIII. Fasc. 1. 4to. *Montpellier* 1887; Ditto (Section des Sciences). Tome XI. Fasc. 1. 4to. *Montpellier* 1887. The Academy.

New York:—American Geographical Society. Bulletin. Vol. XIX. No. 4. 8vo. *New York* 1887. The Society.

Paris:—Société Géologique de France. Bulletin. Sér. 3. Tome XV. Nos. 6–8. 8vo. *Paris* 1887. The Society.

Rostock:—Universität. Inaugural-Dissertationen, &c., 1887. 8vo. and 4to. *Rostock, &c.* The University.

Toronto:—Canadian Institute. Proceedings. Vol. V. Fasc. 1. 8vo. *Toronto* 1887. The Institute.

Washington:—Surgeon-General's Office. Index-Catalogue of the Library. Vol. VIII. Lar. 8vo. *Washington* 1887. The Office.

Observations and Reports.

Calcutta:—Meteorological Observations made at Six Stations in India. August, 1887. Folio [*Calcutta*]. The Meteorological Office, India.

Dorpat:—Observatory. Meteorologische Beobachtungen. Juni—September, 1887. 8vo. *Dorpat.* The Observatory.

Dublin:—Dunsink Observatory. Astronomical Observations and Researches. Part 6. 4to. *Dublin* 1887. The Observatory.

India:—Great Trigonometrical Survey of India. Synopsis of Results. Vol. VIIA. 4to. *Dehra Dun* 1887. The Survey.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. No. 14. 8vo. *London* 1887. The Director.

Milan:—Osservatorio di Brera. Pubblicazioni. No. 30. 4to. *Milano* 1887. The Observatory.

Paris:—Bureau des Longitudes. Annuaire pour l'An 1888. 12mo. *Paris.* The Bureau.

Observations, &c. (*continued*).

Service Hydrographique de la Marine. Annales Hydrographiques. No. 696. 8vo. *Paris* 1887; Fifteen Charts, 1887.
Dépôt de la Marine.

Stonyhurst:—College Observatory. Results of Observations, 1886. 8vo. *Market Weighton* 1887. The Rev. S. J. Perry, F.R.S.

Sydney:—Australian Museum. Report, 1886, with Supplement. Folio. *Sydney* 1887. The Trustees.

Observatory. Results of Rain and River Observations made in New South Wales and part of Queensland, 1886. By H. C. Russell. 8vo. *Sydney* 1887. The Observatory.

Tacubaya:—Observatorio Astronómico Nacional. Anuario. 1888. 12mo. *México* 1887. The Observatory.

Toronto:—Meteorological Service of the Dominion of Canada. Report. 1884. 8vo. *Ottawa* 1887. The Superintendent.

Washington:—Bureau of Navigation. The American Ephemeris and Nautical Almanac for 1884. 8vo. *Washington.*
The Bureau.

Journals.

Architects' Register (The) Vol. II. 8vo. *London* 1887.
The Editor.

Canadian Record of Science. Vol. III. No. 1. 8vo. *Montreal* 1888.
The Natural History Society of Montreal.

Fisherman's Nautical Almanac (The) for 1888. By O. T. Olsen. 8vo. *Great Grimsby.* Mr. Olsen.

Horological Journal. Vol. XXX. No. 354. 8vo. *London* 1888.
The British Horological Institute.

Mittheilungen aus der Zoologischen Station zu Neapel. Bd. VII. Hefte 3–4. 8vo. *Berlin* 1887. Dr. Dohrn.

Naturalist (The) No. 151. 8vo. *London* 1888. The Editors.

Nederlandsch Kruidkundig Archief. Ser. 2. Deel V. Stuk 1. 8vo. *Nijmegen* 1887. The Netherlands Legation.

Revista do Observatorio. Anno 2. Num. 12. 8vo. *Rio de Janeiro* 1887. The Imperial Observatory, Rio de Janeiro.

Victorian Year-Book for 1885–86. 8vo. *Melbourne* 1886.
The Government Statist of Victoria.

Young Scientist (The) Vol. I. No. 4. 8vo. *London* 1888.
The Editor.

February 23, 1888.

Mr. JOHN EVANS, D.C.L., Treasurer and Vice-President, in the Chair.

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

The following Papers were read:—

I. "On the Relation between the Structure, Function, and Distribution of the Cranial Nerves. Preliminary Communication." By W. H. GASKELL, M.D., F.R.S. Received February 9, 1888.

In a previous paper* I have pointed out that the structure, distribution, and function of the spinal nerves, as well as the arrangement of their centres of origin in the spinal cord, all lead to the conclusion that these nerves are divisible into two parts; (1) a somatic part, supplying the external surface of the body and the muscles derived from the muscle plates, and (2) a splanchnic part, supplying the internal surfaces and organs and the muscles derived from the lateral plates of mesoblast.

I also pointed out that the cranial nerves were built up on a similar plan and arose from similar centres of origin to the spinal nerves; that they too were divisible into somatic and splanchnic groups of the same type as in the spinal nerves.

In that paper I dealt especially with efferent nerves, and pointed out that the somatic efferent nerves are non-ganglionated, and pass from the nerve cells of the anterior horn direct to the muscles derived from the muscle plates; on the other hand, the splanchnic efferent nerves are divisible into a ganglionated and a non-ganglionated group, of which the non-ganglionated motor nerves arise from a lateral group of nerve cells forming part of the lateral horn, which are continued cranialwards as the separate nuclei of such nerves as the facial, &c., and pass from the anterior root to the muscles derived from the lateral plates of mesoblast; while the ganglionated efferent nerves arise either from the cells of Clarke's column or from those of the lateral horn or from both, and pass to the so-called sympathetic

* 'Journ. of Physiol.,' vol. 7, p. 1.

ganglia to supply the muscles of the vascular system, alimentary canal, &c.

The whole argument in that paper was based upon the structure and distribution of the nerves of anterior roots, so that in speaking of the sympathetic ganglia as ganglia of a splanchnic root, all the evidence for vasomotor nerves, &c., went to show that these ganglia might be considered as belonging to anterior (efferent) roots rather than to posterior (afferent) roots.

Again Onodi's observations that such ganglia are offshoots from the spinal ganglia on the posterior root do not militate against the motor character of these ganglia, since in the lower animals and in the first two cervical nerves of the higher animals both anterior and posterior roots pass into the spinal ganglion, so that there is no difficulty in imagining that as the motor portion of the original spinal ganglion travelled away from its parent ganglion mass, the motor or efferent nerves would no longer pass into the spinal ganglion but would pass free from it, being connected only with the separated vagrant portions of the original ganglion, *i.e.*, with the sympathetic system.

There is in fact no evidence to show that the anterior and posterior roots are not truly efferent and afferent, and there is also no evidence to show that the afferent ganglia have travelled away from their original situation in the same way as the efferent ganglia. We may therefore consider the ganglia of the afferent spinal nerves, both somatic and splanchnic, as *stationary* in position, and as forming the root ganglia of the various nerves, while the ganglia of the efferent spinal nerves are *vagrant*, and form the so-called sympathetic system.

A complete segmental spinal nerve is then composed of (1) anterior root with vagrant ganglion; (2) posterior root with stationary ganglion.

The anterior root again is divisible into two parts; (1) a large-fibred medullated, and (2) a small-fibred medullated part, of which the latter only is in connexion with the ganglia of this root; the non-medullated fibres being these splanchnic ganglionated fibres which have lost their medullary sheath in one or other of the ganglia.

If the cranial nerves are built up on the same type as the spinal nerves, it follows that in them too we must have (1) an anterior root and (2) a posterior root, with a root ganglion stationary in position close to the exit of the nerves from the central nervous system into which both anterior and posterior roots may or may not pass; also the anterior root must consist of a small-fibred ganglionated portion and a large-fibred non-ganglionated portion, the ganglion on the anterior root being in all probability vagrant and not stationary.

In order to see how far the cranial nerves conform to the same type as the spinal nerves, I will consider their structure and distribution seriatim, leaving out of consideration for the present the olfactory, the optic, and also, for reasons to be mentioned, the auditory nerves.

Beginning then with the IIIrd or oculo-motor nerve, we are dealing with a nerve whose function at the present time is purely motor, a nerve therefore which is ordinarily spoken of as representing an anterior root; in this nerve we find indications of a large-fibred and a small-fibred part. Tracing up these fibres outwards to their destination it is seen that the large fibres pass off to supply the eye muscles supplied by this nerve, while most of the small fibres separate out from the large fibres and pass directly into the ganglion oculo-motorii. This ganglion, which is in the main formed by these small medullated fibres of the IIIrd nerve, *i.e.*, radix brevis, is increased in size by the addition of ganglion cells formed on the radix longa from the trigeminal, and others in connexion with sympathetic fibres; the fibres in the ganglion are all of small size, and the short ciliary nerves which arise from it have not a single large fibre among them. The nerve fibres of the short ciliary nerves are almost all medullated, and according to most observers (Bidder and Volkman) are more numerous than those entering the ganglion, so that in this case these small nerve fibres which are motor to the ciliary and splanchnic muscles do not lose their medullary sheath in their passage through the ganglion, a peculiarity which distinguishes them from the motor nerves of the vascular system, and is suggestive in connexion with the fact that these muscles though unstriped in structure are to a certain extent voluntary in action.

Also the nerve cells of this ganglion are distinctly of two kinds, most of them unipolar, of the same type as those of a spinal ganglion, the minority multipolar of the type of the so-called sympathetic ganglion cells: this also suggests that this difference in the type of nerve cell is associated with the presence or absence of a medullary sheath in the nerves issuing from the ganglion, and does not necessarily imply that these unipolar cells are connected with posterior root fibres, and that therefore, as has been supposed, this ganglion is the root ganglion of the oculo-motor nerve.

We see then clearly that the oculo-motor ganglion is the ganglion of these small-fibred efferent nerves of the IIIrd nerve.

The IIIrd nerve then conforms in its structure, and in the vagrant character of its motor ganglion, to the plan of a spinal nerve as far as its anterior root is concerned. Where then is its posterior root? If it conforms to the plan laid down, the ganglion on the posterior root, *i.e.*, its root ganglion, ought to be situated on the nerve near its exit from the central nervous system, and here, in fact, I have found it in the nerves of man and sheep. I have made a series of consecutive sections through the rootlets of the IIIrd nerve of man, beginning from its exit out of the brain and passing peripheralwards, and have found that in the different rootlets a well-marked ganglion is formed in the same way as any spinal ganglion, with, however, one important difference; the nerve cells and groups of nerve cells have degenerated, but their place and position remain conspicuously marked out with characteristically arranged masses of peculiar neuroglia-like connective tissue substance. So striking is the resemblance to a spinal ganglion, that with a low power it is difficult at first sight to believe that it is not a section of a functional ganglion which is exposed to view.

These degenerated ganglia are limited to a definite portion of each nerve rootlet just as in a spinal ganglion; centralwards of the ganglion the degenerated tissue can be traced as a strand of the same peculiar neuroglia-like connective tissue into the brain; peripheralwards of the ganglion all trace of altered nerve tissue or ganglion cells has disappeared.

Here then we have what appears to me without doubt to be the phylogenetically degenerated posterior root and root ganglion of the IIIrd nerve; so that in its posterior root, and in the situation of its root ganglion, it conforms also to the plan of a complete spinal nerve.

In the IVth nerve I find the same structure, an anterior root composed of a large-fibred portion and a small small-fibred portion; the destiny of this latter, and its connexion with any vagrant motor ganglion, I have not yet had time to trace out.

Soon after the IVth nerve leaves the valve of Vieussens, it forms upon it a conspicuous spinal ganglion of the same character as those on the rootlets of the IIIrd nerve, the cells of which are all degenerated, and the degenerated posterior root fibres are conspicuous between the brain and this ganglion, but cease peripheralwards of the ganglion.

In the VIth nerve the small-fibred part of the anterior root is much more doubtful than in the case of the two preceding nerves; so, too, with the posterior root, its ganglion is limited to a few degenerated nerve cells, and is nothing like so conspicuous as in the case of the IIIrd and IVth nerves.

In the so-called motor root of the Vth nerve we see again distinct groups of small fibres together with the large motor fibres. I have not yet had time to trace out these small fibres to their respective motor ganglia, but have little doubt that they will be found to bear the same relation to the spheno-palatine ganglion as those of the IIIrd nerve do to the oculo-motor ganglion. In the so-called motor root of the Vth nerve is found also a degenerated posterior root, with its ganglion in the same situation and of the same character as in the preceding nerves.

The so-called motor root of the Vth nerve is therefore a complete nerve belonging to the same group as the IIIrd, IVth, and VIth, and does not require the sensory portion of the Vth to make it resemble a spinal nerve.

Leaving aside for the moment the consideration of the sensory part of the Vth nerve we come to the VIIth nerve; here we find the anterior root manifestly composed of a large-fibred and a small-fibred portion, the latter being derived mainly from the *n. intermedius*, though some of the fibres are in the roots of the facial itself. The ganglion geniculatum bears the same relation to these small fibres as the ganglion oculo-motorii to those of the IIIrd nerve, and ganglia which are still further vagrant are seen in the submaxillary ganglion, &c. The ganglion of its posterior root is found in the rootlets of the facial in the usual position, directly after their exit from the brain, and in man both nerve fibres and nerve cells are degenerated in the same way as in the case of the cranial nerves already considered.

The cranial nerves considered up to this point form a natural group all arranged on the same plan with a ganglionated and non-ganglionated anterior root, and a phylogenetically degenerated posterior root and ganglion.

Passing now to the nerves of the medulla oblongata, we find another group with different characteristics. Here there is no sign of any degenerated posterior roots or spinal ganglion; here we find not degeneration of any component but separation of the component parts of a spinal nerve, so that the separate nerves no longer, as in the previous cases, represent each a perfect nerve. Thus in the IXth, Xth, XIth, and XIIth nerves of *man* at all events, the somatic portions of the posterior roots are absent in the nerves themselves with the exception of the auricular branch of the vagus, but clearly are not absent in reality, for the structure of the medulla oblongata shows that they have become diverted from these nerves to help form the sensory part of the Vth, and the Gasserian ganglion. The somatic motor part of this group is present, not as forming a part of each nerve, but as a separate nerve, the hypoglossal or XIIth nerve, the nucleus of origin of which extends along the whole length of the medulla oblongata. The ganglia jugularia of the IXth and Xth nerves which give origin in the Sauropsida to the laryngo-pharyngeal nerve, are the spinal ganglia of the splanchnic portions of the posterior roots of this group, while the ganglion petrosum of IX, and the ganglion trunci vagi (the vagrant character of which is well shown in such animals as the crocodile) are the motor ganglia of the small-fibred portions of the anterior roots of these nerves. Finally, the non-ganglionated splanchnic large-fibred motor nerves have not separated off to form a separate nerve like the XIIth, but remain as the motor nerves of the laryngeal and pharyngeal muscles. In fact, the IXth and Xth nerves with the medullary part of XI contain all the splanchnic elements belonging to a spinal nerve, or rather a group of spinal nerves, and in man at all events contain none of the somatic elements (with the exception of the auricular branch of the vagus), the somatic portions being represented by the hypoglossal, and a portion of the sensory root of the trigeminal.

Turning our attention to the sensory root of V, we see no sign of any degenerated ganglion or degenerated posterior root; it clearly possesses a functional, well-developed spinal ganglion, the Gasserian; and according to human anatomists, it is exclusively derived from the ascending root of the Vth nerve, *i.e.*, it arises in close connexion with the posterior horn along the whole length of the central nervous system comprised between its point of exit and the middle of the cervical region of the cord. In the absence, then, of any signs of degeneration among its fibres, combined with the presence of a degenerated posterior root ganglion in the so-called motor root of V, we may, I think, fairly conclude from the peculiarity of its origin that the sensory part of V and the Gasserian ganglion does not represent the posterior root of a nerve of which the so-called motor part of V is the anterior root. The explanation of the peculiarities of the origin of the sensory somatic elements of the ascending root of V, as well as of the corresponding sensory splanchnic elements of the ascending root of X must be sought for in the explanation of the presence of the degenerated posterior root ganglia of the Group I of cranial nerves already mentioned.

As far as VIII is concerned, it will suffice at present to say that it does not possess an undoubted degenerated ganglion, that part of it, at all events, possesses a functional spinal ganglion, and that it is a complex nerve, the structure of which requires a much more extensive investigation than I have as yet been able to give it.

In connexion with the presence of these degenerated posterior roots and spinal ganglia, it is significant that in the region of the brain from which these roots spring, groups of strongly pigmented cells are found, the reason for the presence of which is unknown. Of these groups the cells of the *locus cœruleus* are in structure and position clearly the termination of Clarke's column, and are therefore in all probability connected with the remnants of the small-fibred ganglionated efferent portions of some of the nerves of this group; the cells, on the other hand, of the *Substantia nigra* are in apparent connexion with and are embedded in the direct continuation of the degenerated posterior root fibres of the IIIrd nerve.

To sum up, then, it is clear that apart from I, II, and VIII, the rest of the cranial nerves are built up on the same type as the spinal nerves, and that their peculiarities are such as to divide them into two groups, viz., (1) those which arise from the mid-brain and hind-brain, *i.e.*, III, IV, V_m, VI, VII, all of which are at present, *i.e.*, in man, motor, but possess a degenerated posterior root and ganglion; and (2) those which arise from the med. oblongata, viz., IX, X (in part), XI (in part), XII, V_s (in part), which are characterised not by the loss of any component part, but by the scattering of the different components; a scattering which bears an intimate connexion with the making good of the loss of the sensory elements of Group I.

Finally, certain points connected with the question of the segmental value of the cranial nerves other than those already discussed are worthy of mention. The question of the segmental arrangement of any nerves may as far as their distribution is concerned be considered in a twofold light; 1st, the evidence for any segmental arrangement of somatic parts, as, for instance, of somatic muscles; and 2nd, evidence for any segmental arrangement of splanchnic parts, such as visceral clefts and arches, and the visceral muscles formed from the walls of such clefts.

In order to compare cranial nerves with spinal nerves we must compare structures of the same kind; if the segmental arrangement is based in the one case on the formation of myotomes, then we must search for the corresponding myotomes in the other, if on visceral arches, then we must argue upon the basis of visceral arch formation throughout. Now, van Wijhe has pointed out that the cranial muscles are derived from two groups of muscle segments, (1) a set of myotomes corresponding to the segmented muscle plates throughout the animal which form the muscles I have called non-ganglionated somatic, and (2) a set of lateral plates of mesoblast, lining the walls of the different visceral and branchial cavities, which give rise to the group of muscles which I have called non-ganglionated splanchnic. These latter muscles are unknown segmentally except in the head, they probably form in the trunk, as will be shown immediately, the diaphragm and transversus abdominis muscles.

Now van Wijhe, on the strength of his embryological researches, divides the head of the Selachians into nine segments, with the following arrangement of muscles and nerve supply.

	Muscles from myotomes (somatic).	Visceral clefts.	Muscles from lateral plates (splanchnic).	
1st segment..	Eye muscles, IIIrd nerve			
2nd ,, ..	,, IVth ,,	1. Mandibul.	Vth motor part	Mastication and gill muscles.
3rd ,, ..	,, VIth ,,	2. Hyoid	VII	
4th ,, ..	......			
5th ,, ..	......	3. 1st branch.	IX	
6th ,, ..	......	4. 2nd ,,	X	
7th ,,	Muscles from skull to shoulder girdle XIIth nerve	5. 3rd ,,	X	
8th ,,		6. 4th ,,	X	
9th ,,		7	X	

From which we see that if we look upon the myotomes as representing the primitive segmentation, while the visceral clefts and muscles from the lateral plates of mesoblast represent the secondary segmentation (Branchiomery), the 4th, 5th, and 6th segments are unrepresented by any muscles, or rather the 4th and 5th, for he notices the slight muscle formation in the 6th somite, but was unable to trace any special muscle to it.

Now looking at these two groups of muscles, the splanchnic and somatic, we see that the muscles of mastication and expression differ in structure, colour, nature of contraction, and general appearance from the muscles of the eye and the somatic muscles generally, with the exception of the specialised tongue muscles. I find also that the motor nerves of the IIIrd, IVth, and VIth nerves are in the dog much larger in calibre than those of the facial and slightly larger than those of the trigeminal, the eye muscles being innervated by nerves of the same size as the large motor nerves of anterior roots, *i.e.*, from 14·4 μ—18 μ, while the facial muscles are innervated by nerves of the same size as the large motor nerves of the vagus and glossopharyngeal which supply the pharyngeal and laryngeal muscles, *i.e.*, from 9 μ—10·8 μ.

Any nerve fibres, therefore, of the size of those of the VIth nerve, for instance, would be very conspicuous and easily followed if they appeared in among the smaller fibres of the facial. Such is the case; the facial roots possess a group of large fibres of the size of the somatic motor nerves in among the smaller fibres; a series of sections through the facial has enabled me to trace one group of these large fibres, and it is a beautiful sight to see them separating out one by one to come to the edge of the nerve, and finally to form a small nerve, which is found to be the *n. stapedius.* The rest of them leave the facial nearer its exit from the brain, and I think pass out as the nerve supplying the *Levator veli palati* muscle. The difficulty, however, of combining the dissection of these parts with the necessary freshness and freedom from damage which is requisite to ensure a good osmic preparation, has prevented me up to the present from making sure of this latter point.

When the facial leaves the stylo-mastoid foramen it is free from these large fibres. I venture to suggest that the structure of these two muscles, their origins, their shape, colour, and appearance, combined with the size of their motor fibres, all lead to the conclusion that they belong to the same group as the somatic eye muscles, and represent van Wijhe's missing 4th and 5th myotomes. Further, we see this, that if the splanchnic voluntary muscles derived from the lateral plates of mesoblast are differentiated from the somatic voluntary muscles derived from the myotomes by the size of their motor nerve fibres, we ought to find the same relation in the trunk as in the head; this seems to me to be the case, the nerve fibres of the phrenic in the rabbit separate out from those of the 4th and 5th cervical nerves as a group of fibres of smaller size than the surrounding motor nerves of those segments, and also the fibres of the nerve supplying the *m. transversus abdominis* in the dog are distinctly smaller than those of the nerve supplying the *m. obliq. sup.* I conclude then that the primitive segmentation of the head is shown by the somatic muscles of the IIIrd, IVth, and VIth nerves, *m. stapedius*, *m. levator veli palati*, and the muscles of the XIIth nerve, a segmentation which is in agreement with the segmentation of the trunk. In addition to this a secondary segmentation has taken place in the formation of the gills; the muscles belonging to this segmentation show a segmental arrangement in connexion with the gills but not in the case of the trunk, for as far as I know the *m. transversus* and the diaphragm show no sign of segmentation.

In this preliminary communication I cannot discuss all the problems which are opened out by the new light which examination of the structure of the cranial nerves has shed upon their distribution and past history. The explanation of the degenerated posterior roots of certain of the cranial nerves is certainly to be found in the history of the vertebrate animal, and I hope in the course of the summer to publish the full paper of which this is a preliminary account, and in that paper to make some attempt to account for this remarkable phylogenetic degeneration. In conclusion, I may remark that Marshall has previously noticed in the chick a group of ganglion cells at the origin of the IIIrd nerve out of the brain. Also it has been pointed out to me that my discovery of degenerated nerve cells and nerve tissue in such cranial nerves as the IIIrd is not, as I thought, entirely new. The structures in question have been observed by Thomsen and other pathologists. The explanation which I have given is, however, entirely new.

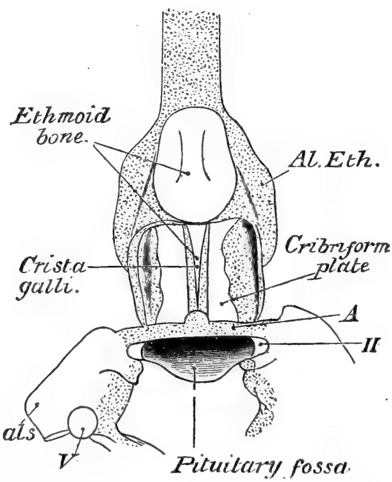
II. "Preliminary Note on the Development of the Skeleton of the Apteryx." By T. J. PARKER, B.Sc., Professor of Biology in the University of Otago.* Communicated by W. K. PARKER, F.R.S. Received February 9, 1888.

[Note by W. K. P.—This is not the first, but it is the most important, of the "Notes" sent to me by my son, on the development of this, the *lowest and most Reptilian* of all birds known. *Seven* stages before hatching have been obtained, and will yield, I am satisfied, most instructive results and, added to what has already been done in the other forms of the Ratitæ, will give something like completeness to our knowledge of the morphology of these archaic types. It is not merely as low kinds of birds that the Struthious types are so important to the biologist; they are so intimately related to the more primitive forms, both of Reptiles and Mammals, that any new fact as to their structure is of great value. This short paper is accompanied by one of my own, purposely to throw light upon its meaning and bearing.]

Extracts from a Description of the Skull of Apteryx at about Period of Hatching.

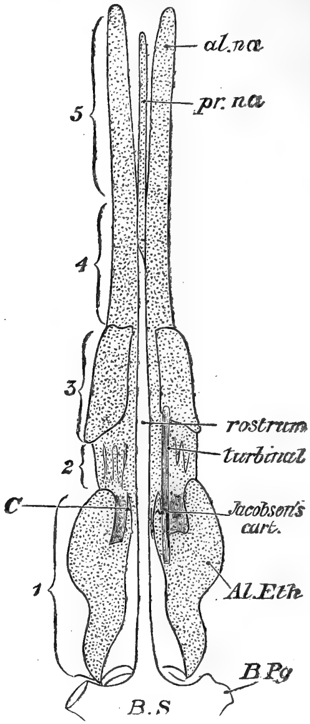
Each alisphenoid is connected by a rod of cartilage (A) with the postero-dorsal angle of the mesethmoid; a *transverse cartilaginous bar* is thus produced, which forms the anterior boundary of the pituitary fossa and the dorsal boundary of the optic foramen.

FIG. 1.



* The Note here given is on the skull only, but I have already received notices of things found in the rest of the skeleton of the apteryx that are only second in importance to those upon the skull.—W. K. PARKER.

Fig. 2.



Ventral View.

The united olfactory capsules consist of (*a*) the mesethmoid, passing in front into the pre-nasal cartilage; (*b*) the *lateral ethmoidal plates*; and (*c*) the turbinals.

The mesethmoid forms in its posterior half a vertical plate, with a thickened lower rim; its posterior border is vertical, and forms the front boundary of the pituitary fossa; the posterior portion of its dorsal border (*cr. gall.*) is concave, from before backwards, and has exactly the relations of the crista galli of a Mammal. Passing forwards from the crista galli, the mesethmoid reaches the surface of the skull, where, with the adjacent portions of the lateral ethmoids, it forms a lozenge-shaped area between the posterior ends of the nasals. Cephalad of this it gradually narrows in the vertical direction, finally becoming in the anterior portion of the beak a mere rod, the pre-nasal cartilage or basi-trabecular. *There is now no trace of the parietal fenestra mentioned by W. K. Parker on the authority of Blanchard* ("Ostrich Skull," p. 173).

In the lateral ethmoidal plates of this bird no clear distinction can be drawn between ali-ethmoidal, ali-septal, and ali-nasal, any more than between mesethmoid, septum nasi, and pre-nasal. The dorsal border of the mesethmoid, from anterior end of crista galli to within about 2 cm. of end of beak, sends off horizontal plates on each side, which pass at first outwards, then downwards, and finally, in part of their extent, inwards, thus forming roof, outer wall, and in part floor of the nasal chambers.

The precise relation of these lateral ethmoidal plates varies in different regions, and it is convenient to consider them as consisting of five portions.

In the first or posterior portion, besides passing outwards and downwards, they take a sweep backwards, and then inwards, thus forming an almost complete somewhat shell-like covering for the principal or inter-orbital portion of the olfactory capsules. *To this portion the name ali-ethmoid may be applied.* Each ali-ethmoid is a thin plate of cartilage, convex externally, forming the outer border of the *cribriform space* (olfactory foramen) by its dorsal free edge, and closely applied below by its ventral edge to the mesethmoid, immediately dorsad of the rostrum; in front it is continuous laterally and dorsally with the second portion, and ventrally ends irregularly, presenting a deep anterior emargination, which separates a slender, forwardly directed process (c) from the main ali-ethmoidal cartilage.

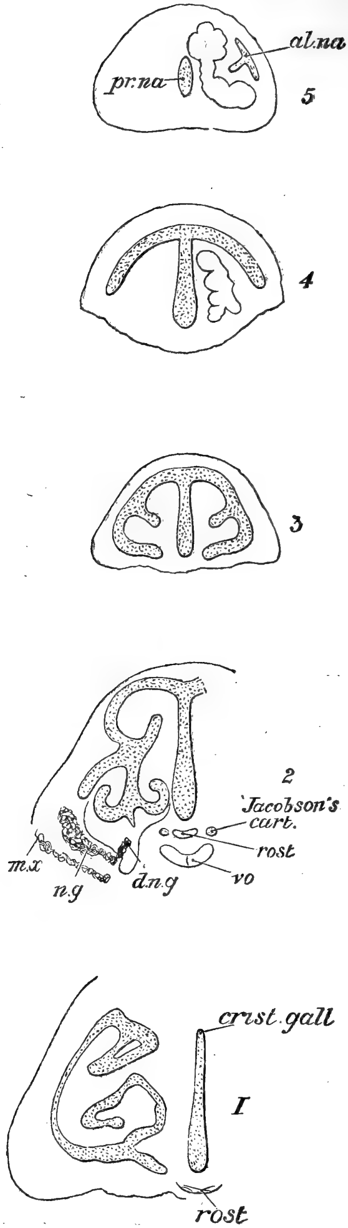
In its second portion the lateral ethmoidal cartilage furnishes only roof and sides to the nasal chamber, the floor being absent, and thus the turbinals are visible from below.

In its third portion it again turns inwards so as to furnish a floor to the nasal cavity, in the form of a plate with a straight lower border abutting against the mesethmoid, and with oblique anterior and posterior edges.

In their fourth portion the lateral ethmoids again furnish only roof and outer walls to the off-chamber, and in their fifth or anterior portion, they are entirely unconnected with the mesethmoid (pre-nasal), and form two slightly divergent obliquely placed plates of cartilage which are continued to the end of the beak, passing dorsad of the nostrils. To these the name ali-nasals might be applied, but owing to the unique position of the anterior nares, the relations of these and other parts of the ethmoidal region is strikingly different from what we are familiar with in other birds.

There is a single ethmoidal ossification, mesethmoid, in the form of a bone composed of horizontal and vertical portions and T-shaped in transverse section. The horizontal portion is shield-shaped, and appears on the surface of the skull between the posterior ends of the nasals: the vertical portion ossifies the whole postero-dorsal region of the mesethmoid: the bone in question is therefore partly mes- and partly ecto-ethmoidal.

Fig. 3.


al.na
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5
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Jacobson's cart.
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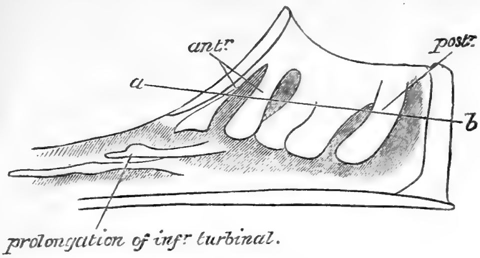
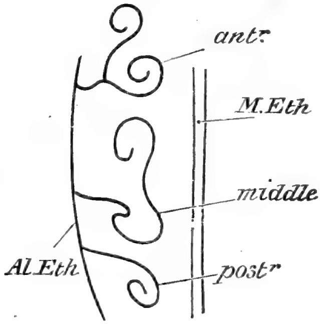
Fig. 4.



Mesial surface of turbinals.

Fig. 5.



Horizontal section along *a*, *b*.

The turbinals consist of in-growths of the ali-ethmoid into the nasal cavity, and are altogether three in number. They may be called respectively anterior, middle, and posterior turbinals. The posterior turbinal has the form of a vertical scroll of very thin cartilage, attached by the upper and lower ends of one edge to the ali-ethmoid, but free in the middle; it is rolled upon itself caudad, forming about one turn and a half.

The middle turbinal is attached along the whole length of one vertical edge: from its attachment it passes mesiad, then turns sharply laterad, then curves mesiad again, and passes forwards (upheld) as a broad plate somewhat indented in the middle by a vertical furrow, which gives it, when viewed from the inner face, the appearance of a double fold: this broad plate is attached to the ali-ethmoid all along its vertical edge, and in front is rolled upon itself caudad, forming a scroll of one turn which is attached above and below but free in the middle.

FIG. 6.

The anterior turbinal arises as a single somewhat oblique plate, attached to the ali-ethmoid along its whole length: it soon divides into two plates, each of which becomes rolled upon itself forwards, forming two oblique scrolls.

Finally, the anterior turbinal is continued forwards into the narrow portion of the olfactory chamber (2nd and 3rd portions of lateral ethmoid, see sections), as a horizontal plate, which, like the main portion of the turbinal, divides into two: these are rolled upon themselves respectively dorsad and ventrad, forming two horizontal scrolls, which become simpler and simpler, finally being reduced to a single narrow horizontal plate, which is continued as far forwards as a small arterial foramen in the lateral ethmoidal cartilage at the junction of its 3rd and 4th portions.

In addition to these, which may be called the turbinals proper, the 5th anterior (free) portion of the lateral ethmoidal cartilage sends inwards for a short distance a narrow horizontal shelf-like process (fig. 3, sect. 5), beginning immediately caudad of the nostril, and nearly as far back as the junction of the lateral ethmoids with the mesethmoid.

On each side of the ventral edge of the mesethmoid, in the vomerine region is a slender, free rod of cartilage, shaded dark in fig. 2, imbedded in connective tissue, and lying parallel with, and either immediately above or slightly laterad of, the dorsal edge of the vomer. It is about 10 mm. long, and about 0·14 mm. in diameter. This is obviously the vestigial cartilage of Jacobson's organ, figured but not described in "Ostrich Skull" (Plate 10, fig. 14), described in "Skull of Bird," Plate 2 (p. 109, Note): called *upper labial* in Snake (Plate 30, fig. 2), and *nasal floor* in Lizard (Plate 44, figs. 3 and 4).

III. "On Remnants or Vestiges of Amphibian and Reptilian Structures found in the Skull of Birds, both Carinatæ and Ratitæ." By W. K. PARKER, F.R.S. Received February 9, 1888.

One of the most remarkable structures found in the skull of certain *Amniota* or higher Vertebrata—Reptiles and Mammals, is the so-called "Jacobson's organ."

A pair of these curious gland-like bodies, each carefully placed in its own special capsule, may be seen in Serpents, Lizards, and Mammals; but they are not present in Tortoises, Crocodiles or Birds, as far as our present knowledge goes. Rathke, in his work on the Snake's Skull, spoke of the "nasal glands and their capsules," and for a long while this term made them to be confused with the *nasal glands* of birds, which have nothing whatever to do with "Jacobson's organs."

These structures are largest in Serpents, Lizards, and Monotremes, next in order come the Marsupials, Edentates, and Insectivores, then the Mammalia generally, including Man himself, in whom they appear for a time, and then vanish away.*

These structures lie just above the anterior incisive foramina. I

* See Kölliker, 'Ueber die Jacobson'schen Organe des Menschen,' Leipzig, 1877.

need not now state anything further with regard to them, as I have already given in my papers on the Skull of the Snake, the Lizard, the Edentata, and Insectivorous Mammalia, numerous figures and descriptions of them. I must, however, repeat one or two facts; in the Snake and Lizard these gland-like bodies lie each in a little dish, formed by the vomer of that side, covered in by another vomerine bone—the septomaxillary. They are also protected at the opening of the capsule by a pedate tract of cartilage, derived from the ali-nasal fold, which, in the Snake, frequently becomes detached from its root.

In low Mammalia there are several vomers, *ten* in *Cuscus maculatus*, a low kind of Phalanger. Now in most of the lower kinds of Mammalia, examined by me, a pair of small anterior vomers lie on the inside of Jacobson's organs, but the capsule itself is formed by a peculiar fold of cartilage—the recurrent cartilage, which closes in upon itself and unites its edges round the gland.

As a rule these "recurrent cartilages" retain their union with the alinasal folds, as in the Lizard; in the Rabbit they are distinct, as in the Serpent (Howes).

Now in Birds these cartilages not unfrequently appear, but no "Jacobson's organ" has been found with them. The Birds whose *vomerine region* comes nearest to that of a low Mammal are the "Turnicidæ," or Hemipods, and the great group of the Passerine birds (Coracomorphæ, or Ægithognathæ of Huxley).

It is not uncommon for the *ox-faced vomer* of these birds to be formed of two pairs of bony centres, and these become not only fused together, but actually grafted upon the floor of the cartilaginous nasal capsule, in the same manner as is common in the lower kinds of Mammalia.

Now I find remnants of the cartilaginous capsule of Jacobson's organs, not only in the Hemipods and in the lower Neotropical Passerines (*Homorus*, *Synallaxis*, *Anœretes*), but also in some of the highest of the singing-birds, namely, the Wren (*Anorthura troglodytes*), and also in some of the Woodpeckers (Picidæ), outside the Passerine order.

In my paper on the "Skull of the Ostrich Tribe" ('Phil. Trans.,' 1866, Plate 10, fig. 14, *a.i.t.*), I figured and described, *but did not understand*, a peculiar cartilage perched right and left upon the large vomer of the Rhea. I have been for a long time satisfied that this also is one of the vomerine or Jacobson's cartilages, and this view is corroborated, and to my mind proved, by what my son has found in the palate of the Apteryx.

Now if my son's figure* of the transversely-vertical section through these cartilages and the crura of the vomer in the Apteryx, be com-

* In the Paper preceding this.

pared with various figures in my Memoirs on the Mammalian Skull (Parts 1, 2, and 3, 'Phil. Trans.'), it will be seen that it so nearly corresponds with sections of the skull of the Pig, the Edentata, and the Insectivores, especially those taken just behind Jacobson's organ, that without explanation it would be impossible to tell which figure belonged to the Bird, and which to the Mammal.

In the Fowl, the Duck, and other Precocial birds, the embryo of the eighth day of incubation is in the Amphibian stage; then, the web goes beyond the toes, on the foot, whilst the rudimentary wing shows clearly a paddle with three digits in it, the first shortest, and the third not much shorter than the second. Very soon, however, after this, the first and second digits acquire a *claw*. Thus the Reptilian stage has been attained; for I know of no existing Amphibian with claws, except the Cape Nailed Toad (*Dactylethra*).

Before I had ever offered any of my Morphological Papers to the Royal Society, I had stumbled upon a part in the development of the skull in the chick, the importance of which is to me much greater now than ever.

Since that time, besides working out the development of the skull in many types of Birds, all the Ratitæ except the Apteryx, and in family after family of the Carinatæ, I have had the opportunity to work it out in all the main types of existing Reptiles; the result is to me very remarkable.

In all the Ichthyopsida, except the cartilaginous Fishes—Marsipobranchs and Elasmobranchs (Hag, Lamprey, Shark, Skate), the base of the skull is supported by a long splint-bone, the nature of which had been completely misunderstood by the old school of anatomists, but which was put into its right category by our great Reformer, Professor Huxley, and called by him "parasphenoid."

This great superficial basi-cranial beam is largest in those Ganoids that are half Selachian—the Sturgeons and their allies: but it is very large in the other (Holostean) Ganoids, in the Teleostei, Dipnoi, and all the Amphibians.

It is not part of the true skull, it is the subcutaneous part of a dermal scute, formed inside the infolded skin of the mouth, and is a truly Teleological bone, developed for support to badly ossified *endocrania*, just as such skulls are supplemented by dermal bones—"Dermostoses" or "Parostoses," above, and on each side. This bone, well seen in the Frog, is dagger-shaped, and reaches from near the foramen magnum behind, to the nasal capsule in front, the "guard" of the dagger supporting the auditory capsules. Now in Serpents only the *blade* is present; in Lizards only a very fine thread of bone representing the blade; in some, *e.g.*, *Trachydosaurus rugosus* ("Cyclodontidæ"), even this is wanting. It is not present in those very Amphibian forms the Chelonians; and in Crocodiles, I can only find a small remnant of the "guard" right and left, or two "basitemporal" plates, soon buried up by the huge pterygoid.

In all birds they are large, as large as in Frogs and Toads; this is equally true of the *Dinornis* and of the smallest Humming-bird.

There is a tendency to break up into lesser bony parts; thus *for a day or two* in the chick there are two "basitemporal" and one "rostral" centre; but in several species of the Ranidæ, *e.g.*, the Bull-frog among others, the point of the dagger-shaped bone is separately ossified, and remains distinct.

In the Paradoxical Frog (*Pseudis paradoxa*) there is no "handle" to the dagger; the same form of *parasphenoid* is common among the water-birds, *e.g.*, *Alca*, *Uria.* This is an ossification which is *the earliest to appear* in skulls that take on any kind of ossification; it is also the first bone to appear in an embryo bird, as in the larval Frog.

These facts, and many others that I could mention, make it evident that in seeking for a clue to the uprise of the Feathered Fowl, we may leave out of immediate consideration all the existing types of Reptilia: ancient Amphibians, or Reptiles just rising out of Amphibian lowliness, are the forms that alone will help us in this search.

We do get some light upon the Reptilian relationship of Birds, but it is at best a *scattered light;* the head of a bird is like that of the *Ichthyosaurus,* in its great facial elongation, the neck- and limb-regions of a Bird are those of a *Plesiosaurus,* whilst the hips and legs are like those of the "Ornithoscelida."

Scarcely any Urodeles, and only a few of the Anura, show any special elongation of the "intertrabecula" or pre-nasal rostral cartilage; this must have been very long in the Ichthyosauri as in the Selachians, and as in the embryos of all Birds.

In the Tadpole, with its oral aperture in front of the head, the quadrate cartilage or suspensorium of the lower jaw is parallel with the fore part of the basis-cranii, or *trabeculæ.* During transformation the quadrate hinge gradually gets further and further back until, in the Bull-frog, it is beneath the neck, close to the shoulder. The pterygo-palatine arcade, which was a mere connecting band between the quadrate and the trabecula, becomes the long palato-pterygoid arch or arcade, and the fore part of it is tri-radiate, and has received a term for each ray.

Thus the suspensorial part or pedicle is the ethmo-palatine, the anterior free spike the pre-palatine, and the hinder part which runs into the pterygoid is the post-palatine.

The anterior part of the pterygo-palatine arcade is distinct from the pterygoid in the Salamanders and their allies—Urodeles—and the pterygoid in them is an outgrowth of the quadrate which grows forwards towards the palatine, but does not coalesce with it, except in *Ranodon sibiricus*.*

This endoskeletal cartilaginous palatine, with its peduncle and fore and hind ray or *crus*, appears in several kinds of birds, in addition to their *normal parosteal* palatine—a mere membrane bone, as in Reptiles and Mammals.

This vestige or remnant remains in the adult; *it is of no apparent use*, and occurs in the Families in the oddest way; sometimes, however, it is present in all the members of some particular Family-group, as for instance in the Musophagidæ or plantain-eaters (*Musophaga*, *Schizorhis*, and *Corythaix*).†

My own later researches show it well in the Oil Bird (*Steatornis caripensis*) and in the Green Tody (*Todus viridis*); but it is well developed in *Scythrops* (see 'Linn. Soc. Trans.,' ser. 2 (Zool.), vol. 1, plate 23, figs. 4 and 3, *o.u.*)

In that nearly extinct Neotropical type, *Steatornis*, this curious partly ossified remnant has the three crura, all well marked, and their morphological meaning is evident; albeit the whole piece is so small and feeble that it can serve no purpose in the solid palate of that remarkable bird.

To show how unexpectedly this remnant exists, and does not exist, I will give a list of the Birds in which it has been found in a segmented state as a distinct bony element of the face; it often shows itself as a mere process of the ecto-ethmoid: I do not include those birds in the list.

Motacilla yarrelli *Budytes rayi*	Motacillidæ.
Todus viridis.	Todidæ.
Steatornis caripensis.	Steatornidæ.
Schizorhis *Musophaga* *Corythaix*	Musophagidæ.
Dicholophus.	Dicholophidæ.
Procellaria *Prion* *Thalassidroma* *Diomedea, &c.*	Procellaridæ.
Larus, var. spec.	Laridæ.
Tachypetes.	Tachypetidæ.

Another more partial remnant is seen in the Coracomorphæ or Passerine birds generally, which together make up nearly half the number of known birds.

In my paper "On the Skull of the Urodeles" ('Phil. Trans.,' 1877,

* See Wiedersheim, 'Kopfskelet der Urodelen,' Leipzig, 1877, Plate 5, figs. 69, 70.

† See Reinhardt, 'Om en hidtil ukjendt Knogle i Hovedskallen hos Turakoerne (*Musophagides*, Sundev.)' Copenhagen, 1871, Plate 7.

plate 24, figs. 1–3), I showed that the "post-palatine" tract of cartilage was developed as a distinct nucleus in the Axolotl (*Siredon*).

That distinct nucleus representing the *post-palatine region* of the Frog's skull also re-appears in the Crow, in the Sparrow, and in all the Passerines, as far as I have been able to work them out. It lies outside the hinder part of the normal *parosteal* palatine bone, becomes a solid ear-shaped tract of hyaline cartilage, acquires its own osseous (endosteal) centre, and this, when ossified, coalesces with the normal palatine bone.

The only Reptile in which I have discovered any distinct trace of the *endoskeletal* palatine is the Green Turtle; it is very small (see my paper in the "Challenger Reports," vol. 1, part 5, pl. 12, figs. 9, 9*a*, 2*b* : *e.p.a.*).

These are not all, or nearly all the vestigial structures that are familiar to me in the Bird's skull—to say nothing of the skeleton generally; but they are sufficient, I think, to satisfy any reasonable person that Birds arose, by secular transformation, either from the lowest and most ancient of the true Reptiles, or equally with Reptiles from archaic Amphibia, low in structure, but full of potential excellence, and ready, *pro re nata*, to become Reptile, Bird, or even Mammal, as the case might be.

For many years I have been endeavouring to gather up the fragments of morphology that nothing should be lost; I am satisfied that these lingering but practically useless structures will be found to be very difficult of deglutition to anyone who believes that the Birds that now exist were created in their present form and condition.*

Presents, February 23, 1888.

Transactions.

Baltimore:—Johns Hopkins University. Circular. Vol. VII. No. 62. 4to. *Baltimore* 1888. The University.

Bordeaux:—Société de Médecine et de Chirurgie. 8vo. *Bordeaux* 1887. The Society.

Société des Sciences Physiques et Naturelles. Mémoires. Tomes II–III. 8vo. *Paris* 1886; Observations Pluviométriques et Thermométriques faites dans le Département de la Gironde de Juin 1885 à Mai 1886. 8vo. *Bordeaux* 1886. The Society.

Cardiff:—Naturalists' Society. Transactions. Vol. XIX. Part I. 8vo. *Cardiff* 1887. The Society.

* I have only referred to a few of the memoirs that contain the figures and descriptions of the parts referred to in this paper. They are, however, well known, and are mainly in the 'Transactions' of the Royal, Linnæan, and Zoological Societies.

Transactions (*contiueed*).

Edinburgh:—Royal Society. Proceedings. Nos. 121–123. 8vo. *Edinburgh* 1885–87; Transactions. Vol. XXX. Part 4. Vol. XXXII. Parts 2–4. Vol. XXXIII. Part 1. 4to. *Edinburgh* 1883–87. The Society.

Liverpool:—Astronomical Society. Journal. Vol. VI. Part 4. 8vo. *Liverpool* 1888. The Society.

London:—Pharmaceutical Society of Great Britain. Calendar. 1888. 8vo. *London.* The Society.

Royal Institute of British Architects. Journal of Proceedings. Vol. IV. No. 8. 4to. *London* 1888. The Institute.

Royal Statistical Society. Journal. Vol. L. Part 4. 8vo. *London* 1887. The Society.

St. Bartholomew's Hospital. Statistical Tables of the Patients under Treatment during 1886. 8vo. *London* 1887. The Hospital.

Manchester:—Geological Society. Transactions. Vol. XIX. Part 13. 8vo. *Manchester* 1888. The Society.

St. Petersburg:—Académie Impériale des Sciences. Mémoires. Tome XXXV. Nos. 8-9. 4to. *St. Pétersbourg* 1887. The Academy.

Simla:—Naturalists' Society. Journal. Vol. I. Parts 1–2. 8vo. [*Simla*] 1885-86. The Director, Royal Gardens, Kew.

Vienna:—K. Akademie der Wissenschaften. Anzeiger. Jahrg. 1887. Nos. 26–28. 8vo. *Wien.* The Academy.

Observations and Reports.

Brisbane:—Registrar-General's Office. Report, 1886. Folio. *Brisbane* 1887. The Registrar-General.

India:—Geological Survey. Records. Vol. XX. Part 4. 8vo. *Calcutta* 1887. The Survey.

International Polar Expeditions. Mission Scientifique du Cap Horn, 1882–83. Tomes IV, VI. 4to. *Paris* 1887. Ministères de la Marine et de l'Instruction Publique, Paris.

Beobachtungen der Russichen Polarstation an der Lenamündung. Theil II. Lief 2. 4to. 1887. Société Impériale Russe de Géographie, St. Petersburg.

Lisbon:—Commissão dos Trabalhos Geologicos de Portugal. Communiçaões. Tomo I. Fasc. 2. 8vo. *Lisboa* 1887. The Commission.

London:—Colonial and Indian Exhibition, 1886. Report of the Royal Commission. 8vo. *London* 1887. The Commission.

Meteorological Office. Hourly Readings. 1884. Part 4. 1885. Part 1. 8vo. *London* 1887; Daily Weather Reports. January

Observations, &c. (*continued*).

to June, 1887. 4to. *London*; Weekly Weather Report. 1887. Nos. 12–45. 4to. *London*; Monthly Weather Report. December, 1886. 4to. *London* 1887; Quarterly Weather Report. 1878. Part 4. 1879. Parts 1–2. 4to. *London* 1887; Meteorological Observations at Stations of the Second Order, 1883. 4to. *London* 1888. The Meteorological Office.

Navy. Statistical Report of the Health of the Navy, 1886. 8vo. *London* 1888. The Admiralty.

Moscow:—Musées Publics Roumiantzow. Catalogue de Monnaies et Médailles Romaines. (Russian.) Livr. 2. 8vo. *Moscou* 1887. The Museum.

Paris:—Bureau Central Météorologique de France. Annales. Année 1884. Fasc. 2. 4to. *Paris* 1887. Année 1885. Fasc. 1, 3–4. 4to. *Paris* 1887. The Bureau.

Observatoire. Catalogue de l'Observatoire. Étoiles observées aux Instruments Méridiens de 1837–1881. Tome I. 4to. *Paris* 1887; Positions observées des Étoiles, 1837–1888. Tome 1. 4to. *Paris* 1887. The Observatory.

Rio de Janeiro:—Imperial Observatorio. Annuario. 1885–87. Anno 1–3. 8vo. *Rio de Janeiro*, 1884–86. The Observatory.

St. Petersburg:—Physikalisches Central-Observatorium. Annalen. Jahrg. 1886. Theil 2. 4to. *St. Petersburg* 1887. The Observatory.

Sweden:—Sveriges Geologiska Undersökning. Ser. A. Kartblad med beskrifningar. Nos. 92, 94, 97, 101–102. Ser. Ab. Nos. 11–12. 8vo. *Stockholm* 1887; Ser. Bb. Specialkartor med beskrifningar. No. 5. 8vo. *Stockholm* 1887; Ser. C. Afhandlingar och uppsatser. Nos. 78–84, 86–88, 90–91. 8vo. *Stockholm* 1885–87; Ser. C. Afhandlingar och uppsatser. Nos. 65, 85, 89. 4to. *Stockholm* 1886–87. The Survey.

Washington:—Catalogue of the First Industrial Exposition by the Coloured Citizens of the District of Columbia, September, 1886. 8vo. *Washington* 1887. The President of the Exposition.

U. S. Geological Survey. Mineral Resources of the United States. Report, 1886. 8vo. *Washington* 1887. The Survey.

March 1, 1888.

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

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

In pursuance of the Statutes, the names of the Candidates for election into the Society were read from the Chair, as follows:—

Andrews, Thomas, F.R.S.E.
Bosanquet, Robert Holford Macdowall, M.A.
Bottomley, James Thomson, M.A.
Boys, Charles Vernon.
Burbury, Samuel Hawkesley, M.A.
Buzzard, Thomas, M.D.
Cameron, Sir Charles Alexander, M.D.
Carnelley, Professor Thomas, D.Sc.
Church, Arthur Herbert, M.A.
Clark, John Willis, M.A.
Clarke, Alexander Ross, Colonel R.E.
Corfield, William Henry, M.D.
Cunningham, Professor Daniel John, M.D.
Cunningham, Professor David Douglas, M.B.
Dickinson, William Howship, M.D.
Elgar, Professor Francis, LL.D.
Fletcher, Lazarus, M.A.
Galloway, William.
Gordon, James Edward Henry, B.A.
Greenhill, Professor Alfred George, M.A.
Halliburton, William Dobinson, M.D.
Henslow, Rev. George, M.A.
Howorth, Henry Hoyle.
Hughes, Professor Thomas McKenny, M.A.
Jervois, Sir William Francis Drummond, Lieut.-Gen. R.E.
King, George.
Lapworth, Professor Charles, LL.D.
MacMunn, Charles, M.D.
Martin, John Biddulph, M.A.
Matthey, Edward, F.C.S.
Ord, William Miller, M.D.
Palmer, Henry Spencer, Colonel R.E.
Parker, Professor T. Jeffery.
Pedler, Professor Alexander, F.C.S.
Pickering, Professor Spencer Umfreville, M.A.
Poulton, Edward B., M.A.
Poynting, Professor John Henry, M.A.
Priestley, William Overend, M.D.
Ramsay, Professor William, Ph.D.
Sanders, Alfred, M.R C.S.
Sankey, Matthew Henry P. R., Capt. R.E.

Seebohm, Henry, F.L.S.
Sharp, David, M.B.
Shaw, Professor Henry Selby Hele, M.I.C.E.
Sollas, Professor William Johnson, D.Sc.
Stevenson, Thomas, M.D.
Stewart, Major-Gen. J. H. M. Shaw, R.E.
Stokes, Sir William, M.D.
Teale, Thomas Pridgin, F.R.C.S.
Tenison-Woods, Rev. Julian E., M.A.
Thomson, Professor John Millar, F.R.S.E.
Thorne, Richard Thorne, M.B.
Tidy, Professor Charles Meymott, M.B.
Tizard, Thomas Henry, Staff-Commander.
Todd, Charles, M.A.
Tomlinson, Herbert, B.A.
Topley, William, F.G.S.
Trimen, Henry, M.B.
Ulrich, Professor George Henry Frederic, F.G.S.
Ward, Professor Henry Marshall, M.A.
White, William Henry, M.I.C.E.

The following Papers were read:—

I. "On the Changes produced by Magnetisation in the Dimensions of Rings and Rods of Iron and of some other Metals." By SHELFORD BIDWELL, M.A., F.R.S. Received February 9, 1888.

(Abstract.)

In a paper communicated to the Royal Society in 1885,* the author has shown that the elongation which an iron rod undergoes when magnetised does not, as had been generally believed, remain unchanged at a maximum when the magnetising force exceeds that which is sufficient to produce so-called saturation. On the contrary, he finds that when the magnetising force is continually increased beyond this limit, the elongation becomes gradually less and less, until the rod, after first returning to its original length, ultimately becomes actually *shorter* than when in the unmagnetised condition.

The experiments described in that paper are, however, open to objection, on the following grounds:—(1) The field due to the magnetising solenoid was not quite uniform; (2) the effect of the ends of the rods was uncertain, and might have played some material part in the production of the phenomena in question; (3) all the rods used in the experiments retained a certain amount of permanent magnetism; (4) the experiments might with advantage have been carried further. The paper now offered to the Society contains an account of some new experiments which were designed to meet the above objections.

Objections (1) and (2) were met by using rings instead of rods of

* 'Roy. Soc. Proc.,' vol. 40, 1886 (No. 242, p. 109).

iron, observations being made of the changes which occurred in their diameters under the influence of various magnetising forces obtained by passing currents of electricity through coils of wire encircling the rings. To remove the third objection the rings were demagnetised before every observation, by a modification of the method described by Professor Ewing in the 'Phil. Trans,' vol. 176, p. 537. And lastly, the battery employed was increased from seven Grove's cells to thirty.

After an explanation of the precautions taken to guard against the effects of current heating, an account is given of some experiments with three rings arranged in slightly different ways, and the results are compared with those of an experiment made under similar conditions with a straight rod. It was found that in their general character the phenomena of elongation and retraction were just the same in both cases, and were in close agreement with those of the former paper. The differences in mere details were not greater than would probably be found to occur in different specimens of iron of the same form.

Being satisfied that these curious effects of magnetism were practically independent of the form of the iron, and having regard to the fact that it was much easier to obtain intense fields with straight than with circular solenoids, the author thought it worth while to make some further experiments with straight rods. The metals used in addition to iron were cobalt, nickel, manganese-steel, and bismuth; and the highest magnetising force reached about 840 C.G.S. units, the maximum in the old experiments having been 290.

It was found that the retraction of the iron continued to increase with higher forces until it was finally as much as 45 ten-millionths of the length of the rod, when there were indications that a limit was being approached. The retraction of the nickel reached 113 ten-millionths, when it also was evidently not far from its limit.

The behaviour of the cobalt rod was exceedingly curious and interesting. No evidence of any change of length appeared until the magnetising force exceeded 30 or 40 units. Then the length of the rod began to diminish, and continued diminishing until the force was about 400, when the retraction amounted to 50 ten-millionths. But beyond this point the rod gradually became longer again, and the retraction with the highest force of 800 units was only three-fifths of its maximum amount. It was ascertained that the maximum retraction did not coincide with a maximum of magnetisation, as might have been suspected to be the case. It is suggested that iron and nickel might possibly behave in a similar manner under sufficiently high magnetising forces.*

* It is also suggested that some specimens of cobalt and nickel might, like iron, begin with a small preliminary elongation, thus accounting for Professor Barrett's observation that cobalt undergoes elongation when magnetised ('Nature,' vol. 26, p. 585).

Tables and curves are given showing the relation between magnetising force and changes of length in each metal.

Bismuth was found to be slightly elongated in strong fields, though no change could be detected with forces of less than about 500. The greatest elongation observed was about 1·5 ten-millionths of length.

Manganese-steel was almost unaffected. The elongation in a field of 850 was estimated to be about one fifty-millionth of the length.

Finally, it is shown that the mechanical stress produced in iron by magnetism does not account for more than one-fifth part of the observed magnetic retraction.

An Appendix to the paper contains evidence of the high degree of accuracy obtainable by the method of observation employed. In the very great majority of the measurements of elongation and retraction, the probable error was less than one two-and-a-half-millionth part of an inch, or one hundred-thousandth of a millimetre; and the results of experiments made upon different days (the apparatus having been in the meantime dismantled), or with currents of ascending and of descending strength, were strikingly concordant. This degree of precision is attributed to the perfection of the optical arrangements, which rendered it possible to project the image of a wire with such sharpness, that after reflection from a mirror its position upon a scale 24 feet (732 cm.) distant could be read to a quarter of a scale division, each whole division being equal to $\frac{1}{40}$-inch (0·64 mm.). The magnifying power was such that a change of one two-and-a-half-millionth part of an inch (or one hundred-thousandth of a millimetre) in the length of the rod under examination caused the image of the wire to move through about three-quarters of a scale division. More accurately, a scale division corresponds to 0·000018 mm.

The currents used were measured by one of Ayrton and Perry's commutator ammeters, and the accuracy with which the magnetising forces were estimated, though quite sufficient for the purpose of the experiments, does not claim to be very high.

II. "On Electrical Excitation of the Occipital Lobe and adjacent Parts of the Monkey's Brain." By E. A. SCHÄFER, F.R.S., Jodrell Professor of Physiology in University College, London. Received February 13, 1888.

The cortex of the occipital lobe has been explored electrically by Ferrier and by Luciani and Tamburini. In ten experiments upon monkeys Ferrier was unable to obtain any movements on stimulation of this part. Excitation of the angular gyrus produced conjugate deviation of both eyes to the opposite side, with sometimes an upward inclination when the anterior limb was stimulated, and a downward inclination when the electrodes were applied to the posterior limb. Luciani and Tamburini obtained only a conjugate deviation to the opposite side, without any constant upward or downward inclination, and they got a similar but less marked movement by stimulating the whole of the external surface of the occipital lobe.

The following are the results of my own observations :—Electrical excitation of the posterior limb of the angular gyrus, of the upper end of the middle temporal gyrus* (which is continuous with it) of the whole cortex of the occipital lobe (inclusive of its mesial and under aspects) and of the quadrate lobule, causes conjugate deviation of the eyes to the opposite side. The movement is not, however, in all cases a simple lateral deviation, but the lateral movement may be combined with an upward or downward inclination according to the part stimulated. Thus (1) excitation of a superior zone which comprises on the external surface the posterior limb of the angular gyrus, the upper (posterior) end of the middle temporal gyrus, and the part of the occipital lobe immediately behind the external parieto-occipital fissure, and on the mesial surface the quadrate lobule immediately in front of the upper end of the internal parieto-occipital fissure, and the occipital lobe for a short distance behind the upper end of that fissure, produces, besides the lateral deviation, a downward inclination of the visual axes which is sometimes—especially when the stimulation is applied at or near the mesial surface—so marked as greatly to obscure the lateral deviation.

(2.) Excitation of an inferior zone comprising the whole of the inferior surface of the lobe, the lower part of the mesial surface, and the posterior or lowermost part of the convex or external surface, produces, besides the lateral deviation, an upward inclination of the visual axes which, like the downward movement resulting from stimulation of the superior zone, may be so marked as partly to obscure the lateral deviation.

(3.) Excitation of an intermediate zone which comprises the greater part of the external surface (where it gradually broadens out laterally) and extends over the margin of the great longitudinal fissure to include a narrow portion of the mesial surface, produces neither upward nor downward inclination of the visual axes, but a simple lateral movement.

These zones are not sharply marked off from one another but merge gradually into one another, so that if the electrodes be applied near to the upper or lower limit of the intermediate zone there is produced a

* Excitation of the upper end of the superior temporal gyrus gives a similar result. Since this is commonly accompanied by a movement of the opposite ear, it is usually considered that subjective auditory sensations have been called up by the excitation.

slight downward or upward inclination accompanying or immediately following the lateral movement.

The upward inclination of the eyes is often accompanied by elevation of the upper lids, and the downward inclination by depression of these lids.

Simultaneous excitation of corresponding points on the two hemispheres by the same stimulus usually produces a struggle between the muscles producing the lateral movement, the eyes quivering, but not being directed more to one side than the other. On one occasion, however, when corresponding points of the mesial surfaces were simultaneously stimulated slight convergence of the optic axes was obtained.

If, as is highly probable, the movements of the eyes, which occur on excitation of the occipital lobe and adjacent parts, are the result of the production of subjective visual sensations, these effects of excitation of the several parts of that lobe and the adjoining portions of the brain would appear to indicate—1. A connexion of the whole visual area of each hemisphere with the corresponding lateral half of each retina. (This has already been ascertained to be the case from the result of removing the whole of the area on one side, bilateral homonymous hemianopsia being thereby produced.)

2. A connexion of the superior zone with the superior part of the corresponding lateral half of each retina.

3. A connexion of the inferior zone with the inferior part of the corresponding lateral half of each retina.

4. A connexion of the intermediate zone with the middle part of the corresponding lateral half of each retina.

If we imagine the visual areas of the two cerebral hemispheres to be united in the middle line we may conceive each retina as projected in its normal position over the united area. It will then at once appear that the upper and lower parts of both retinas will fall upon the corresponding parts of the united area, that the outer part of the left retina and the inner part of the right will fall upon the outer portion of the left side of the united area, and *vice versâ*, and that a vertical line bisecting each retina will fall along the line of union of the two cerebral visual areas. The parts concerned with direct or central vision will therefore correspond with a part of the mesial surface. And each pair of "identical points" of the retinas will correspond with one and the same spot of the cerebral surface.*

* A more detailed account of this investigation will appear in the April number of 'Brain.'

III. "A Comparison of the Latency Periods of the Ocular Muscles on Excitation of the Frontal and Occipito-Temporal Regions of the Brain." By E. A. Schäfer, F.R.S., Jodrell Professor of Physiology in University College, London. Received February 13, 1888.

Conjugate deviation of the eyes to the opposite side is produced by excitation of entirely different regions of the cerebral cortex. The parts which when electrically excited produce this movement are: (1) An area in the frontal region of the hemisphere which is included in the motor or psychomotor zone of authors;* (2) the superior temporal gyrus; (3) the upper end of the middle temporal gyrus; (4) the posterior limb of the angular gyrus; (5) the whole cortex of the occipital lobe including its mesial and under surfaces; (6) the quadrate lobule.

Of these parts, excitation of which produces this result (conjugate deviation of the eyes to the opposite side), one, viz., the frontal area, is distinguished from the rest by the fact that its removal produces paralysis of that movement. This fact has been seized upon by Ferrier as indicating an important functional difference, the movements in the one case being probably caused by the direct action of this part of the cortex upon the centre of origin of the nerves to the ocular muscles; but in all other cases by indirect action, the movement when, *e.g.*, the visual or auditory region is stimulated being the result of visual or auditory impressions (subjective sensations) being provoked in the brain by the excitation, and these impressions producing indirectly the action in question. Others have supported the view that in all cases the movement is the result of the setting up of subjective sensations, but that in the case of the frontal area these are tactile or are connected with the muscular sense.

It seemed to me that light would be thrown upon the question if the period of latent stimulation of the ocular muscles were accurately determined under exactly the same conditions for the frontal and posterior (temporal and occipital) areas respectively. The result of this determination, which I have made in a number of monkeys, is to show that the latent period is longer by some hundredths of a second in the case of stimulation of the occipital lobe, or of the superior temporal gyrus than when the frontal area is stimulated; thus indicating that in the former case the nervous impulses must be transmitted through at least one more nerve centre than in the latter.

* For the exact limits of this area see a paper, "Ueber die motorischen Rindencentren des Affengehirns," in 'Beiträge zur Physiologie, C. Ludwig gewidmet,' 1886.

It seemed probable that this additional centre would be the frontal area itself, but further experiments have proved that this is not the case—at least not necessarily so. For the movement is still obtained on exciting the occipital lobe, or the superior temporal gyrus, even after complete excision of the whole of the frontal area, and indeed of nearly the whole of the so-called motor region on both sides of the brain in front of the fissure of Rolando. It would seem, therefore, that under these conditions the additional centre must be looked for elsewhere—possibly in the grey matter of the corpora quadrigemina, or in the basal ganglia.*

In this investigation, as well as in that related in the preceding paper, I have received much valuable aid from my assistant, Mr. E. P. France, whose services I desire cordially to acknowledge.

The expenses have been defrayed by the Association for the Advancement of Medicine by Research.

Presents, March 1, 1888.

Transactions.

Boston:—American Academy of Arts and Sciences. Memoirs. Vol. XI. Part 5. No. 6. 4to. *Cambridge, Mass.* 1887. The Academy.

Canada:—Geological and Natural History Survey of Canada. Catalogue of Canadian Plants. Part 3. Apetalæ. 8vo. *Montreal* 1886. The Survey.

Essex Field Club:—The Essex Naturalist. No. 12. 8vo. *Buckhurst Hill* 1887. The Essex Field Club.

Liége:—Société Royale des Sciences. Mémoires. Sér. 2. Tome XIV. 8vo. *Bruxelles* 1888. The Society.

London:—Anthropological Institute. Journal. Vol. XVII. No. 3. 8vo. *London* 1888. The Society.

Geological Society. Quarterly Journal. Vol. XLIV. No. 173. 8vo. *London* 1888. The Society.

Royal College of Physicians. List of Fellows, &c. 1888. 8vo. *London.* The College.

Royal Microscopical Society. Journal. 1887. Part 6*a*. 1888. Part 1. 8vo. *London*; List of Members, 1888. 8vo. *London.* The Society.

Royal United Service Institution. Journal. Vol. XXXI. No. 142. 8vo. *London* 1888; List of Members. 1888. The Institution.

* The method employed and the more detailed results of these experiments will be published in an early number of the 'International Journal of Anatomy and Physiology.'

Transactions (*contiueed*).

Meriden :—Scientific Association. Transactions. Vol. II. 8vo. *Meriden, Conn.* 1887. The Association.

Pesth :—Königl. Ungarische Geologische Anstalt. Földtani Közlöny. Kötet XVII. Füzet 7-12. 8vo. *Budapest* 1887; Über Ungarische Porcellanerden; von Ludwig Petrik. 8vo. *Budapest* 1887; Mittheilungen über die Bohrthermen zu Harkány; von W. Zsigmondy. 8vo. *Pest* 1873.
The Institute.

Sydney :—Australian Museum. Descriptive Catalogue of the Medusæ of the Australian Seas. By R. von Lendenfeld. 8vo. *Sydney* 1887 (2 copies). The Museum.

Linnean Society of New South Wales. Proceedings. Vol. II. Part 3. 8vo. *Sydney* 1887; List of Contributions to the Proceedings, Ser. 1, Vols. I–X. 8vo. *Sydney* 1887.
The Society.

Turin :—R. Accademia delle Scienze. Atti. Vol. XXIII. Disp. 2–3. 8vo. *Torino* 1887–8. The Academy.

Burdett (H. C.) Burdett's Official Intelligence. 1888. 4to. *London.*
Mr. Burdett.

Carruthers (G. T.) The Sun's Great Waterfall. 8vo. *Umballa* 1887.
The Author.

Delaurier (E.) Recherches Expérimentales sur la Pondérabilité de l'Éther Universel. (MS.) Folio. [1888.] The Author.

Favaro (A.) Per la Edizione Nazionale delle Opere di Galileo Galilei. (Prospectus.) 8vo. *Firenze* 1888.
Ministero della Pubblica Istruzione.

Folmer (N.) Handleiding tot het Omschrijven der Platen: Vervolg. 8vo. *Groningen* 1887. The Author.

Ganser (A.) Alles reale Sein beginnt als Act eines intelligenten Wollens. 8vo. *Graz* 1888. The Author.

Gilbert (J. H.), F.R.S. Results of Experiments at Rothamsted on the Growth of Root-crops: a Lecture. 8vo. *Cirencester* 1887.
The Author.

Gowers (W. R.), F.R.S. A Manual of Diseases of the Nervous System. 2 vols. 8vo. *London* 1886, 1888. The Author.

Holden (E. S.) List of Recorded Earthquakes in California, Lower California, Oregon, and Washington Territory. 8vo. *Sacramento* 1887. The University of California.

Kops (J.) Flora Batava. Aflev. 279–80. 4to. *Leiden* [1887].
The Netherlands Legation.

Newton (A.), F.R.S. The Early Days of Darwinism. (From Macmillan's Magazine.) 8vo. [*London* 1888]. The Author.

Prestwich (J.), F.R.S. Geology: Chemical, Physical, and Stratigraphical. Vol. II. 8vo. *Oxford* 1888. The Author.

Sclater (P. L.), F.R.S., and W. H. Hudson. Argentine Ornithology: a Descriptive Catalogue of the Birds of the Argentine Republic. Vol. 1. 8vo. *London* 1888. The Authors.

March 8, 1888.

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

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

The following Papers were read:—

I. "On some New and Typical Micro-organisms obtained from Water and Soil." By GRACE C. FRANKLAND and PERCY F. FRANKLAND, Ph.D., B.Sc. (Lond.), F.C.S., F.I.C., Assoc. Roy. Sch. of Mines. Communicated by Professor T. H. HUXLEY, F.R.S. Received February 15, 1888.

(Abstract.)

In a previous communication,* the authors have given a detailed description of a number of micro-organisms—Bacilli and Micrococci—which they had obtained in the course of investigations on the distribution of micro-organisms in the atmosphere. The present paper deals similarly with a number of typical and characteristic micro-organisms which they have derived from various natural waters.

The authors refer to the forms which have been obtained from water by previous observers, more especially to the "peach-coloured bacterium," the "*Cladothrix dichotoma*," and the "*Crenothrix kühniana*," as well as to others which have been more recently isolated by means of the method of gelatine-plate cultivation.

The authors point out the striking difference between the aërial and aquatic micro-organisms, micrococci being the predominant forms amongst the former, whilst bacillar forms are almost exclusively present in water. In fact all the aquatic forms described are bacilli.

* "Studies on some New Micro-organisms obtained from Air," 'Phil. Trans.,' B, vol. 178, p. 257.

The chemical action which the several micro-organisms described exert upon certain solutions containing salts of ammonia and of nitric acid respectively has been investigated by one of the authors, with the result that whilst none of the forms in question were found to oxidise ammonia either to nitrous or nitric acids, several of them were found to exert a powerfully reducing action on nitrates, converting the latter into nitrites, others were without any action on the nitric acid, and others again caused the disappearance of an appreciable proportion of the nitric acid without production of a corresponding amount of nitrite. The authors point out that these differences in the behaviour of micro-organisms when introduced into solutions containing nitrates are capable of furnishing very important data for distinguishing between forms which otherwise present very close resemblance. Thus *Bacillus subtilis* and *Bacillus cereus*, previously described by them as closely resembling each other, can be easily distinguished by the behaviour which they respectively exhibit towards the nitrate-solution, for whilst both grow luxuriantly in this medium, the *Bacillus subtilis* has no action on the nitric acid which can be quantitatively recovered, the *Bacillus cereus*, on the other hand, powerfully reduces the nitrate with formation of nitrite.

The nitrate-solution employed for the purpose of these experiments contained potassium phosphate, magnesium sulphate, calcium chloride, calcium nitrate, invert sugar, peptone, and an excess of calcium carbonate.

The following is a brief account of the descriptions given of the various micro-organisms:—

Bacillus arborescens.—This is seen under a high power (× 1000 diameters) to be a slender bacillus giving rise to wavy threads, sometimes of considerable length. No spores were observed. In drop cultivations it is seen to be vibratory.

On gelatine plates it produces highly characteristic colonies. Under a low power (× 100 diameters) the centre is seen to consist of a thin axial stem with root-like branches from each of its two extremities, this stem thickens as growth proceeds, and the ramified extremities become so largely developed that the whole colony has the appearance of a wheat sheaf. The plate is slowly liquefied, and the periphery of the colony extends irregularly and to some distance from the centre, over the surface of the gelatine, giving rise to beautiful iridescent colours.

On potatoes it produces a fine deep-coloured orange pigment.

On nitrates it has no action in the solution employed.

Bacillus aquatilis.—This is a slender bacillus also giving rise to wavy threads. No spores were observed, and the individual bacilli are seen in drop cultivations to exhibit only an oscillatory motion.

On gelatine plates the contour of the colony becomes more and more irregular as they approach the surface; when liquefaction of the gelatine commences, which only takes place excessively slowly, convoluted bands of threads are seen to extend from the centre towards the periphery.

It grows with great difficulty in all the media employed with the exception of the aqueous solution, in which it grows abundantly, but does not convert the nitrate into nitrite.

Bacillus liquidus.—This is a short fat bacillus of very variable dimensions. In drop cultivations they are seen usually hanging together in pairs, and exhibit great motility.

It liquefies the gelatine, rapidly producing large circular depressions with almost clear contents on gelatine plates.

It produces a smooth shining expansion on agar-agar, and on potatoes a thick flesh-coloured pigment.

It reduces the nitrate powerfully in the aqueous solution employed.

Bacillus vermicularis.—This is a large bacillus with rounded ends, giving rise to extensive vermiform threads. It produces fine oval spores. In drop crystallisations it exhibits oscillatory movement only.

It powerfully reduces nitrates to nitrites.

Bacillus nubilus.—This is a fine slender bacillus, which gives rise to wavy threads. No spores were observed. In drop cultivations the isolated bacilli exhibit violent circular movements with but little motion of translation.

On gelatine plates the growth is very characteristic, nothing being visible but patches of cloudy expansions with, in some cases, a very faintly-defined centre. The gelatine rapidly becomes softened, and liquefaction soon follows.

In gelatine-tubes the same characteristic cloudy appearance is produced. Its growth in the aqueous solution described results in the reduction of a very small proportion of the nitrate to nitrite.

Bacillus ramosus.—This is a large bacillus much resembling *B. subtilis*, giving rise to long threads and spores, which are, however, rounder in shape than those of the latter organism. In drop cultivations the isolated bacilli exhibit very slight oscillatory movement.

The colonies on the gelatine plates are seen to consist of a cloudy centre with tangled root-like branches which extend in every direction. Later liquefaction of the gelatine takes place.

In gelatine the whole of the tube becomes impregnated with fluffy ramifications, later liquefaction takes place, and a tough pellicle forms on the surface.

When grown on potatoes, it forms a dry continuous surface expansion which is almost quite white.

It exerts a powerfully reducing action on nitrates in the solution employed.

Bacillus aurantiacus.—This is a short fat bacillus of very variable dimensions. No spores were observed. In drop cultivations the isolated bacilli are seen to be motile.

On gelatine plates it produces bright orange pin-heads, but on potatoes it gives rise to a magnificent brilliant red-orange pigment, which does not however extend far from the point of inoculation.

It reduces the nitrates to nitrites only very slightly in the solution employed.

Bacillus viscosus.—This is a short bacillus about three or four times as long as broad, occurring mostly in pairs. No spores were observed. It is exceedingly motile.

It very rapidly liquefies the gelatine, rendering it very viscid and colouring it green. On agar-agar the whole surface quickly assumes a green tint.

No reduction of the nitric acid takes place when grown in the aqueous solution described.

Bacillus violaceus.—This is a bacillus varying in thickness, sometimes appearing short and stout, but when grown on agar assuming a far more slender appearance; it also gives rise to short threads. Spore formation was observed. In drop cultivations they are seen to be motile, the movement being, however, principally vibratory and rotatory.

It produces on agar-agar a fine dark violet expansion.

It powerfully reduces nitrates to nitrites when grown in the aqueous solution employed.

Bacillus diffusus.—A fine slender bacillus recurring frequently in pairs, and giving rise also occasionally to long undulating threads. No spores were observed. In drop cultivations the bacilli are seen to execute vigorous oscillatory and rotatory movements, but do not traverse the field of the microscope.

On gelatine plates the colonies give rise on reaching the surface to a halo, which, extending from the centre, spreads to a considerable distance round, and is composed of a very thin and characteristically mottled expansion.

It slightly reduces the nitrates to nitrites when grown in the aqueous solution employed.

Bacillus candicans.—This bacillus varies very much in form in one and the same cultivation and still more in cultivations with different media; sometimes it has almost the appearance of a micrococcus, at other times it shows a tendency to grow into short threads. In drop cultivations the same variety of forms was observed, but in no case was anything but oscillatory motion visible.

When grown on gelatine plates it produces surface expansions much resembling drops of milk.

Although it grows abundantly in the aqueous solution employed, it exerts no reducing action on the nitric acid.

Bacillus scissus.—In form this organism much resembles *Bacillus prodigiosus.* In no case were spores observed. In drop cultivations it is seen to be very motile.

It produces pale light green surface expansions on gelatine plates which, under a low power (× 100 diameters), are seen to be of fine granular texture, the edge being much frayed out.

In tubes the gelatine and agar-agar become tinted green.

It powerfully reduces nitrates to nitrites in the solution employed.

Of the above, the first nine were derived from water, whilst the remaining three were obtained from garden soil.

The original descriptions are illustrated by drawings of the various micro-organisms as seen in microscopic preparations, and of the appearances to which they give rise in gelatine-plate and other cultivations.

II. "Further Observations on the Electromotive Properties of the Electrical Organ of *Torpedo marmorata.*" By FRANCIS GOTCH, M.A. Oxon., B.A., B.Sc. London. Communicated by Prof. J. BURDON SANDERSON, F.R.S. Received February 23, 1888.

(Abstract.)

In the present memoir the author details the results of further observations as to the electromotive properties of the electrical organ of Torpedo, the experiments being carried out in October, 1887, at the laboratory of the Société Scientifique d'Arcachon.

I. The first part of the work deals entirely with the phenomena of "irreciprocal conduction" in the organ of Torpedo, as described by du Bois-Reymond.

From du Bois-Reymond's experiments it would appear that the organ possesses the remarkable property of conducting an intense current of short duration, led lengthwise through its columns, better when the current is directed from its ventral to its dorsal surface than when directed the reverse way. The former direction coincides with that of the current of the shock of the organ, and is therefore termed by him "homodromous," the latter being opposite in direction, is termed "heterodromous." The evidence rests upon the value of the galvanometric deflections obtained when both currents are allowed to traverse a strip of organ and a galvanometric circuit. The deflections are markedly unequal, particularly when induced currents are used, the homodromous effect being always much greater than the

heterodromous. The homodromous current must therefore either encounter less resistance than the heterodromous, or its electromotive force must be suddenly strengthened, and that of the heterodromous current weakened, by the sudden establishment in the tissue of a new source of electromotive energy. The first is the view taken by Professor du Bois-Reymond.

(1.) The present rheotome experiments reveal (*a*) the new fact that the passage of such intense currents of short duration is always followed by an excitatory response (shock) in the tissue; (*b*) that if the intense current due to this response is allowed to affect the galvanometer as well as the induced or other exciting current, then by obvious algebraic summation the homodromous deflection must be much larger than the heterodromous; (*c*) and that when by means of a fast-moving rheotome the induction shock only is allowed to affect the instrument, no irreciprocity is found.

The author therefore assumes that the phenomena of irreciprocal conduction are in reality excitatory phenomena, the nature of which, from the methods of investigation used, have not been recognised.

(2.) The time relations of this response of the isolated strip of the organ to the direct stimulation by the traversing induction shock are now for the first time investigated, by means of the rheotome, and the influence of temperature and other conditions upon these is shown by experimental evidence.

II. The second part deals with entirely novel phenomena, namely, the excitation of the organ by the current of its own excitatory state. It is shown that in vigorous summer fish every response of the whole or part of the organ to a single excitation of its nerves is followed by a second response, due to the passage through its own substance of the intense current of the first response. In other words the shock of the organ excites its own nerve fibres and nerve endings, producing a feebler second shock, which in a similar manner evolves a feebler third shock; this a fourth, and so on.

The response of the isolated organ to nerve excitation is thus multiple; a primary, secondary, tertiary response following the application to the nerve of a single stimulus. Since all these responses produce currents similarly directed through the columns of the organ, each column during its activity must reinforce by its echoes the force of the primary explosion, both in its own substance and also in that of its neighbours.

III. "Contributions to the Anatomy of the Central Nervous System in Vertebrated Animals. Part I.—Ichthyopsida. Section I.—Pisces. Subsection III.—Dipnoi. On the Brain of the *Ceratodus Forsteri*." By ALFRED SANDERS, M.R.C.S., F.L.S. Communicated by Dr. GÜNTHER, F.R.S. Received February 23, 1888.

(Abstract.)

The brain of *Ceratodus* has the following general arrangement:—The membrane which represents the pia mater is of great thickness and toughness; there are two regions where a tela choroidea is developed: one where it covers in the fourth ventricle, and the other where it penetrates through the third ventricle and separates the lateral ventricles from each other.

The ventricles are all of large size, and the walls of the lateral ventricles are not completed by nervous tissue. The thalamencephalon and the mesencephalon are narrow, and the medulla oblongata is wide.

All the cranial nerves are present except the abducens and the hypoglossal. There is a large communicating branch between the trifacial and the vagus. The glossopharyngeal has no separate root, but is a branch of the vagus. The ganglion of the vagus is not the termination of the main trunk, but is an offshoot from the ramus lateralis; the ganglion gives off the branchial nerves and the ramus intestinalis; the ramus lateralis passing on without entering it.

In the minute structure of the dorsal part of the cerebrum there are four layers to be seen, externally a layer of finely granular neuroglia, with slight indications of radial striation; next a layer of larger sized cells; then another layer of neuroglia with fibrillæ having a tendency to a longitudinal direction; finally, a layer of rounded cells closely crowded together on the internal surface. The ventral part of the cerebrum has only two layers—the external of neuroglia and the internal of rounded cells.

The olfactory lobes resemble the cerebrum in structure; there is an internal layer of cells continuous with those of the cerebrum, and an external layer of glomeruli olfactorii, which seem as if they were the external layer of the cerebrum condensed; between the two there is a layer of longitudinal fibres on which fusiform cells are developed.

The optic lobes also consist of four layers; externally there is a layer of longitudinal fibrils derived from the optic tract; then a layer of smoothly granular neuroglia; then a layer of transverse fibrillæ which collect into a commissure in the central line at the dorsal surface; there are also fusiform and rounded cells sparingly scattered

through it; the internal layer contains cells mostly rounded. At the central line on the dorsal surface there is a ganglion of large cells resembling those of the optic lobe of the Plagiostomata.

The cerebellum is a mere bridge over the fourth ventricle, and its structure presents the usual number of layers; internally there is the fibrous layer which ultimately goes to form the crura cerebelli ad medullam; then the granular layer, the cells of which are of large size compared to those of the same layer in Teleostei and Plagiostomata; then comes a layer of Purkinje cells, the form and number of processes of which are not uniform; the external layer is the molecular, which consists of a coarsely granular network derived from the processes of the Purkinje cells, also a network of finer fibrils and many rounded cells.

In the spinal cord there are three columns of longitudinal fibres on each side in the white substance, viz., the ventral columns between the two ventral roots of the spinal nerves, the lateral columns between the dorsal and ventral roots, and the dorsal columns between the two dorsal roots; fibres of large size are scattered throughout the two former columns, but are collected principally in the ventral; the dorsal columns consist entirely of fibres of minute size.

The principal feature in the white substance is a fibre of gigantic dimensions which is situated on the summit of the ventral columns —one on each side; it consists of a common medullary sheath enclosing, where the fibre is largest, about forty to fifty axis-cylinders; these have the characteristics of the axis-cylinders of the ordinary fibres of the white substance, but have no separate medullary sheaths; this fibre is traceable throughout the spinal cord; commencing opposite the posterior end of the abdomen, it extends forward to a short distance behind the exit of the facial nerve; it varies in size and becomes of the greatest diameter near the posterior end of the medulla oblongata; its axes escape through the medullary sheath and join the longitudinal fibres of the ventral columns; near its anterior termination all the axes have escaped except one; at this point it bears a great resemblance to Mauthner's fibre in the Teleostei. This remaining fibre decussates with that of the other side a short distance behind the exit of the facial nerve and joins the root of that nerve.

In the grey substance of the spinal cord there are two series of ganglia—one in the ventral horn, which consists of multipolar cells often of very large size; they send processes into the ventral and lateral columns which often become the smaller-sized longitudinal fibres. The cells of the other series are of smaller size and are situated in the substantia gelatinosa centralis; they are smooth in outline and give off one or two processes; they probably have to do with the dorsal roots of the spinal nerves. Cells also of this kind occur at other places as in the fibræ rectæ, and in the field of the ventral columns.

The transverse commissures are—one in the spinal cord which passes through the substantia gelatinosa centralis over the central canal; another occurs on the ventral side of the anterior part of the medulla oblongata and corresponds to the commissura ansulata of Teleostei; it is connected with the commissure in the dorsal part of the optic lobes. Two other commissures are present corresponding respectively to the anterior and posterior commissures of the third ventricle of Mammalia.

There is no chiasma of the optic nerve visible externally; what there is of it is situated in the substance of the thalamencephalon.

The anterior root of the fifth nerve arises from a ganglion occupying a broad swelling in the lateral part of the grey matter of the floor of the fourth ventricle. The posterior root arises from the summit of the restiform bodies.

The facial passes backwards in a small tubercle at the junction of the floor of the fourth ventricle with the restiform bodies.

The acusticus arises from a bundle of fibres which are situated on the summits of the ventral columns, and appears to be a continuation forward of that part of the multiaxial fibre which has not decussated.

The vagus has five roots; they pass backward and enter in succession the same tubercle as, and to the outside of, the facial nerve; the three posterior roots are double, so that the vagus is equivalent to eight nerves, and consists entirely of dorsal roots.

Two nerves are given off from the ventral side of the medulla oblongata, each of which has two roots; they do not join the vagus but pass back some distance in the vertebral canal and emerge on a level with the exit of the dorsal roots of the spinal nerves.

The second and third spinal nerves supply the pectoral fin and pursue the course usually followed by the hypoglossal when that nerve is present in Teleostei.

The fibres of the ventral roots of the spinal nerves enter in a direction upward and forward toward the inner edge of the multiaxial fibre, between it and the central canal, and then passing over the dorsal edge of the same, are either lost in the grey substance of the ventral horn, join a process of one of the multipolar cells, or become one of the longitudinal fibres of the ventral column.

The brain of *Ceratodus* presents an embryonic condition in three respects, viz., first in the extreme size of the ventricles and in the tenuity of the substance of their walls; second, in the alternating origins of the dorsal and ventral roots; third, in the fact that the origins of the dorsal roots are close to the central line.

Compared to *Protopterus* it differs in the shape and the imperfection of the cerebral lobes, and in the fact of its having a well-developed rhinencephalon, but it agrees in the narrowness of the thalamencephalon and mesencephalon and in the breadth of the medulla oblongata, as also in the rudimentary character of the cerebellum.

Ceratodus agrees also with the Ganoids in the comparative narrowness of the mesencephalon and in the proportions of the cerebellum.

With the Plagiostomata it agrees in the structure of the optic lobes, both orders presenting a ganglion of large cells in the dorsal part. With the Teleostei it agrees in the multiaxial fibres which, a short distance anterior to its termination, resemble the Mauthner's fibres, also in the position and the fact of their decussation.

With *Petromyzon* it agrees in the structure of the tela choroidea which covers the fourth ventricle.

Presents, March 8, 1888.

Transactions.

Calcutta:—Indian Museum. A Catalogue of the Moths of India by E. C. Cotes and Col. C. Swinhoe. Part 2. 8vo. *Calcutta* 1887. The Trustees.

Cambridge:—Philosophical Society. Proceedings. Vol. II. Part 3. 8vo. *Cambridge* 1888. The Society.

Cambridge, Mass.:—Harvard University. Bulletin. No. 39. 8vo. 1888; Annual Report, Harvard College. 1886–87. 8vo. *Cambridge* 1888. The University.

Charleston:—Elliott Society of Science and Art. Proceedings. August 26, 1886, to July 28, 1887. 8vo. [*Charleston.*] The Society

Essex Naturalists' Field Club:—The Essex Naturalist. 1888. No. 1 and 2. 8vo. *Buckhurst Hill* 1888. The Club.

London:—Entomological Society. Transactions. 1887. Part 5. 8vo. *London* 1888. The Society.

Royal Institute of British Architects. Journal of Proceedings. Vol. IV. No. 9. 4to. *London* 1888. The Institute.

Marlborough:—Marlborough College Natural History Society. Report. No. 36. 8vo. *Marlborough* 1888. The College.

Paris:—École Normale Supérieure. Annales. Tome V. Nos. 1–2. 4to. *Paris* 1888. The School.

Société de Géographie. Bulletin. Tome VIII. Trim. 4. 8vo. *Paris* 1887. The Society.

Rome:—Reale Accademia dei Lincei. Annuario. 1888. 12mo. *Roma.* The Academy.

St. Petersburg:—Comité Géologique. Bulletin. (Russian.) Tome VI. No. 1. 8vo. *St. Petersburg* 1887. The Comité.

Vienna:—K. Akademie der Wissenschaften. Anzeiger. Jahrg. 1888. Nr. 1–3. 8vo. *Wien.* The Academy.

Ball (Sir R. S.), F.R.S. On the Plane Sections of the Cylindroid. Seventh Memoir on the Theory of Screws. 4to. *Dublin* 1887.
The Author.

Collins (Rev. H.) A Treatise on Nature. 8vo. *London* 1886.
The Author.

(Davidson (T.), F.R.S.) Account of the Unveiling of a Memorial to the late Dr. Davidson. From the *Brighton Herald*, February 18, 1888. (Two Copies.) Mr. E. Crane.

Mueller (Baron F. von), F.R.S. Iconography of Australian Species of Acacia. Decades 5–8. 4to. *Melbourne* 1887.
The Public Library, Melbourne.

Palagi (F.) Sulla Costituzione della Nebbia e delle Nubi. Nota seconda. 8vo. *San Marino* 1888. The Author.

Prince (C. L.) Summary of a Meteorological Journal, 1887, kept at Crowborough Observatory, Sussex. Folio. *Crowborough* 1888. The Author.

Sasse (E.) Das Zahlengesetz in der Weltgeschichte. 8vo. [1888.]
The Author.

Schäfer (E. A.), F.R.S. Experiments on Special Sense Localisations in the Cortex Cerebri of the Monkey. 8vo. *London* [1888].
The Author.

Woodward (A. S.) Note on the extinct Reptilian Genera Megalania (Owen), and Meiolania (Owen). 8vo. [*London*] 1888.
The Author.

March 15, 1888.

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

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

Professor Oliver Joseph Lodge (elected 1887) was admitted into the Society.

The following Papers were read:—

I. "On certain Mechanical Properties of Metals, considered in Relation to the Periodic Law." By W. CHANDLER ROBERTS-AUSTEN, F.R.S., Professor of Metallurgy, Normal School of Science, and Royal School of Mines, South Kensington. Received March 15, 1888.

(Abstract.)

The author points to the great industrial importance of the influence exerted by small quantities of metallic and other impurities on masses of metals in which they are hidden. He states that this is most marked in the case of iron, and that when Bergman discovered, in 1781, that the difference between wrought iron, steel, and cast iron depends on the presence or absence of a small amount of "graphite," he was astonished at the smallness of the amount of matter which is capable of producing such singular changes in the properties of iron. The evidence as to the importance of small quantities of impurity is quite as strong in other directions at the present day, as is shown by the statement of Sir Hussey Vivian, that one-thousandth part of antimony converts "best select" copper into the worst conceivable, and by the assertion of Mr. Preece, that "a submarine cable made of the copper of to-day," now that the necessity for employing pure copper is recognised, "will carry double the number of messages that a similar cable of copper would in 1858," when the influence of impurities in increasing the electrical resistance of copper was not understood.

Allusion is made to the effect of a small quantity of tellurium on bismuth. Commercially pure bismuth has a fracture showing brilliant mirror-like planes, but if one-thousandth part of tellurium be present the fracture is minutely crystalline. Specimens of such bismuth

were submitted to the Society. The author states that in his own experiments he has employed gold prepared by himself with great care, the purity of which has been recognised by M. Stas. A portion of this gold was recently used by Professor Thorpe in a determination of the atomic weight of gold. Gold was selected for the experiments for the following reasons:—It can be prepared of a very high degree of purity; it possesses considerable tenacity and ductility; the accuracy of the results of the experiments is not likely to be disturbed by the oxidation of the metal or by the presence of occluded gases; and the amount of impurity added to the gold can be determined with rigorous accuracy. The influence of small quantities of metallic impurity in rendering gold brittle has long been known, and is frequently referred to by the older metallurgists, especially by Geber, Biringuccio, and Gellert, and by Robert Boyle. The first systematic experiments on the subject were made by Hatchett at the request of the Privy Council, and were communicated to the Royal Society in 1803. Hatchett concluded that certain metals, even when present in so small an amount as the $\frac{1}{1900}$ part of the mass, will render gold brittle, and he stated that "The different metallic substances which have been employed in these experiments appear to effect gold in the following decreasing order:—1. Bismuth; 2. Lead; 3. Antimony; 4. Arsenic; 5. Zinc; 6. Cobalt; 7. Manganese; 8. Nickel; 9. Tin; 10. Iron; 11. Platinum; 12. Copper; 13. Silver." Mr. Hatchett did not, however, employ pure gold, and in his time the importance of submitting metals to mechanical tests was not understood.

The author then proceeds to describe the results of his own experiments, and he states that in selecting tenacity as the test to which the metal should be submitted with a view to ascertain the effect of the added matter, the following considerations presented themselves. W. Spring has built up alloys by compressing the powders of the constituent metals, and by pointing to the evidence of molecular mobility in *solid* alloys he has done much to show the close connexion which exists between cohesion and chemical affinity. Raoul Pictet considers that there is intimate relation between the melting points of metals and the lengths of their molecular oscillations, the length of the oscillation diminishing as the melting point rises; and, as Carnelley has pointed out, "We should expect that those metals which have the highest melting points would also be the most tenacious." It is known that the melting points of metals are altered by the presence of small quantities of foreign matter, and their cohesion is also thereby altered. The degree of cohesion may thus be investigated either by the aid of heat or by mechanical stress. It might be well to ascertain the amount of change in the melting point of gold produced by the presence of the different elements in small quantity, but, unfortunately, slight variations in high melting points are very difficult to determine with even approximate accuracy, and it appeared to be better to ascertain the effect of metallic and other impurities on the cohesion of the gold, as indicated by the amount of force externally applied in an ordinary testing-machine, and in that way to ascertain whether the effect of added metals is amenable to any known law.

The purest gold attainable has a tenacity of 7·0 tons per square inch, and an elongation of 30·8 per cent. on 3 inches. Professor Kennedy found that a less pure sample which contained 999·87 parts of gold in 1000, broke with a load of 6·29 tons per square inch; it had an elastic limit of 2·12 tons per square inch, and elongated 18·5 per cent. before breaking. In the following experiments only the purest gold that could be prepared was employed. The effect on the tenacity of gold produced by adding to it about 0·2 per cent. of various metals and metalloids is shown in the following table, in which the results are arranged according to the tensile strengths:—

Name of element added.	Tensile strength.	Elongation, per cent. (on 3 inches).	Impurity per cent.	Atomic volume of impurity.
	Tons per sq. in.			
Potassium........	Less than 0·5.	Not perceptible.	Less than 0·2	45·1
Bismuth.........	0·5 (about)	,,	0·210	20·9
Tellurium........	3·88	,,	0·186	20·5
Lead............	4·17	4·9	0·240	18·0
Thallium.........	6·21	8·6	0·193	17·2
Tin.............	6·21	12·3	0·196	16·2
Antimony	6·0 (about)	qy.	0·203	17·9
Cadmium........	6·88	44·0	0·202	12·9
Silver...........	7·10	33·3	0·200	10·1
Palladium........	7·10	32·6	0·205	9·4
Zinc.............	7·54	28·4	0·205	9·1
Rhodium........	7·76	25·0	0·21 (about)	8·4
Manganese.......	7·99	29·7	0·207	6·8
Indium	7·99	26·5	0·290	15·3
Copper..........	8·22	43·5	0·193	7·0
Lithium	8·87	21·0	0·201	11·8
Aluminium	8·87	25·5	0·186	10·1

Reasons are given for adding the comparatively large amounts of impurity (two-tenths per cent.), notwithstanding that even "traces" of certain metals would have produced very marked effects upon gold, and evidence is adduced to show that exact concordance in the respective amounts of matter added to the gold is not of much importance.

The testing-machine employed is of the form devised by Professor Gollner, and used by him at Prague. It is a double lever vertical machine working up to a stress of 20 tons.

The author points out that these results lead to the conclusion that the tenacity of gold is affected by the elements in the order of their atomic volumes, and he discusses the evidence in favour of this view at some length, pointing especially to the fact that while those elements, the atomic volumes of which are higher than that of gold, greatly diminish its tenacity, silver, which has nearly the same atomic volume as gold, hardly affects either its tenacity or its extensibility. He shows that, so far as the experiments have been conducted, not a single metal or metalloid which occupies a position at the base of either of the loops of Lothar Meyer's curve (which is a graphical representation of the periodic law of Newlands and Mendeléef) diminishes the tenacity of gold, while, on the other hand, metals which render gold fragile all occupy higher positions on Meyer's curve than gold does, and he urges that the relations between these small quantities of the elements and the masses of metal in which they are hidden are under the control of the law of periodicity, which states that "The properties of the elements are a periodic function of their atomic weights." Carnelley has given strong evidence in favour of supplementing the law as follows :—" The properties of *compounds* of the elements are a periodic function of the atomic weights of their constituent elements," and the question arises, May the law be so extended as to govern the relations between the constituent metals of alloys in which, as is well known, the atomic proportions are often far from simple?

The effect on gold of small but varying quantities of metals singly and in presence of other metals, demands examination, and their influence on the specific gravity of gold must be ascertained. Until this has been done no explanation as to the mode of action of elements with large atomic volumes will be attempted.

II. "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, 1888.

(Abstract.)

The first part of the paper gives details of the general arrangements made for observation—the selection of a site, the erection of the instruments and a hut to cover them; and refers to the unfavour-

able conditions under which the observations were made. The second part gives the results of the observations. These were of two kinds.

1. Before and after totality the order of appearance and disappearance of a number of bright lines in the spectrum of the chromosphere and inner corona was watched. The lines selected were those observed by Mr. Lockyer in the Egyptian eclipse of 1882, and the observations were undertaken with a view to the confirmation of his results.

The lines are denoted for convenience by small letters as follows:—

	λ		λ		λ
a	4870·4	*e*	4917·9	*h*	4932·5
b	4871·2	*f*	4919·6	*i*	4933·4
c	4890·0	*g*	4923·1	*k*	4956·5
d	4890·4			*l*	4970·0

With this nomenclature a table given by Mr. Lockyer in a short account of his results ('Roy. Soc. Proc.,' vol. 34, 1863, pp. 291, &c.) shows that lines *g* and *l* are seen by Tacchini in prominences, while *a*, *b*, *c*, *d*, *e*, *f*, and *k* are seen in spots.

Mr. Lockyer saw *g* and *i* 7 minutes before totality,
and in addition *k* and *l* 3 „ „
and all the lines .. 2 „ „

In my own observations I saw *g* 3 minutes before totality,
and in addition *i* 40 seconds „

while the moment of appearance of all the lines was indistinguishable from the commencement of totality.

After totality clouds obscured the sun for a short time; but on their clearing the visibility of *g* and *k* was noted; *i* could not be seen.

The three lines *g*, *i*, and *k* were extremely short, and did not appear to extend beyond the chromosphere before and after totality.

The unfavourable conditions under which the observations were made as compared with Mr. Lockyer's—with a low sun and through passing clouds, and an atmosphere charged with moisture which doubtless diminished the light in this region of the spectrum considerably—perhaps account in some measure for the striking difference in vividness of the phenomena. The solar activity was also much nearer minimum in 1886 than in 1882. As far as they go, however, the observations are confirmatory of Mr. Lockyer's, except in the visibility of the line *k* after totality. This line was not noted before totality, and it is possible that the observation may be spurious, although the evidence for it is as good as that for all the observations, which were found to be generally of a difficult character. The instrument used was a 6-inch refractor by Simms, with a grating spectroscope, the grating being $1\frac{1}{2}$ inch square, ruled with 17,000 lines to the inch. The second order of spectrum was used.

2. During totality I was directed to look for currents in the corona. I can only report a negative result. The structure of the corona appeared in a 4-inch refractor, with a power of 80, to be radial to the limb throughout, and no striking differences in special localities were noticed.

Appended to the paper are two drawings which do not attempt to give more than the distances to which the coronal rays extended in various directions. One was made by Mr. St. George with an opera glass, and the other by Lieutenant Smith with the naked eye; but in the latter case the observer's eyes had been specially covered fifteen minutes before totality, and the brighter portions of the corona were screened from him by a disk of angular diameter three times that of the moon. He consequently traced the rays much further than Mr. St. George, though, allowing for this difference in conditions, the drawings are fairly accordant.

III. "On the Ultra-Violet Spectra of the Elements. Part III. Cobalt and Nickel." By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received February 27, 1888.

(Abstract.)

The authors compare the results obtained by the Rutherfurd grating which they used in measuring the wave-lengths of the iron lines with those obtained with the larger Rowland's grating used for measuring the wave-lengths recorded in this paper, and find them closely concordant. They next compare the measures of wave-lengths of the cadmium lines obtained by them by means of a plane Rowland's grating and a goniometer with an 18-inch graduated circle with those obtained by Bell with a large concave grating of 20 feet focal length. The result of the comparison is that the plane grating gives measures which agree very closely with those given by the concave grating, while the former gives more light and is better for complicated spectra, such as those described in this paper, because the overlapping spectra of different orders are not all in focus together as they are when a concave grating is used.

The authors give a list of 580 ultra-violet lines of cobalt and 408 lines of nickel. They find a certain general resemblance of the two spectra, but no such exact corrrespondence as the close chemical relationship of the two metals would render probable. They point out that the coincidences of lines of the two metals are hardly, if at all, more in number than would have been the case if the distribution of the lines had been fortuitous. They give a map of each spectrum to the same scale as Ångström's normal solar spectrum.

IV. "A Class of Functional Invariants." By A. R. FORSYTH, M.A., F.R.S., Fellow of Trinity College, Cambridge.

Received March 7, 1888.

(Abstract.)

The memoir is occupied with the investigation of a class of functional invariants, constituted by combinations of the partial differential coefficients of a function of more than one independent variable. As the number of independent variables is limited to two, partly for the sake of conciseness, the general definition of such an invariant is that it is *a function ϕ of the partial differential coefficients of a dependent variable* z *with regard to* x *and* y, *such that when the independent variables are changed to* X *and* Y, *and the same function* Φ *is formed with regard to the new variables, the relation*

$$\Phi = J^m \phi$$

is satisfied, where

$$J = \frac{\partial(x, y)}{\partial(X, Y)}$$

The transformations for which detailed results are given are of the homographic types:

$$\frac{x}{\alpha_1+\beta_1 X+\gamma_1 Y} = \frac{y}{\alpha_2+\beta_2 X+\gamma_2 Y} = \frac{1}{\alpha_3+\beta_3 X+\gamma_2 Y^3},$$

so that

$$J = \begin{vmatrix} \alpha_1, & \alpha_2, & \alpha_3 \\ \beta_1, & \beta_2, & \beta_3 \\ \gamma_1, & \gamma_2, & \gamma_3 \end{vmatrix} (\alpha_3+\beta_3 X+\gamma_3 Y)^{-3}.$$

The characteristic properties of such invariants are—

(i.) Every invariant is explicitly free from the variables z, x, y, but necessarily contains p and q;

(ii.) It is homogeneous in the differential coefficients of z, and is of uniform and the same grade in differentiations with regard to each of the independent variables;

(iii.) It is symmetric or skew symmetric with regard to these differentiations;

(iv.) It satisfies four form-equations, viz. :—

$$\Delta_1\phi = q\frac{\partial\phi}{\partial p} + 2p\frac{\partial\phi}{\partial r} + 2\left(t\frac{\partial\phi}{\partial c} + 2s\frac{\partial\phi}{\partial b} + 3r\frac{\partial\phi}{\partial a}\right) + \dots\dots = 0,$$

$$\Delta_2\phi = p\frac{\partial\phi}{\partial q} + 2q\frac{\partial\phi}{\partial t} + 2\left(r\frac{\partial\phi}{\partial b} + 2s\frac{\partial\phi}{\partial c} + 3t\frac{\partial\phi}{\partial d}\right) + \dots\dots = 0,$$

$$\Delta_3\phi = q\frac{\partial\phi}{\partial s} + 2s\frac{\partial\phi}{\partial r} + t\frac{\partial\phi}{\partial s} + 3b\frac{\partial\phi}{\partial a} + 2c\frac{\partial\phi}{\partial b} + c\frac{\partial\phi}{\partial d} + \dots = 0,$$

$$\Delta_4\phi = p\frac{\partial\phi}{\partial s} + 2s\frac{\partial\phi}{\partial t} + r\frac{\partial\phi}{\partial s} + 3c\frac{\partial\phi}{\partial d} + 2b\frac{\partial\phi}{\partial c} + a\frac{\partial\phi}{\partial b} + \dots = 0,$$

and two index-equations, viz.:—

$$3\lambda\phi = 2p\frac{\partial\phi}{\partial p} + q\frac{\partial\phi}{\partial q} + 4r\frac{\partial\phi}{\partial r} + 3s\frac{\partial\phi}{\partial s} + 2t\frac{\partial\phi}{\partial t} + \dots\dots$$

$$3\lambda\phi = p\frac{\partial\phi}{\partial p} + 2q\frac{\partial\phi}{\partial q} + 2r\frac{\partial\phi}{\partial r} + 3s\frac{\partial\phi}{\partial s} + 4t\frac{\partial\phi}{\partial t} + \dots\dots,$$

in the last two of which λ is the index, an integer determinable from the form of ϕ by inspection. In these equations p and q are the partial differential coefficients of the first order; r, s, t those of the second order; a, b, c, d those of the third order; and so on.

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. By means of the solutions of the equations $\Delta_1\phi = \Delta_2\phi = \Delta_3\phi = \Delta_4\phi = 0$, considered as simultaneous partial equations, and by using the remaining equations, the following propositions relating to irreducible invariants in a single dependent variable z are established:—

Invariants can be ranged in sets, each set being proper to a particular rank;

There is no invariant proper to the rank 1, and there is one, viz., $q^2r-2pqs+p^2t$, proper to the rank 2;

There are three invariants proper to the rank 3;

For every 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 partial differential coefficients of order n.

Every invariant can be expressed in terms of this aggregate of irreducible invariants; and the expression involves invariants of rank no higher than the order of the highest differential coefficient which occurs in that invariant.

A special class of invariants, proper to ranks in numerical succession, is given by combinations of A_0, A_1, A_2, ... where—

$$A_0 = (r, s, t \between q, -p)^2,$$

$$A_1 = (a, b, c, d \between q, -p)^3,$$

$$A_2 = (e, f, k, h, i \between q, -p)^4, \ldots \ldots ;$$

and the combinations are such that, when the transformations

$$A_{m-2} = m\,!\,(m-1)\,C_{m-2}$$

are effected, the resulting forms are the same combinations of the quantities C as the leading coefficients of the fundamental covariants of a binary quantic.

Some of the properties of the irreducible invariants involving differential coefficients of two dependent variables z and z' are obtained; and in particular it is shown that there is a single simultaneous irreducible invariant, $pq'-p'q$ proper to the rank 1, and that there are four such invariants proper to the rank 2.

The theory of eduction is next considered. A number of eductive operators similar to $A_0^{-\frac{1}{2}}\left(q\frac{\partial}{\partial x} - p\frac{\partial}{\partial y}\right)$ are given; such an operator, applied to an absolute invariant, educes another absolute invariant. Some illustrations are given, and some results, the analogues of reversor operations, are obtained by means of successive educts.

Finally it is shown that the theory of binary forms can be partly connected with the theory of functional invariants. The equations $\Delta_3\phi = 0 = \Delta_4\phi$ are satisfied by $A_0, A_1, A_2, \ldots$, so that these quantities may be regarded as a succession of binary quantics in q and $-p$ as variables; and the same equations are characteristic of the simultaneous concomitants of such quantics. The functional invariants can therefore be expressed in terms of these simultaneous concomitants.

Presents, March 15, 1888.

Transactions.

Batavia :—Bataviaasch Genootschap van Kunsten en Wetenschappen. Tijdschrift voor Indische Taal-, Land- en Volkenkunde. Deel XXXII. Aflev. 2. 8vo. *Batavia* 1888. The Society.

Cambridge, Mass. :—Harvard College. Museum of Comparative Zoology. Bulletin. Vol. XIII. No. 6. 8vo. *Cambridge* 1887. The Museum.

Dublin :—Royal Irish Academy. Proceedings (Polite Literature and Antiquities). Vol. II. No. 8. Ditto (Science). Vol. IV. No. 6. 8vo. *Dublin* 1888; Transactions. Vol. XXIX. Parts 1–2. 4to. *Dublin* 1887; "Cunningham Memoirs." No. 4. 4to.

Transactions (*continued*).

Dublin 1887; List of Papers published in the Transactions, Cunningham Memoirs, and Irish Manuscript Series of the Academy, 1786–1886. 4to. *Dublin* 1887. The Academy.

Frankfurt-am-Main :—Senckenbergische Naturforschende Gesellschaft. Abhandlungen. Band XV. Heft 1. 4to. *Frankfurt-am-Main* 1887. The Society.

Leipzig :—Königl. Sächsische Gesellschaft der Wissenschaften. Berichte. (Philol.-Hist. Classe.) Band XXXIX. Hefte 4–5. 8vo. *Leipzig* 1888. The Society.

London :—British Association. Report. 1887. 8vo. *London* 1888. The Association.

Odontological Society of Great Britain. Transactions. Vol. XX. No. 4. 8vo. *London* 1888. The Society.

Manchester :—Geological Society. Transactions. Vol. XIX. Parts 14–15. 8vo. *Manchester* 1888. The Society.

Mexico :—Sociedad Científica "Antonio Alzate." Memorias. Tomo I. Núm. 1–5. 8vo. *México* 1887–8. The Society.

Philadelphia :—American Philosophical Society. Proceedings. Vol. XXIV. No. 126. 8vo. *Philadelphia* 1887. The Society.

Stockholm :—Kongl. Vetenskaps-Akademie. Öfversigt. Årg. XLV. No. 1. 8vo. *Stockholm* 1888. The Academy.

Observations and Reports.

Bombay : — Magnetical and Meteorological Observatory. Brief Sketch of the Meteorology of the Bombay Presidency. 1886–87. Folio. *Bombay* 1887. The Observatory.

Brisbane :—Colony of Queensland. Report of Census, 1886, with accompanying Maps. Folio. *Brisbane* 1887; Report of the Registrar of Friendly Societies. 1887. Folio. *Brisbane.* The Government of Queensland.

Cadiz :—Instituto y Observatorio de Marina de San Fernando. Anales. Observaciones Meteorológicas. Año 1886. 4to. *San Fernando* 1887. The Observatory.

Calcutta :—Indian Meteorological Memoirs. Vol. III. Part 2. 4to. *Calcutta* 1887; Report on the Administration of the Meteorological Department of the Government of India, 1886–87. 4to. [*Calcutta*] 1887; Meteorological Observations recorded at Six Stations in India. September, 1887. Folio. [*Calcutta.*] The Meteorological Office, Calcutta.

Cambridge, Mass. :—Astronomical Observatory of Harvard College. Annals. Vol. XIII. Part 2. 4to. *Cambridge* 1888. Annual Report, 1887. 8vo. *Cambridge.* The Observatory.

Observations, &c. (*continued*).

Dun Echt :—Observatory. Circulars. Nos. 152–53. 4to. *Dun Echt* 1888. The Earl of Crawford, F.R.S.

India :—Geological Survey. Memoirs. Vol. XXIV. Part 1. 8vo. *Calcutta* 1887. Memoirs (Palæontologia Indica). Ser. 10. Vol. IV. Part 2. 4to. *Calcutta* 1887; A Manual of the Geology of India. Part 4. 8vo. *Calcutta* 1887. The Survey.

Melbourne :—Observatory. Report, 1887. Folio. *Melbourne.* The Observatory.

Milan :—Reale Osservatorio Astronomico di Brera. Osservazioni. Anno 1887. 8vo. [*Milano.*] The Observatory.

Rome :—Pontificia Università Gregoriana. Bullettino Meteorologico. Vol. XXVI. Num. 9. 4to. *Roma* 1887. The University.

Transit of Venus. Report of the Committee appointed by the Government to superintend arrangements for the Observation of the Transit of Venus, 1882. Folio. (Two copies.) *London* 1887. The Admiralty and the Treasury.

Washington :—U.S. Naval Observatory. Observations, 1883. 4to. *Washington* 1887. The Observatory.

March 22, 1888.

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

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

The Right Hon. Charles Douglas Richard Hanbury-Tracy, Lord Sudeley, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following papers were read :—

I. "On the Skull, Brain, and Auditory Organ of a new Species of Pterosaurian (*Scaphognathus Purdoni*) from the Upper Lias, near Whitby, Yorkshire." By E. T. NEWTON, F.G.S., F.Z.S., Geological Survey. Communicated by Dr. ARCHIBALD GEIKIE, F.R.S. Received March 1, 1888.

(Abstract.)

The fossil Pterodactyl skull which is the subject of this communication was obtained from the Upper Lias of Lofthouse, near Whitby, by the Rev. D. W. Purdon, of Wolverhampton. It is the first Pterodactyl found in the Yorkshire Lias, and is a new form, allied to the Continental Jurassic species *Scaphognathus* (*Pterodactylus*) *crassirostris* of Goldfuss. The structure of the skull, including the back, base, and palatal regions, is better shown than in any previously discovered specimen; and in addition to this the brain and parts of the auditory organs have been exposed.

In its present condition the skull is about five and a half inches long; but apparently about two inches of the front are wanting. The elongated snout gives the skull a very bird-like appearance; but its most striking features are the five apertures, surrounded by bone, seen on each side. The orbit is the largest of these apertures; in front of this, and next in size, is the ant-orbital fossa; still further forward is the somewhat smaller external nostril. Behind the orbit is the temporal space, divided by a bony bar into the supra- and infratemporal fossæ. The premaxillæ are united to form the prenasal part of the snout, and send backwards an upper median process which meets the frontals between the orbits. The maxilla is not clearly divided from the premaxilla; but there can be no doubt that

the bone separating the nasal aperture from the ant-orbital fossa is a process of the maxilla. Alveoli for four teeth are preserved on each side; but it is not quite certain whether they all belong to the premaxillæ.

On the upper surface of the skull are to be seen the nasals and prefrontals, on each side of the premaxillary process. The frontals form the upper boundaries to the orbits and are confluent posteriorly with the parietals. The supra-occipital region has been broken away. Strong buttresses extend outward from the postfrontal and parietal regions to form the supra-temporal bar. There is on each side a large lachrymal bone forming the greater part of the upper and hinder boundary of the ant-orbital fossa. The jugal and quadrato-jugal are of a somewhat unusual form; the former bounding the lower half of the orbit, and the latter enclosing in an open V the greater part of the infra-temporal fossa. The quadrate is a wide but thin plate seen chiefly at the back of the skull. The base of the cranium is remarkable for its depth and extreme antero-posterior flattening; and viewed from behind, a pair of long rods are seen extending from its lower margin, one on each side, to the inner angles of the quadrates. These bones are regarded as the homologues of the basi-pterygoid processes of the sphenoid, such as are seen in some lizards and birds, as for example in the Chameleon and Emu.

From the point of junction of the quadrate and basi-pterygoid process a bone runs along the palate, and dividing anteriorly forms the hinder boundary of the internal nostril, its outer portion joining the maxilla and its inner being continuous with a median bone occupying the position of a vomer. This bony bar, it is thought, represents the palatine and pterygoid bones, and its relations agree better with those of the lizard than with those of the bird, seeing that it does not come into such close contact with the base of the skull as it does in birds, but is thrust outwards by the long basi-pterygoid process.

The back of the skull is essentially lacertilian. A large paroccipital bone extends outwards from the sides of the foramen magnum, and its distal end expanding, embraces the upper part of the quadrate. The relation which the base of the paroccipital bears to the semicircular canals shows that it must be chiefly formed by the opisthotic element, as Prof. W. K. Parker has shown to be the case in lizards, and not by the exoccipital as it is in birds.

By removing the frontal and parietal bones of the left side, a cast of the brain cavity has been exposed, which there can be no doubt represents the form of the brain, just as closely as does that of a bird's cranial cavity. In proportion to the size of the entire skull, the brain of this Pterodactyl is very small, being not more than one-eighth of its length. Each cerebral lobe is oval in shape, and about as thick as it is wide. The olfactory lobe is small. Behind the cerebrum is a pair of large optic lobes, occupying a prominent position on the sides of the brain, and extending upwards well to the upper surface, but not meeting above in the middle line. The region of the cerebellum has been broken away, and its exact form therefore is somewhat uncertain; but judging from portions which remain, it is tolerably clear that it extended between the optic lobes, and may have reached as far forwards as the cerebrum. Attached to the side of the medulla oblongata is a large flocculus, such as occurs in this position in birds.

It was the finding of the flocculus which led to the discovery of some parts of the auditory apparatus. On clearing away the stone in this region, a small tube filled with matrix was found arching over the pedicle of the flocculus and dipping down between it and the optic lobe. This tube occupies the position of the anterior vertical semicircular canal in the goose. By tracing the canal backwards and downwards it was found to join another similar tube forming an arch behind the flocculus, that is, in just the position of a posterior vertical semicircular canal. By careful excavation below the flocculus, a portion of a third tube was found, arching outwards in a horizontal plane, and this is believed to be the external semicircular canal.

The similarity between the base of the fossil skull and that of the Chameleon led to the inference that the fenestra ovalis would be found to be similarly placed in both, and by clearing away the matrix from the orbit and temporal fossa this inference was proved to be correct. The form and relations of the quadrate bone make it highly probable that this Pterosaurian had no ear-drum.

A comparison of this fossil with the skulls of known Pterosauria, leaves no doubt that it is more nearly related to the *Scaphognathus (Pterodactylus) crassirostris* than to any other species, but as it differs from that form, and is evidently new, it is to be named specifically *Scaphognathus Purdoni*.

The Pterosaurian skull, as exemplified by this Lias fossil, resembles more the Lacertilian than any other type of Reptile skull; and seeing that the skulls of birds and lizards are in many points very similar, one is not surprised to find in this fossil characters which are also found in both these groups. In considering, therefore, the relation which the Pterosaurian skull bears to those of birds and lizards, the characters should be especially noticed which serve to distinguish between the two groups, thus:—

1. In birds the brain-case is larger in proportion to the size of the skull than it is in lizards.

2. The quadrate, pterygoid, and palatine bones are movable on the skull in birds; but more or less fixed in lizards.

3. In birds the hinder end of the palatine and front end of the pterygoid are brought into close relation with the rostrum of the sphenoid. This is not the case with lizards.

4. The orbit is rarely completed by bone in birds, and never by the jugal. In lizards the orbit is surrounded by bone, and the jugal forms part of it.

5. In birds there is no prefrontal bone, while it is always present in lizards.

6. No bird has a supratemporal bar of bone, but it is always developed in lizards.

7. In lizards the paroccipital process is large and formed by the opisthotic. In birds the paroccipital is small and formed by the exoccipital.

8. In birds the bones of the cranium are early ankylosed; in lizards they nearly always remain separate.

9. Birds have the premaxillæ large and united into one bone, in lizards they are usually small.

10. The ant-orbital fossa which is present in birds is only occasionally present in lizards.

11. In birds there is always a lower temporal bar of bone extending from the maxilla to the quadrate. This bar is incomplete in all lizards except *Sphenodon*, although well developed in other reptiles.

The skull of *Scaphognathus Purdoni* agrees with lizards in the first seven of the above characters; and with birds in those numbered 8, 9, 10. Number 11 need not be considered, as it can scarcely be regarded as distinctive. The greater importance of the first seven characters makes it clear that in the structure of the skull, *S. Purdoni* most nearly resembles the Lacertilia.

The brain of *Scaphognathus Purdoni* agrees with that of reptiles in its relatively small size; while the separation of the optic lobes by the cerebellum and the meeting of the latter with the cerebrum, as well as the possession of a distinct flocculus, are important points in which it resembles the brain of the bird. On the other hand the form of the optic lobes is unlike that of any living bird.

The brain of the American fossil bird, *Hesperornis*, shows a striking resemblance to that of *Scaphognathus Purdoni*, for not only is it proportionally smaller than in recent birds, but the relation of the cerebellum and cerebrum to the optic lobes is very similar.

The facts above stated seem to show that the Pterosauria are related to the birds in the form of the brain, and to the lizards in the structure of the skull. This, however, does not constitute the Pterosaurian a transitional form between birds and reptiles, in the sense of the Pterosauria having been derived from reptiles, or of the birds having been derived from Pterosauria; but rather points to *Aves*, *Pterosauria*, and *Reptilia* having been derived from some common ancestral type. These relationships may be thus indicated, taking only a few of the characters of each.

Lizard.	*Pterosaurian.*	*Bird.*
Cerebellum small, optic lobes meeting, paroccipital formed chiefly by the opisthotic.	Cerebellum large and between optic lobes, paroccipital formed chiefly by the opisthotic.	Cerebellum large and between optic lobes, paroccipital formed chiefly by exoccipital.

Ancestral Type.

Cerebellum small, optic lobes meeting, paroccipital small, and formed by both exoccipital and opisthotic.

II. "The Atoll of Diego Garcia and the Coral Formations of the Indian Ocean." By G. C. Bourne, B.A., F.L.S., Fellow of New College and Assistant to the Linacre Professor in the University of Oxford. Communicated by Professor E. Ray Lankester, F.R.S. Received March 12, 1888.

[Plate 4.]

The whole of the following paper was planned and a great part of it was written when Captain Wharton's letter appeared in 'Nature,' on Feb. 23. Captain Wharton has anticipated my objections to the theory put forward by Mr. Murray in explanation of the formation of atolls and barrier reefs, and has suggested that the growth of corals on the periphery of a submerged bank is sufficient to explain the elevated rim of a barrier reef or atoll, and the contained lagoon channel or lagoon. In this I cordially agree with him, and can only express my satisfaction that so eminent an observer should have arrived, after an extended study, at conclusions nearly identical with mine. In accounting for the luxuriance of coral growth upon the peripheral slopes of an atoll, I differ slightly from Captain Wharton, and the publication of his letter has led me to extend and modify the plan of the latter half of this paper, in order to show more clearly the points in which I agree with and those in which I differ from him. I may take this opportunity of expressing my thanks to Captain Wharton for his kindness in sending me notes on the structure of the Cosmoledo and Farquhar groups, and to Dr. S. J. Hickson who has given me the benefit of his experience in N. Celebes.

In my visit to Diego Garcia in 1885 I took with me, among other works, the splendid papers of Alex. Agassiz on the Tortugas and Florida reefs, and diligently compared all his observations with structures existing at Diego Garcia. I was much struck by the analogy between the formations, but I also found differences in the stratification of rocks, &c., which led me to the conviction that there has been a very small amount of elevation in the Chagos group, which certainly has not been the case in the Florida reefs. This conviction was shared by an old resident on Diego Garcia, M. Spurs, a naturalist who has devoted many years to the study of coral reefs.

A reference to the map will show that Diego Garcia is a typical atoll; a narrow strip of land varying in width from a mile to 30 yards, nearly completely encircles a lagoon of irregular shape. The lagoon is open to the ocean towards the north-west, its mouth being divided by three small islets into four channels, of which three are sufficiently deep to allow ships to enter the lagoon. The three islands are known by the names of Bird Island, Middle Island, and East Island (Ile des Oiseaux, Ile du Milieu, and Ile de l'Est), the last named being the largest of the three, and the one on which I spent much of my time during my visit. The whole of the land composing the atoll is very low; the highest point in the island is not more than 30 feet above the level of high tide, and this height, which is quite exceptional, is due to the accumulation of great heaps of sand through the action of the S.E. trade winds which blow with considerable strength for more than one-half of the year. Diego Garcia is the southernmost atoll of the Chagos group; it lies in S. lat. 7° 26′, E. long. 72° 23′, and forms the last of the great chain of coral formations reaching from the Laccadive Islands through the Maldives to the Chagos group. To its south-west lie the submerged atoll-shaped reefs known as Pitt's Bank and Centurion's Bank, to its north lies the huge submerged atoll known as the Great Chagos Bank. It is an interesting fact that throughout the Laccadive, Maldive, and Chagos groups there is no instance of a fringing or of a barrier reef; nothing but coral structure rises above the waves, all the islands are atolls; none of these are upraised, but there are several submerged banks. The existence of this long line of atolls seemed to be one of the strongest arguments in favour of Darwin's theory on the formation of coral reefs. If the depths given in Stieler's hand-atlas are correct, the three groups stand on a submarine bank lying 1000 fathoms below the surface in an ocean of an average depth of 2000 fathoms.

In Diego Garcia the nature of the soil varies considerably from place to place. In some localities it consists of nothing else than bare coral rock upon the surface of which coral boulders are scattered about; in other places it is composed wholly of calcareous sand, and one may dig down for 6 or 8 feet without finding coral rock. It is obvious after a short examination that some parts of the land are older than others, and that the great strip of land was formerly a series of disconnected islets which have since been joined together by the accumulation of sand and coral *débris* between them. In the older parts of the island, which have apparently been covered with vegetation for a considerable period, a thick peaty mould has been formed by the decay of fallen leaves and stems of trees and shrubs.

Throughout the island the outer or seaward shore is higher than the inner or lagoonward shore, owing to the pile of coral boulders thrown up in the form of a low rampart along the former by the action of the waves. In most places a flat reef extends fully 60 yards seaward of the rampart; and this reef is just uncovered at low spring tides. As a rule the inner shore slopes gently down into the lagoon for some distance and then pitches down rather suddenly to a depth of 10 or 12 fathoms, but in some places there is a depth of 6 or 8 fathoms close up to the inner shores. Marshy pools of fresh or brackish water are found in the centre of the strip of land on the S.E. and W. sides of the island; into these the sea enters in many cases during the highest spring tides, and at the S.E. and S. ends of the island it has established permanent breaches into some of these pools, through which the tide runs in and out regularly from the lagoon. Thus there are formed sheets of water like secondary lagoons within the strip of land; these are known on the island by the name of *barachois*, and they are of some importance when one comes to consider the amount of change which is continually going on in the island.

Externally the shores slope away very rapidly to considerable depths, the sounding line giving depths of 250 fathoms and upwards at a distance of a few hundred yards from the edge of the reef, excepting at Horsburgh Point at the S.E. side, where a depth of 45 fathoms is found at a distance of 1 mile from the shore. Unfortunately I had not the apparatus wherewith to make a series of sectional soundings outside the island, nor if I had had the apparatus should I have had the means of making use of it. The depths within the lagoon have been accurately determined by H.M.S. "Rambler" in 1885; they vary up to 19 fathoms. After a stay of two or three months on the island one cannot fail to be impressed with the immense amount of change which is continually in progress. Large masses of sand are in the space of a month deposited in one spot to be swept away during the next month and deposited in another. Everywhere there is evidence that the sea has encroached upon the land or that the land has in its turn gained upon the sea. In one place numerous dead and fallen coco-nut palms show where old established land has been carried away; in an adjoining spot tracts of sand, either bare or covered with a scanty growth of young shrubs, show where the combined action of wind and waves has added a new piece to the island. Within the lagoon the currents are constantly changing in force and direction, and their every change affects the growth of coral in their track. In estimating the structure of the atoll these changes should be kept in mind, although their complexity makes it far more difficult to arrive at a correct conclusion.

In the course of my investigations I learnt to distinguish the following kinds of coral rock formed by the action of the waves or wind or both combined.

Firstly, *reef rock,* a tolerably homogeneous mass of compacted coral *débris,* the component parts of which are so thoroughly infiltrated with carbonate of lime held in solution in the sea-water that the masses and fragments of coral composing the rock are rarely distinguished from one another. This form of rock exhibits a fine horizontal stratification; it is invariably formed under the sea or between tide marks.

Secondly, *boulder rock,* formed just above high tide mark by means of the masses of coral which are transported across the reef by the waves and are piled up to form the low rampart already alluded to. The interstices of the boulders are soon filled up with coral *débris* and sand, and are cemented together by the spray. Such rock is only formed on the seaward shores and invariably shows a stratification dipping downwards towards the sea.

Thirdly, *shingle rock,* which may be of two kinds. The first kind is horizontally stratified and is scarcely distinguishable from reef rock, except in its finer texture; it is formed below water or between tide marks by the agglomeration of small pieces of broken coral, among which are included numerous shells of molluscs, remains of crustacea, echinoderms, &c., and in the more sheltered parts of the lagoon it may include considerable masses of dead madrepores imbedded in their natural position in the rock. This rock is of looser texture than the reef rock. The second kind of shingle rock is formed above high-water mark by the action of the waves. It is entirely composed of small fragments and exhibits a fine stratification dipping seawards at an angle.

Lastly, there is the *sand rock* formed above water by the action of the wind. Wherever masses of fine sand are piled up within reach of the spray they are gradually compacted, and form a friable rock, the stratification of which dips seaward.

In many parts of the island I observed that the land was composed of stratified reef or shingle rock, the strata of which were perfectly horizontal, and did not dip down towards either shore. Having observed the manner in which the different kinds of coral rock were formed, I was at a loss to understand how such horizontally stratified masses could have been formed in their present position above high water mark, and could only believe that they were originally formed as reef or shingle rock below high water mark, and had been subsequently raised to their present position. I was thus led to believe that a slight elevation had taken place, and this belief was strengthened by a study of the formation of East Islet. This islet is about 800 yards long, and nearly 100 yards broad; its westernmost extremity is composed of masses of sand piled up on the underlying reef rock, and in this place there is a clump of high trees (*Hernandia peltata*). The eastern and by far the larger part of the islet is of different formation. The even surface of the soil is covered with a low scrub, but bears no high trees nor coco-nut palms. It forms a low plateau, the surface of which does not slope down towards the lagoon, but is perfectly horizontal, and stands 4 feet above the very highest spring tides. The shore on the lagoonward side shows an abrupt fall of 6 feet to the reef, which in this place extends for a distance of 50 yards towards the lagoon, and is only left uncovered at the lowest spring tides. At the eastern extremity of the island there is no reef, but from 1½ to 2 fathoms of water are found within a few yards of the shore. This point is exposed to the ocean, and a strong and constant current sets against it, so that it is undergoing a considerable amount of erosion. On the north or seaward side the reef again extends outwards from the shore, the latter differing from the inner shore in the presence of a talus of large boulders which have been thrown up against it by the waves. Wells have been sunk in various parts of the island, though for some reason which I cannot explain, water is only found in one of them. Numerous pits, some of which are 9 feet deep, have also been dug for the purpose of planting coco-nuts. These pits and wells expose the interesting structure of the superficial part of the island. Beneath a thin surface layer of sand and mould lies a horizontal layer of stratified shingle rock, in which large imbedded coral masses may occasionally be distinguished; this layer is about 2½ feet thick. Beneath it is a layer of loose coral sand about 18 inches thick, and beneath that is another layer of coral rock of the same character as the first, and about 3 feet thick. Beneath this is another layer of friable sand lying on the solid reef rock into which the excavations did not penetrate. These layers lie perfectly horizontally, and do not dip in any direction. They crop out above the reef on the steep eastern and southern shores, and as the loose sand is washed out by the waves the overhanging layer of rock breaks off and falls down in large masses. The central parts of this area are absolutely beyond the reach of any waves at the present time, and as the strata of rock and sand run evenly through it, there is no evidence of its having been formed by successive additions of material through the action of the waves. Nor can it possibly have been formed under the surface of the water unless it has since been raised to its present position, for, as I have said, its upper surface is 4 feet above the level of high spring tides. On one occasion when the tide rose to an abnormal height and invaded several parts of the main island, I saw that the water reached to within 3 feet of the top of the shore, but even then the whole of the upper stratum of coral rock was well above the waves. It is scarcely credible that an even layer of shingle rock could have been formed above the highest high water mark.

Owing to the dense growth of bushes it was not easy to explore the surface of East Islet; but in one spot where the undergrowth was less thick I observed a very shallow, basin-like depression, of which the edges were surrounded by a miniature beach of coral *débris*, giving evidence that the sea had formerly occupied this spot, and yet it is now 4 feet above high water mark. Near this spot were lying great blocks of *Mœandrina* and *Astrœa*, which could not possibly have been thrown by the waves to their present position if the surface on which they lie had not formerly occupied a lower level than it does now. Similar blocks were noticed by Semper in the atoll of Kriangle in the Pelew Islands, but in that case the blocks appear to have been of much larger size.

These facts may not be convincing testimony in favour of a recent elevation of a few feet, but my belief in such an elevation is further strengthened by the following facts communicated to me by M. Spurs, a resident for twenty-five years at Diego, an ardent naturalist, and much interested in coral formations.

A small shore crab of the genus *Ocypus* is always to be found on the sandy flats between high and low water mark. These crabs, as is well known, form numerous galleries in the fine muddy sand, which they line with seaweed, &c., to prevent their falling in. These galleries open to the surface by short passages placed perpendicularly, the mouths of which open only a few inches above the level of low tide. This crab is only found on the shore between tide marks; on the dry land its place is taken by *Gecarcinus*, another genus of crab, which forms different burrows. In the west part of East Islet there is an aggregate of friable, scarcely compacted sand, which has somewhat the appearance of half-dried clay. It lies 5 feet above high water mark, and was found by M. Spurs, during some excavations which he had to make for the purpose of constructing a slip for boats, to be riddled with the seaweed-lined galleries of *Ocypus*, evidently long since disused and empty.

Having made this observation on East Island, M. Spurs made a search in similar formations on the main island, and found, he tells me, precisely the same facts in several instances, aggregates of sand lying at some distance above high water mark, riddled with the abandoned burrows of *Ocypus*. Now since the burrows of *Ocypus* are quite characteristic, and could not have been mistaken by so good an observer as M. Spurs for those of another species, and since they are in the present day only found between tide marks, these observations afford a further presumption in favour of a slight elevation having recently taken place. In any case they preclude the idea of any subsidence being in progress, as Mr. Darwin fancied to be the case in the Keeling atoll. M. Spurs further informs me that during the time that he was superintendent of the oil company's estate, he caused more than 30,000 pits to be dug on the main island for the purpose of planting coco-nut palms, and that he frequently observed in different localities the same alternate layers of sand and rock that I have described as existing on East Island. These alternations of sand and rock would suggest alternations of very slight subsidence with very slight elevation, rather than a single movement of upheaval; yet on the supposition that all the layers were formed beneath the water, as their horizontal stratification leads me to believe, I can venture on the following explanation. The mass of rock which forms the base upon which the islets and other dry land rest is solid reef rock, and the whole floor of the lagoon is similarly formed. The latter is covered at depths of 3 or 4 fathoms and upwards by a layer of fine sand, which may attain a thickness of 2 or 3 feet. In protected parts of the lagoon and in spots where the changeable currents have ceased to deposit any quantity of sand, corals will grow in considerable quantities, chiefly those wide-spreading species of Madrepora which cannot find a lodging on the exterior of the reef, where they would be dashed to pieces by the waves. By the continual growth of new colonies on the top of the old ones which have died, a layer of solid rock of considerable thickness may be formed. Whilst diving for corals at the lower part of the lagoon, I often noticed such layers of half-formed rock on which living coral was growing or not, according as the constantly changing currents were at that time throwing up sand in the locality or not. Thus on the west side of the lagoon, off Point Marianne, there are large tracts of recently formed coral rock, on which no living corals are to be seen, whilst on the east side of the lagoon, exactly opposite to Point Marianne, a similar basis of rock is luxuriantly covered with growing coral.

Now as the currents are constantly changing, and as the changes may, as I saw, affect an area some miles in extent, one may suppose that an area was first covered with corals growing on the sand, which everywhere covers the reef rock, when the latter lies more than a fathom below the surface. A change in the currents brought abundant sand to the spot, killed the corals, and deposited an even layer of sand of some little thickness over the rock formed by the skeletons of the dead corals. A further change in the currents would again render the spot suitable for coral growth, and a new layer of rock would be formed over the last layer of sand. I have seen quite analogous formations in progress in a fathom of water a little way above Point Marianne. Raise the formation to the surface, and you get that stratification which occurs in so many parts of the island, a stratification which cannot be explained on any theory of subsidence, and is scarcely less difficult to explain on the supposition of rest. At first I had some hesitation in extending to an island on the borders of the lagoon, as is East Island, a view of the formation of layers of sand and rock derived from an inspection of the interior of the lagoon, but afterwards I saw that similar layers were being formed just within the large reef known as Spurs' Reef, west of Middle Island, so that no objection can be raised on that score. The whole character of the Chagos Group is very much opposed to the theory that atolls and barrier reefs are formed during subsidence. There are several atolls rising above the waves, that of Peros Banhos being 55 miles in circuit, and composed of numerous small islets placed upon a ring-shaped reef through which there are several large and deep channels. Egmont or Six Islands is an instance of an atoll in which the encircling reef is perfect and unbroken by any channels, the land consisting of six islets placed for the most part on the southern and western sides of the reef. There are several submerged banks, nearly all of which have an atoll form. Of these the best known is the Great Chagos Bank, a huge submerged atoll 95 miles long and 65 miles broad, having a depth of 4—10 fathoms over a narrow rim around its periphery, and a central lagoon of a depth varying up to 45 fathoms. South-west of the Great Chagos Bank, distant less than 15 miles, lies the atoll of Six Islands, and on the other side of these, scarcely 12 miles distant, lies another submerged atoll, known as Pitt Bank. South-west of Pitt Bank are two smaller banks, Ganges and Centurion's Banks. Darwin considered that the Great Chagos Bank afforded particularly good evidence of the truth of the subsidence theory. He regarded it as an atoll carried down by a too rapid subsidence below the depth at which reef-building corals flourish. The same would be the case for Pitts Bank and the two others just mentioned. A more intimate knowledge of the Great Chagos Bank, and of the relations of it and other submerged banks to existing land, shows this view to be untenable. In the first place the rim of the Great Chagos Bank is on an average not more than 6 fathoms below the surface, and therefore situated in a depth eminently favourable for coral growth, and there are actually six islets on the northern and western edges rising above the water, and some of them inhabited. Secondly, any such rapid subsidence could not have affected areas only 30 miles apart without involving the Six Islands atoll lying directly between them. A similar argument might be extended to the more northern islands of the Chagos group, and even to Diego Garcia itself, although it lies somewhat apart from the rest of the group. Again, if atolls and barrier reefs are formed around subsiding peaks, it is at least curious that throughout the Laccadive, Maldive, and Chagos groups there are no instances of high islands surrounded by barrier reefs, marking the last remnants of pre-existing land. In the more western parts of the Indian Ocean, between Madagascar and the Seychelles, there are numerous atoll islands, and in long. 60° E. there lie the submerged Saya de Malha Bank and the reef known as Cargados Carajos. Between these two lies the extensive Nazareth Bank, having over it depths of from 14 to 45 fathoms. The Saya de Malha Bank appears to have the characters of a submerged atoll, having a central depression of 65 fathoms, surrounded by a rim which has only 8 to 16 fathoms on its eastern side, but 22 fathoms on the western. Some of the groups north of Madagascar afford very good evidence of upheaval. Aldabra Island, situated in lat. 9° 22′ S., long. 46° 14′ E., is a perfect instance of an upraised atoll. Captain Wharton describes the external shores as consisting of low coral cliffs, about 20 feet high, the surface of the land being composed of jagged coral rock. The lagoon is entered by a passage varying from 11 to 5 fathoms in depth, but its internal portions are either very shallow or partly dry at low water. Not far distant is the Cosmo Ledo group, a perfect atoll, with a lagoon some 4 fathoms deep, or less. There are ten islets of various sizes on the reef, and all of them appear to have been elevated some 10 feet. There are some hills 40 and 50 feet high on the two largest islands, but these appear, according to Captain Wharton, to be formed of blown sand. The Farquhar group and Assumption Island, situated within the same area, have been raised, according to the same authority, some 10 feet. Providence Island, in lat. 9° 14′ S., long. 51° 2′ E., appears to be a low island situated upon the edge of the atoll-shaped Providence reef. At a distance of 19 miles from Providence Island is the island of St. Pierre, which has no fringing reef. It is particularly interesting, for although it is in close proximity to the low Providence atoll, it has been raised about 40 feet above high water, and in the absence of a fringing reef the sea breaks with great violence against a low cliffy coast, hollowing out a number of caverns which, from the description given in the sailing directions for Mauritius and its islands, appear to open inshore by "blow-holes."*

Near and among these raised coral formations are several submerged banks, the most important of which is McLeod Bank, situated in lat. 9° 57′ S., long. 50° 20′ E., between Providence Island and the Cosmo Ledo group. The details show that there is a group of coral

* For the information on the islands north of Madagascar I am indebted to the courtesy of Captain W. J. L. Wharton, R.N., F.R.S.

formations, situated near lat. 10° S., north of Madagascar, in which are found raised atolls—atolls whose dry land just rises above the waves and submerged banks. There can be no clearer proof that atolls are formed in areas of elevation, and, if the facts which I have already stated concerning Diego Garcia are of any weight, it would seem that most of the coral formations of the Indian Ocean mark areas of elevation rather than of rest; certainly they are not evidence of subsidence.

Those who have felt that the evidence brought against Darwin's subsidence theory is too strong to be resisted, must often have felt that no satisfactory explanation of the lagoons of atolls or the lagoon channels of barrier reefs has been given in its place. Semper was the first to suggest that the lagoon was formed by a solution of the interior parts of the reef, and more recently this view has been urged with great force by Murray, who points out in addition, that corals on the periphery of a reef must, from their position, get the advantage over those more interiorly situated, being more directly in the track of food-bearing currents. Neither of these explanations has completely satisfied me. That sea-water exercises a solvent action upon carbonate of lime does not admit of doubt, and that the scour of tides, combined with this solvent action of the water, does affect the extent and depth of a lagoon is obvious. But I challenge the statement that the destructive agencies within an atoll or a submerged bank are in excess of the constructive. It would be nearer the mark to say that they nearly balance one another. In the first place the carbonate of lime held in solution by sea-water is deposited as crystalline limestone in the interstices of dead corals or coral *débris.* Anyone who is acquainted with the structure of coralline rock knows how such a porous mass as a Mæandrina head becomes perfectly solid by the deposition of lime within its mass. This deposition can only be effected by the infiltration of sea-water. In reckoning the solvent action of sea-water, therefore, account must be taken of the fact that a not inconsiderable proportion of the carbonate of lime held in solution is redeposited in the form of crystalline limestone. Of this, it seems, Mr. Murray has not taken sufficient account, and has, therefore, overstated the destructive agency of the sea. Secondly, the growth of corals, and the consequent formation of coral rock within the lagoon, is generally overlooked.

Whilst diving for corals at Diego Garcia I had abundant opportunities of studying the formation of coral rock within the lagoon, in depths under 2 fathoms. The layers of tolerably compact rock thus formed are of no mean extent or thickness; they soon become covered with sand, and are thus protected from the solvent action of the water. I have found it impossible to reconcile Mr. Murray's views with what I saw of coral growth within a lagoon. Not only do the more delicate branching species of the Madreporaria flourish in considerable numbers, but true reef-building species, *Porites*, *Mœandrina*, *Pocillopora*, and various stout species of *Madrepora*, are found there. It is a mistake to suppose that certain species of corals are restricted to the external shores, others to the lagoon. My collections proved that many of the species growing in the lagoon at distances of 5 miles and upwards from its outlet are identical with those growing on the outer reef. In addition to them are numerous species, such as *Seriatopora stricta*, *Mussa corymbosa*, *Favia lobata*, *Fungia dentata*, and many others that are not found on the outside. The reason is that the last-named are either free forms such as *Fungia*, or are attached by such slender and fragile stems to their supports that they could not possibly obtain a foothold and maintain themselves among the powerful currents and waves of the open ocean.

These various species, numbers of which grow close together, form knolls and patches within the lagoon, and it cannot be doubted that their tendency is to fill it up. Again, in reefs which do not rise above the surface, or are awash for the greater part of their extent at low tides, great quantities of *débris*, torn from the outer slopes, are constantly carried over the rim of the reef and tend to fill it up. Hence it follows that in a lagoon entirely surrounded by dry land, or nearly so, as is the case at Diego Garcia, the tendency to accumulation of material within the lagoon would be less than in submerged or incomplete atolls, for *débris* cannot be swept over into the lagoon, and the only constructive agency is the growth of coral. If the power of solution of sea-water is so great, it must be supposed that in complete or nearly complete atolls the lagoon would be deepening rather than shallowing; yet at Diego Garcia the lagoon is obviously shallowing in many places, and has nowhere increased in depth since Captain Moresby's survey in 1837. Indeed, the southern part seems to have shoaled a fathom since that time, and this is the more remarkable, since the S.E. trade winds are by far the most constant and strongest winds there, and tend to accumulate material at the northern rather than at the southern end. The fact is, that these winds sweep the sand out of the southern part, and thus leave an area particularly favourably situated for the growth of corals. Mr. Murray points out that larger atolls generally have deeper lagoons than small atolls, and urges this fact in support of his theory; but here again the facts in the Chagos group are against him. Victory Bank is a submerged atoll, the Solomons is an atoll with a large extent of dry land, in each the lagoon attains a depth of 17—18 fathoms, and in Diego Garcia the lagoon, although far larger, does not attain a greater depth. Peros Banhos is far smaller than the Great Chagos Bank, yet in both the lagoons attain nearly the same maximum depth, viz., 41 fathoms for Peros Banhos, 44 fathoms for the Great Chagos Bank.

Speaker's Bank is very little larger than Peros Banhos; its lagoon is far shallower, having a maximum depth of 24 fathoms.

These considerations have led me to discredit the solution theory as an explanation of lagoons and lagoon channels, and other objections have been lately urged with great force by Captain Wharton. The conclusion which I reached, after carefully considering the conditions of submerged banks of atoll form, is that the ring-shape of the outer reef is to be explained by the peculiarly favourable conditions for coral growth found on the external slopes. Although corals may and do flourish in lagoons, they are only found in knolls and patches, and are always liable to be smothered when, by a change in the tidal currents, sand is thrown down upon the place where they are growing. On the external slopes, however, corals grow in extraordinary abundance, and chiefly those massive forms whose skeletons take so conspicuous a share in the formation of coral rock. If once it is admitted that the periphery of the reef offers peculiarly favourable conditions to the growth of reef-forming corals, it follows that, as the reef rises to the surface its external parts will outstrip the more internal, and will reach the surface first, forming a rim around a central depression or lagoon. This elevated rim will be as marked a feature in submerged as in complete atolls. Not long after I had arrived at this conclusion, and whilst the earlier part of this paper was writing, Captain Wharton published a letter on coral formations in 'Nature,' in which he arrives at identical conclusions. His knowledge of coral islands is so extensive that his views have great authority, and I am extremely pleased to find that his opinions on this subject are the same as those which I have formed. The only point in which I differ with him concerns the explanation of the favourable conditions on the external slopes.

Following Agassiz, Murray, Guppy, and others, Captain Wharton supposes that the favourable conditions consist in the increased food supply brought by the superficial currents of the ocean. This I cannot believe to be a complete explanation. The quantity of food present must of course determine the existence of coral polypes in any particular locality, as it does that of all other animals, but it cannot be considered to be the chief favouring cause of coral growth on the external shores of an atoll for several reasons. If the prime cause of luxuriant coral growth is an abundant food supply, and if, as we may assume for the present, the food consists in the minute pelagic animals borne in ocean currents, there must always be a definite relation between ocean currents and coral formations. Some authors (Agassiz and Murray) have gone so far as to say that coral reefs are only formed in the track of great ocean currents. This is hardly the case. In the Pacific a study of the chart shows that atolls and barrier reefs are formed irrespective of currents, and some large groups, such as the Paumotu Islands, seem to lie altogether to one side of the prevailing currents. The islands north of Madagascar (Cosmo Ledo, Farquhar groups, &c.) do not lie in the track of the Mozambique current, but to one side of it, and the Chagos group does not lie in any constant current, but is at one season of the year washed by the currents caused by the S.E. trades, at another by the irregular currents caused by the N.W. monsoons. During the latter season there are often long periods of absolute calm during which the currents are merely tidal. The S.E. trades, however, are the dominant winds both in force and frequency, and if the coral growth were dependent chiefly on the supply of food brought by surface currents, those corals growing on the windward side would naturally have the advantage in the food supply. Situated in the direct tract of the current, they would receive an abundant supply of living organisms, and then the impoverished current, sweeping past the sides of the reef, would become poorer and poorer in organic life as it flowed towards the leeward side, till finally on the further shore, the backwash would hardly bring any sufficient supply of food for coral growth. A reef, therefore, would tend to extend in the direction of the current, and the longer diameters of the atolls might be expected in the Chagos groups to lie S.E. and N.W. This is not the case. Diego Garcia and Speaker's Bank lie north and south; Peros Banhos is nearly square; the Solomons lie N.E. and N.W.; the Great Chagos Bank lies east and west. Pitt's Bank does lie S.E. and N.W., but the rim on the northern and north-western side is nearer to the surface by some 5 fathoms than it is on the southern and south-eastern side, indicating a more vigorous coral growth on the side turned away from the prevailing current. In the case of a submerged bank it is difficult to see why the corals situated to leeward should be better off as regards food supply than those living in the interior of the lagoon, for the superficial parts of the current would flow freely over the windward rim and bring abundant food into the lagoon. But in the Great Chagos Bank the northern rim is, on the average, higher than the south-eastern, and all the islets are placed on the northern and western parts of the rim. A study of the corals growing within the lagoon of Diego Garcia is in this case of considerable interest. If their existence depended upon pelagic life brought to them by currents, it would be expected that the most numerous coral patches would be found at the northern end of the lagoon in the track of the tidal currents. There is a considerable area of active coral growth to the south and west of Middle Islet, but beyond this there is no relation whatever between the luxuriance of the coral patches and the mouth of the lagoon. Corals grow most vigorously along the shore between Minni Minny and East Point, and most vigorously of all at the southern end of the lagoon, where they are most remote from the influence of currents setting in from the ocean. Yet, as I have already shown, it is to the growth of coral alone that the shoaling of this part of the lagoon can be attributed. These facts are a sufficient argument against the idea that currents teeming with pelagic life are the prime factors in determining coral growth. It must be remembered that we are very ignorant about the food of corals. There are very few accounts of the food found in their digestive cavities, and it is purely an assumption to speak of their feeding only on pelagic organisms. I have in another place reported the presence of vegetable matter in the curiously modified digestive stomodæum of *Euphyllia*, and I have no doubt, after what Dr. Hickson has told me of the relations of corals to mangrove swamps in Celebes, that far more corals are vegetable feeders than has hitherto been supposed. The lagoon of an atoll is always full of decaying vegetable matter derived from the shore bushes and palms, and it seems likely that there is a connexion between the richer coral growth along the shores of a lagoon and the supply of vegetable *débris* from the shore.

My observations incline me to the belief that the most important circumstances affecting coral growth are the direction and velocity of currents. My observations are confirmed in every particular by those made by Dr. Hickson in Celebes, and communicated by him to the British Association in 1887. Corals grow best in places where a moderate current flows constantly over them. They are killed in still water by the deposition of sediment, and they will not grow in places where a strong current sets directly against them. I noticed at Diego Garcia in many places, but particularly at the east end of East Islet, that a strong and direct ocean current is most unfavourable to coral growth, and that the reef is barren and suffering rapid erosion at such exposed spots as allow the whole force of the current to fall directly upon them. As the current parts and flows round the obstacle, one meets with a reef covered with *débris*, but barren of live coral; further on, as the current moderates in force, one finds a few growing heads of coral; and, finally, at the further end of the reef, where the current has abated its force considerably, there is a luxuriant bed of living corals and Alcyonaria. This can be seen in perfection on the southern reef of East Islet. Dr. Hickson tells me that he has observed the same facts at Celebes, that direct and strong currents are unfavourable to coral growth, that moderate tangential currents are extremely favourable, and sluggish or still water again unfavourable. This view, which both of us can support by many observations, is much at variance with the old accepted saying that corals grow best where the breakers are the heaviest. It appeared to me that heavy breakers are not favourable to coral growth, because of the quantity of shingle which they dash against the soft-bodied polyps. Some massive forms might withstand the force of breakers and violent currents if the polyps could be sufficiently protected from the shingle, but the branching Madrepores are soon broken off and swept away, and even the more massive Mæandrina soon follows, for whilst the surface of the colony grows the base is dead, is soon riddled by boring sponges, Serpulæ, &c., and is no longer able to bear the strain put upon it. The great mass then breaks off and is rolled along the reef, pounding other corals in its course. I was long puzzled at Diego Garcia when I found that the outer reef was nearly barren of coral, even where it is covered by a foot or two of water at the lowest spring tides. I noticed directly after my arrival that the sea always broke on the reef west of Middle Islet (Spurs' Reef), and believing, from what I had read, that this must be a most favourable spot for coral growth, I took the first opportunity of visiting it at low tide. To my surprise I waded for nearly a mile, nearly waist deep, without finding a single living coral on the seaward flat, although the lagoon just within the reef was filled with knolls of living corals growing at various depths. All around the shores I found the same thing, flat reefs of barren coral rock, sloping very gently down to the ocean, on whose surfaces, even where they were constantly covered with water, no live corals were to be found. Just at the edges of the reef, where the sea breaks at low spring tides, I could detect during the reflux of the waves, solid masses of *Millepora*, and dead rock covered with Millepores, but no live Madrepores, excepting where narrow channels ran up into the reef, the sides of which generally bore a few colonies of *Mæandrina*, *Porites*, or *Madrepora aspera*. On the other hand, it can readily be seen that the external slopes, just beyond the rim of the reef behind the breakers, are covered with masses of coral. A short experience of wading across the reefs showed me the reason of the want of coral on the flat upper surfaces. *Débris*, torn from the corals growing on the slopes, is continually washed across the flat reef surfaces, and in strong breezes large masses are rolled along. Even in the calmest weather the coarse fragments can be felt sweeping past one's legs with some force, and one can readily understand that the soft polypes could not withstand the constant wear and tear. The moderate currents, on the other hand, do not carry large fragments, but prevent deposition of sand which would choke the corals, and at the same time supply the conditions of nutrition necessary to their growth. In still water, sand is thrown down in large quantities, and this and the absence of a constant supply of food prevents coral growth. The conclusions to which I came were that the external slopes afforded conditions eminently favourable to coral growth, that the upper surfaces of the shore reefs were very unfavourable, and that in certain parts of the lagoon favourable conditions again occur, though the growth is never so luxuriant there as on the external slopes. The favourable conditions on the last-named places are explained by the behaviour of a current when it meets with an obstacle which presents a sloping wall to it. The lowest parts of a strong and deep current are the first to strike the sloping wall or glacis; they are in part divided and sweep around the sides of the obstacle, but a large part of the current is deflected upwards over the slope, and its action, combined with that of the more superficial parts of the same current, results in a moderate current flowing upwards over the slope (*vide* Plate 4, fig. 2). This moderate current favours coral growth on the slope, but at its upper edge the superficial current combines with the upward stream, and the two dash onwards with increased force over the flat upper surface, destroying all coral life there. At the sides of an island the current flows tangentially, with moderated force, especially in its more superficial portions, and affords the necessary conditions on the external slopes there, whilst on the reverse side of the island the backwash affords weak currents, which are highly favourable to coral growth. Thus it is that everywhere around the island the external slopes are covered with a luxuriant bed of corals.

To fully understand the manner in which currents are moderated in flowing past an obstacle, it is necessary to remember that water possesses a certain amount of adhesiveness, and tends to cling to the sides of the obstacle, so that the current is always rather stronger at a little distance from the obstacle, whatever it may be, than it is where it runs close against it, and the rougher the surface past which it flows the greater is the adhesion, as is well known to everyone who has had experience in boating or shipbuilding.

The net result of all this is that the corals are always thickest along the slopes around a coral reef, and the reef tends to increase at its periphery, growing upwards there, whilst it tends at the same time to spread outwards. These principles hold good in the case of a submerged bank as well as in the case of a reef that is awash, and a submerged bank must tend in the course of time to reach the surface in its circumferential portions, and form an atoll-shaped reef, on the rim of which detritus may be heaped from place to place, forming shingle cays or islets which may temporarily form dry land. In atolls where storms are of frequent occurrence, regular storm-beaches may be formed, till the fragments piled high upon one another may form low islets standing some 6 or 10 feet above high water mark, upon which vegetation may subsequently find a footing. Atolls are often formed in this way, without any elevation taking place, and such has undoubtedly been the case in the Florida reefs, where atolls (the Tortugas) and barrier reefs and islands have been formed in an area of complete rest. No one who has read the admirable work of Alex. Agassiz on the Florida reefs can fail to agree with the author's conclusion that the islets there have been formed by the action of the wind and waves alone, without any assistance from the upheaval of the bed of the sea. But I am not satisfied that this has been the case in the Chagos group. Storms are of very infrequent occurrence there, and the horizontal masses of reef rock standing above high water mark cannot be attributed to the normal action of the prevailing winds and currents.

In the Florida reefs the nature of the soil betrays its origin—its strata slope towards the sea on every side, and the lamination of the rocks attests the long-continued action of waves and spray. But the alternate horizontal layers of sand and rock occurring so abundantly at Diego Garcia are quite different; they do not dip seawards, their composition differs from the rocks of the Florida reefs, and their edges, instead of showing signs of accumulation of fresh material, are often bluff, and show that the sea is gradually eating them away. It is difficult to explain these appearances except on the hypothesis of slight elevation. It might be objected that if any upheaval had taken place, the banks lying at various depths below the surface would have been raised to different heights, and that it would be in the highest degree unlikely that so many would be raised some 4 or 5 feet above high water mark and no more throughout so large areas as the Laccadive, Maldive, and Chagos Islands, and the various low groups in the Pacific. The force of the objection must be admitted, but it may be observed that atolls raised from 10 to 40 feet above the waves are not so uncommon as has been hitherto supposed, and that the numerous submerged banks lying at very various depths show that all the reefs have not been raised to one height in a single area of elevation. The uniform level of many atolls and barrier reefs admits of a further explanation. A reef raised some few feet above the sea level is at once attacked by the waves, and as the rim is very narrow, it must soon be worn away till the whole of the land is eaten away, and its surface is brought awash once more. Thus every slight movement of elevation would soon be compensated by the denuding action of the waves. The island of St. Pierre, already described, is a good instance of this process of erosion. It cannot be doubted that this island, which has recently been raised 40 feet, is undergoing rapid waste, and must soon be reduced to the level of the sea. At Diego Garcia I was astonished at the rapid destruction of dry land which is in progress, on the outside as well as the inside of the lagoon. The destruction is not so great on the outside as on the inside as a rule, for in the former case the rampart of coral boulders thrown up by the waves compensates in many places for their erosive action. But in the bay above Horsburgh Point, exposed to the full strength of the S.E. trades, the destruction is very great. M. Spurs, an old resident on the island, writes to me on this subject: "Cette déstruction est très rapide; Diego perd en moyenne un pied de terrain par an, tant intérieurement qu'extérieurement, excepté aux pointes N.E. et N.O., où une partie des sables entrainés du fond de la baie par les vents de sud-est, conservent à ces deux pointes leur largeur première."

M. Spurs has overestimated the rate of destruction, but there can be no doubt that it is very considerable. It is most conspicuous along the shores bordering the lagoon. The stumps of coco-nut palms, the newly-made breaches into the land, forming shallow inland lagoons, the vertical faces of old banks of half consolidated sand all attest it. Just above Point Marianne is a road running along the lagoonward shore, which when I left the island had been narrowed by the action of the sea to a mere path, and was in some places almost impassable, as the sea had made clean breaches across it, and found its way into some shallow fresh water lagoons lying on the other side of the road. I was assured that this road had been over 12 feet wide some years previously, and that it was formerly separated from the lagoon by a narrow strip of land of an equal width. Perhaps the best evidence of the destruction of land is afforded by the "barachois" at the southern extremity of the island. These barachois are inland lagoons connected with the main lagoon by a narrow outlet some 2 fathoms deep or more. They are filled and emptied every tide, and their floor is intersected by numerous small channels running in every direction. No corals grow within the barachois, and a slight study convinces the observer that the daily scour of the tides is denuding their shores and floors very considerably.

Barachois are formed in the following way:—During unusually high tides, when the waters of the lagoon are dammed back by a north-westerly wind of unusual violence, the water rises to great heights and invades the land in several places. In some instances it actually makes a breach in the lagoonward shore, and fills up the shallow depressions which are often found in the middle of the strip of land. A pool of salt water is thus formed, which kills the coco palms and other vegetation growing in its bed, and as this process is repeated again and again, in the course of a few years a channel is cut out between the pool and the lagoon, which finally becomes so deep that spring tides, and finally even neap tides, run in and out of the pool regularly. As soon as these conditions are established the channel is scoured out and deepened, and the daily tides scour out the bed of the pool, forming a complete barachois.

It is not easy for one who has not seen it to understand how much of the loose soil of a coral islet can be moved by a single tidal encroachment. It happened that I was riding past the very thin strip of land between Minni Minny and Barton Point the day after an abnormally high tide. The strip of land here is not more than 30 yards across, and the sea had washed right over it on the previous day, clearing away an amount of soil which was almost incredible. My companion M. Casimir Leconte told me that the sea had not been known to wash over this place before. It was apparent that after a few more of such high tides as I had witnessed, a permanent breach would be made at this spot, and another lagoon outlet would be formed, which would be continually deepened as the tide set through it. At the south-eastern side of the island I noticed that the land was being rapidly destroyed on the outer shores just opposite to a half-formed barachois, whose margins are situated not 60 yards from the outer shore. If the same process of external destruction continues, whilst the barachois is deepened and scooped out from within, it will not be many years before the ocean makes a new channel into the lagoon at this point. Thus the continuous strip of land which now nearly encircles the lagoon of Diego Garcia is tending to be split up again into a series of islets. At the points where the breaches are made the tides and ocean currents will rush with great force into the lagoon and will scour out deep channels similar to that now existing between Middle and East Islets.

These facts taken together show how the normal action of tides, winds, and waves is constantly tending to lower to the sea level any dry land that may have been formed by elevation or otherwise. It does not seem to me to be surprising that the majority of atolls and barrier reefs are, under such circumstances, only just able to maintain their surfaces above the sea level.

No explanation of atoll formation would be complete if it did not include an explanation of the great Maldive atolls. Without attempting to enter into a lengthy discussion of Darwin's views, I will give my own explanation of the atoll. Tilla-dou-Matte atoll is, as is well known, a huge atoll composed of atolls. The islets forming the rim of the main atoll are themselves atolls with their own lagoons; the main lagoon contains a few secondary atolls corresponding to the coral patches in an ordinary atoll. It will be generally admitted that coral reefs are constantly increasing to seaward because of the excessive growth of coral on their external slopes.* As the inward shores of an atoll are constantly being removed, and an atoll if completely formed tends to be broken up again into small islets when it has reached a certain size, and as the channels between the islets must be continually deepened by the scour of the tides until deep passages are formed, an atoll like Diego Garcia

* This statement may at first sight seem at variance with what I have just said about the rapid destruction of land on the outer and inner shores of an atoll; but in the latter case it is *land above water* that is destroyed. Coincidently with this process the reef-rock below water is constantly tending to raise itself and to spread in all directions, owing to the perpetual growth of corals and the accumulation of their skeletons.

may be expected to reach in time a condition like that of Peros Banhos. It is probable that a large bank like the Great Chagos Bank, when it reaches the surface, can never give rise to a continuous strip of land, but must consist of a chain of islets separated by channels of some depth and by tracts of submerged reef. The islets and tracts of reef in either case would be bounded by deeper channels, and these channels, swept by strong currents, would become wider and deeper, for corals could not thrive in them. After a time the islets would become so far isolated, and the entries into the lagoon would become so large and numerous, that oceanic conditions would prevail in the lagoon, and then there would be around each separate islet or piece of reef all the necessary conditions for the formation of a new atoll. The currents would strike upon one side of the islet or reef, sweep round it, and give a backwash at the further side; the corals would flourish in the circumferential parts of the reef surrounding the islet, and new atolls with shallow lagoons would be formed.

In Tilla-dou-Matte the lagoons of the secondary atolls are tolerably deep. In this case they must have been formed before any land reached the surface. Applying the same reasoning as in the former case, it can readily be understood how in the case of the Great Chagos Bank, which has wide and deep breaches in many places, the isolated reefs as they grow to the surface must tend to assume an atoll form. An examination of the chart shows that this is the case. The Great Chagos Bank in the course of time will rise to the surface as an atoll composed of secondary atolls or atollons, similar to, but on a smaller scale than, Tilla-dou-Matte atoll. The explanation of atollons in the centre of a large lagoon in which oceanic conditions have been established, is quite obvious.

In conclusion, I may sum up by saying that the strength and direction of currents appears to me to be the main influence on coral growth; that the behaviour of currents on meeting an obstacle with sloping shores, explains the superabundant growth of corals on the outer slopes of a reef, whether submerged or awash; that the growth of corals on the periphery of a bank being in great excess of the growth in its interior portions is sufficient to explain the formations known as atolls and barrier reefs without the aid of the solution theory proposed by Mr. Murray, and ably defended by him in a recent number of 'Nature.' I have shown that Mr. Murray has overestimated the effects of solution in neglecting the compensating action of the re-precipitation of the carbonate of lime held in solution, and the formation of coral rock within the lagoon through the agency of coral growth there. I have also shown that the *rôle* played by currents is not what is supposed; that the carriage of food by currents must be considered of subsidiary importance in estimating their effect, though

I would be the last to deny that organic material brought by currents must determine the existence of coral polyps in every instance. I have confined myself to a discussion of the islands of the Indian Ocean, because I have no practical knowledge of coral formations in other parts of the world, and it would be rash to dogmatise about their structure when the conditions may be different. In the Indian Ocean I may fairly assume that the coral groups are subjected to the same influences that I studied at Diego Garcia. No doubt many of my statements will be contradicted by observers in the Pacific Islands and elsewhere. I can only say that they are true of the group which I have visited, and that within the limits of that group they form contradictions to existing theories. The seas in which coral reefs are formed are not all subject to the same conditions. In the Chagos group there is always a heavy swell on the ocean, and the sea breaks with a great force against the outer shores, and even over the shallower parts of submerged banks; the breakers are said to reach a height of 15 to 18 feet, but I never saw them so large at Diego Garcia. In other groups of coral formations the sea is wonderfully calm, and the sea rarely breaks on the outer shores with any violence. Some islands are situated in the direct course of great ocean currents, others are not, and are swept by the minor currents caused by prevailing winds. However one looks at the subject one must realise that the laws governing the formation of coral reefs are exceedingly complex, and that many circumstances have to be taken into account before any perfect explanation of their structure can be obtained. Action and reaction, destruction and reconstruction, growth and decay are constantly at work; the result of the multitude of nicely balanced forces, seemingly antagonistic, is the atoll or reef. It seems to me that the current theories of the formation of coral reefs attempt to explain everything by one or two agencies, whereas the agencies are numerous, and interact in a most complicated manner. Mr. Murray is undoubtedly right in laying stress on the necessities of nutrition suitable for coral growth, and the solvent action of sea water, but he has not taken count of other agencies which tend to modify and obscure these, and therefore his theory is not in itself sufficient to explain the question. I cannot expect that my views will be of universal application, since my observations were made on so limited a field, but I hope that competent observers will spend some months on coral formations in different parts of the world and give the closest attention to their facies, without overlooking the minutest particular bearing on coral growth on the formation of the reef. Then only shall we be in a position to construct a general theory of coral reefs.

[*Note.*—The life history of corals shows that they are not adapted to live in strong and direct currents. The free larvæ (planulæ) swim

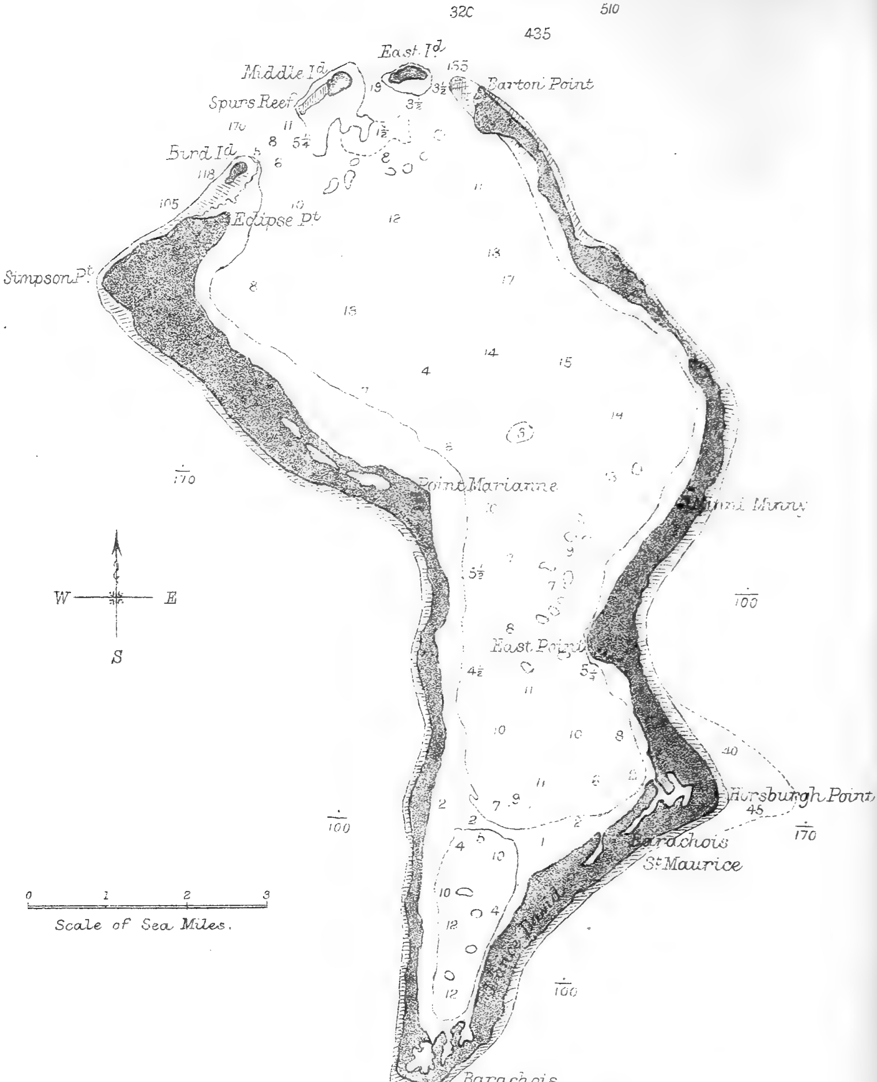

East Id
Middle Id
Barton Point
Spurs Reef
Bird Id
Eclipse Pt
Simpson Pt
Point Marianne
East Point
Horsburgh Point
Barachois St Maurice
Barachois de Sud
W
E
S
0
1
2
3
Scale of Sea Miles.

Fig. 1.

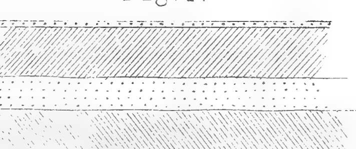


Fig. 2.

about for some time by means of their cilia before they attach themselves to a suitable spot, and undergo their further development. These planulæ are necessarily swept away by strong currents to the further shores of a reef, and it has been shown experimentally by von Koch that they will not attach themselves in a strong current, or if attached, will loose their hold when a strong current is directed upon them. Although coral colonies grow larger by budding, they originate in every case from planulæ, and no great group of corals could grow in a place where the strong currents prevented planulæ from attaching themselves.—*March* 20].

EXPLANATION OF PLATE.

FIG. 1 represents a diagrammatic section through the surface soil of East Islet showing the alternate layers of sand and rock of which it is composed. Scale ⅛ inch to the foot.

FIG. 2 shows the way in which a current striking a sloping bank is deflected upwards over its surface until it joins the superficial part of the main current at the upper edge of the reef.

III. "The Chemical Composition of Pearls." By GEORGE HARLEY, M.D., F.R.S., and HARALD S. HARLEY. Received February 23, 1888.

Although there are many qualitative analyses of pearls, from our being unable, in their voluminous literature, to find any evidence of a quantitative analysis of their ingredients having been recorded, we undertook the examination of several varieties, of which the following is an account:—

1st. As regards oyster pearls. Of these three varieties were examined, British, Australian, and Ceylonese.

The qualitative analyses showed that they all had an identical composition, and that they consisted solely of water, organic matter, and calcium carbonate. There was a total absence of magnesia and of all the other mineral ingredients of sea-water—from which the inorganic part of pearls must of course be obtained. Seeing that ordinary sea-water contains close upon ten and a half times more calcium sulphate than calcium carbonate, one might have expected that at least some sulphates would have been found along with the carbonates, more especially if they are the mere fortuitous concretions some persons imagine them to be—a view we cannot endorse, from the fact that by steeping pearls in a weak aqueous solution of nitric acid, we are able to completely remove from them all their mineral constituents without in any way altering their shape, and but very slightly changing their naked eye appearances, so long as they are permitted to remain in the solution. When taken out they rapidly dry and shrivel up. We shall take occasion to point out in our next communication, which will be on the microscopic structure of pearls, that a decalcified crystalline pearl bears an intimate resemblance to a decalcified bone, in so far as it possesses a perfectly organised matrix of animal matter. No phosphates whatever were found in any of the three before-named varieties of pearls.*

The next point being to ascertain the exact proportions of the substances composing the pearls, and pure white pearls being expensive, from our having ascertained that all the three kinds we were operating upon had exactly the same chemical composition, instead of making separate quantitative analyses of them, we simply selected two pearls from each variety, of as nearly the same size and weight—giving a total of 16 grains—and analysed them collectively, the result obtained being—

Carbonate of lime†	91·72	per cent.
Organic matter (animal)	5·94	„
Water	2·23	„
Loss	0·11	„
	100·00	

From this it is seen that notwithstanding that mother-of-pearl consists of precisely the same ingredients, their proportions are quite different from what they are in fine, pure white pearls (we say fine pure white, because pearls vary greatly in purity, and those we analysed were good ones), which are infinitely denser, and consequently harder than the mother-of-pearl constituting the shells in which they are formed. The analysis of mother-of-pearl given in Watts' 'Dictionary of Chemistry' is—

Carbonate of lime	66·00	per cent.
Water	31·00	„
Organic matter	2·50	„

thus showing that while mother-of-pearl contains less than half the quantity of organic matter pearls do, it at the same time possesses close upon fourteen times more water. This fact appears to us all the more surprising as, not alone to the naked eye, but even under the

* Phosphates are referred to as being present in pearls by Rudler in his article in the 'Encyclopædia Britannica.'

† The carbonic acid was estimated by disengaging it with dilute sulphuric acid into a soda-lime tube, and calculating the increase in weight (as described by Lunge and Hurter in 'The Alkali Maker's Pocket-book'). The amount of the organic matter by noting the loss by weight after calcining—slightly moistening the mass with a solution of ammonium carbonate.

microscope, the structure of the mother-of-pearl of the shell and of pearls is almost identical.

[One can scarcely imagine that the analyst could have possibly employed in his investigation a piece of shell while it was yet in a fresh and consequently moist state.

As regards the hardness of pearls, again, it may perhaps be as well for us to remark that good pearls have a much denser texture than the majority of persons appear to suppose, as may be gleaned from the following facts.

On one occasion being desirous to crush into powder a split-pea sized pearl, we folded it between two plies of note-paper, turned up the corner of the carpet, and placing it on the hard bare floor, stood upon it with all our weight. Yet notwithstanding that we weigh over 12 stone, we failed to make any impression whatever upon the pearl, and even stamping upon it with the heel of our boot did not suffice so much as to fracture it. It was accordingly given to the servant to break with a hammer, and on his return he informed us that on attempting to break it with the hammer against the pantry table, all he succeeded in doing was to make the pearl pierce through the paper and sink into the wooden table, just as if it had been the top part of an iron nail, and that it was not until he had given it a hard blow with the hammer against the bottom of a flat-iron that he succeeded in breaking it.

In addition to the foregoing we may likewise take occasion to mention that shell-fish pearls are not nearly so easily dissolved in strong vinegar as the interesting tale of Cleopatra having taken a large pearl from her ear, and, after having dissolved it in vinegar, drunk it to the health of her lover Antony, would lead one to believe. For during our experiments we have learned that not only does it take many days to dissolve out the mineral constituents of a large pearl in cold vinegar, but that it even requires several hours to extract the mineral matter, by boiling vinegar, from a pearl not bigger than a garden pea. While in neither case, moreover, can the pearl be thus made to disappear, as from the fact of the organic matrix of a pearl being totally insoluble in vinegar, even after every particle of its earthy substance has been removed, it still remains of the same shape, bulk, and almost identical appearance as before. Hence we fear that if the Cleopatra legend is to be believed at all, it requires considerable modifications ere it can be brought into harmony with scientific truth. There is, indeed, only one way in which a large pearl, such as the one Cleopatra is said to have employed, could be dissolved in vinegar at a supper-table, and that is by having it completely pulverized by a hard hammer and a strong arm before applying the vinegar to it. For once the mineral constituents of a pearl have been reduced to the state of an impalpable powder, they not only readily dissolve, but effervesce like a seidlitz powder—though much less strongly—when brought into contact with strong vinegar, and thus on their being diluted with water may be transformed into what might be called a cooling lover's potion, while from the organic matter having at the same time as the mineral constituents been minutely subdivided, its presence would scarcely be recognisable in the solution.]*

2nd. Composition of cocoa-nut pearls.

Qualitative analyses of pearls found in cocoa-nuts have been published by both Dr. J. Bacon and Dr. Kimminis.† But their analyses differ somewhat, for while Bacon found carbonate of lime and an organic substance akin to albumen, Kimminis met with nothing whatever in them except pure carbonate of lime. We subjected a portion of a garden pea sized cocoa-nut pearl, weighing 14 grains (kindly given to us by Messrs. Streeter) to analysis, and found that, like shell-fish pearls it consisted of carbonate of lime, organic matter (animal), and water.

The pearl which we examined was sent to Messrs. Streeter by their agent at Singapore (the same place from whence Bacon obtained his specimen), and as we stated last year (on June the 8th), when we exhibited at the *soirée* of the Royal Society both drawings and microscopic sections of it, we are exceedingly sceptical of the pearl we examined being in reality the product of a cocoa-nut, for the following reasons. It had all the external appearances of the pearls found in the large clams (*Tridacna gigas*) of the Southern Ocean, being perfectly globular, with a smooth, glistening, dull white surface, and resembling them exactly in microscopic structure. Besides which in chemical composition it bore no similarity to cocoa-nut milk, to which it is supposed to be related. For cocoa-nut milk is said to contain both the phosphate and the malate, but not the carbonate of lime. That there are pearls found in cocoa-nuts we do not presume to deny; all we mean to say is that we are doubtful if the specimen we examined had such an origin.‡

3rd. As regards mammalian pearls.

These so-called pearls have been met with in human beings and in

* Added March 27th, 1888.

† See 'Proceedings of the Boston Society of Natural History,' vol. 7, 1861, p. 290; vol. 8, 1862, p. 173; 'The Tropical Agriculturist,' April, 1887; and 'Nature,' 16th June, 1887 (Dr. Hickson and Mr. Thiselton Dyer).

‡ Since this paper was in type I have kindly had my attention drawn by Dr. Hickson to a letter from J. G. F. Riedel, of Utrecht, in 'Nature,' 15th September, 1887, in which he states that in 1886, while in North Celebes, he found a pearl "in the endosperm of the seed of the cocoa-nut." And that he has in his possession "two melati pearls (*Jasminium sambac*); one tjampaka pearl (*Michelia longifolia*), found in the flowers, according to the natives. One of the cocoa-nut pearls has a pear-shaped form, the length being 28 mm. The common name amongst the natives for this kind of pearl is mustika."—G. H., 1st March, 1888.

oxen. The first person who kindly called our attention to those of the ox was the late Professor Pannum, of Copenhagen, who in 1874 presented us with some specimens he had found in the gall-bladder of a Danish ox.

In so far as naked eye appearances are concerned, a good specimen of the variety of pearl now spoken of is quite undistinguishable from a fine specimen of oriental oyster pearl, from its not only being globular in shape, and of a pure white colour, but from its also possessing the iridescent sheen so characteristic of oriental oyster pearls of fine quality.

In chemical composition, however, mammalian pearls bear no similarity whatever to pearls found in shell-fish, for they are composed of an organic instead of an inorganic material, namely cholesterin. In minute structure again, they bear a marked resemblance to the crystalline variety of shell-fish pearls.

The quantitative analysis of human pearls yielded in 100 parts—

Water	2·05
Solids	97·95

The solids consisted of—

Cholesterin	98·63
Animal matter	1·37

From this it is seen that human pearls are in reality nothing more nor less than exceedingly pure cholesterin biliary concretions.

This note on the chemical composition of pearls is intended as a prelude to a paper we purpose shortly laying before the Society on the microscopic structure of the different varieties of pearls we had the honour of exhibiting sections of with the lime-light, as well as microscopic drawings, at the *soirée*, on the 8th June, 1887, and of which a detailed report was given in the 17th No. of the 'Cheltenham Ladies' College Magazine,' pp. 37—42, by J. F. Muspratt.

IV. "On the Vertebral Chain of Birds." By W. K. Parker, F.R.S. Received March 8, 1888.

A few years ago I noticed a remarkable fact in the development of the Green Turtle (*Chelone viridis*), namely, that whilst *thirteen* myotomes are developed in the cervical region, the intercalary vertebral segments found afterwards are only *eight*.*

More recently, whilst working out the development of the vertebræ in various types of Birds, it struck me that we have in these high forms creatures in which the vertebral chain has been greatly

* See "Challenger" Reports, *Zoology*, vol. 5, Plate 1, fig. 3, pp. 48 and 50.

shortened during their *secular development.* It seems to me to be probable that the Amphibian stock from which birds arose—becoming Reptiles in their ascent, but spurning that intermediate stage—were long, eel-like forms, not dissimilar to *Amphiuma* and *Menobranchus* among the existing Urodeles. I will therefore state what evidence there is of evolutional abbreviation in the development of the species in existing Birds.

Working with Foster and Balfour's 'Elements of Embryology' beside me, I was struck with one part of their description, and with my own preparations showing the phenomenon. At page 157 we read as follows:—

"The notochord [in the Chick] is on the sixth day at the maximum of its development, the changes which it henceforward undergoes being of a retrograde character.

"From the seventh day onward it is at various points encroached upon by its investment. Constrictions are thus produced, which first make their appearance in the intervertebral portions of the sacral region. In the cervical region, according to Gegenbaur, the intervertebral portions are not constricted till the ninth day, though as early as the seventh day constrictions are visible in the vertebral portions of the lower cervical vertebræ. By the ninth and tenth days, however, all the intervertebral portions have become distinctly constricted, and at the same time in such vertebral portions there have also appeared two constrictions, giving rise to a central and to two terminal enlargements. In the space therefore corresponding to each vertebra and its appropriate intervertebral portion, there are in all four constrictions and their enlargements."

I had long ago noticed, figured, and described a similar moniliform condition in the cephalic portion of the notochord in the Chick,* and this observation set me speculating upon the dying out of the axial segmentation in the region of the skull.

Now this peculiar *secondary* and *temporary* segmentation of the notochord is not equal throughout the whole chain of rudimentary vertebræ; I can only find *two beads* in the sacral region, and *none* in the caudal.

Nevertheless, taking these beadings as a true historical record of development, and allowing for them in such a bird as the Common Swan (*Cygnus olor*), we get, hypothetically, a very long ancestral form. In that bird there are thirty presacral, twenty-one sacral, and thirteen caudal vertebræ that are developed as distinct vertebral segments of the axis. Then, if we treble the presacrals and double the sacrals, we add eighty-one to the actual sixty-four of the modern bird, and thus obtain *more than twelve dozen*—145—vertebræ with which to accredit the ancestral form.

* 'Phil. Trans.,' 1869, Plate 81, figs. 2 and 7, and Plate 82, fig. 3, p. 771.

On the Number of Vertebræ in Existing Birds.

The Swan, one of the noblest of the *Precocial* birds, comes the nearest of any of the Carinatæ to the huge, almost wingless Struthious types in the large number of its vertebræ; indeed in the cervical region it has more vertebræ than any bird I have yet examined, namely, *twenty-five*, and its general sacral region is as long as in the large Ratitæ, so that this bird, although so exquisitely specialised as a flying, swimming, sailing, and walking bird, has not departed very far from the Struthious birds in respect of the length of the spine. More than this, in my recent researches into the development of this and cognate birds, I find that the Swan has been built upon the Struthious foundation—so to speak. In the parts that suspend themselves from the twenty-one sacral vertebræ, the hip-girdle moieties, it is most clearly seen that the difference in these parts between this bird and the African Ostrich (*Struthio camelus*) is altogether one of gentle transformation by a *late growth* of cartilage. Arrest the pelvis of the embryo Swan, when only two-thirds ripe, and in the ossification afterwards unite the *pubes* by ankylosis, and then the two pelves would correspond, point by point. So that this part of the skeleton passes in the most orderly manner, first through a general Reptilian, then through an Ornithoscelidan, and then through a Struthious stage, before it takes on the characteristic form of the Swan, the pelvis of which is one of the largest and most remarkable in the class, and quite typical, nevertheless, as the pelvis of a Carinate bird.

Returning to the Vertebral Chain, I may now show how, for adaptive purposes, that series of axial segments gets shorter and shorter as we ascend towards the smallest and highest of the "Altrices," the highest kind of birds, with tender young, and, as a rule, arboreal nidification.

Even within the limits of the Anatidæ, the family to which the Swan belongs, the cervical and sacral series get reduced to about three-fourths the number found in the Common Swan. Indeed, in the genus *Cygnus*, itself, I find a variation, for in *C. nigricollis* there are only twenty-four cervical vertebræ.

But among the larger Precocial birds the number varies extremely, and in passing from species to species, in the Cranes (Gruidæ), I find no two alike in this respect. Once, however, amongst the noblest and most intelligent of all the birds, the Passerines, and we come upon a uniformity that is as remarkable as the variety seen in the wading, and land, and water birds.

The Crows stand at the top of the Passerines, and being the largest kind, they have the longest vertebral chain.

In the old* Rook or Carrion Crow (*Corvus frugilegus* and *C. corone*),

* The fledgling is more generalised, and has twelve vertebræ united by the diapophyses in the sacral series.

the vertebral series is *twenty* in the presacral region, or only two-thirds as many as in the Common Swan, eleven in the sacral region, or half as many as are enclosed by *the ilia* in the Swan, whose first *caudal* corresponds with the last sacral of many birds.

Instead of sixty-four, I can only find forty-two vertebræ developed in a Crow, twenty pre-sacrals, eleven sacrals, and eleven caudals.

Now taking a familiar bird, the Common Chat (*Pratincola rubetra*), I find that it has only nineteen presacrals, eleven sacrals, and originally eleven caudals, but only *seven* distinct in the adult. Now we get in the *cervical region* in this little bird, *fourteen* vertebræ, one less than in the Crow, little more than half as many as in the Swan, and just twice as many as in the normal Mammal.

I take up the next that comes, the Yellow Wagtail (*Budytes rayi*), and it has the same number as the Chat; and, indeed, in only one species of Passerine bird, namely, *Petroica bicolor*,* from Western Australia, are there only *sixteen* free vertebræ in front of the *compound sacrum*.

As a rule, however, in the lesser birds of this Order the number is marvellously uniform, and agrees with what I have given above.

But the lesser species of Passerines amount in number to nearly one-third of the known species in the whole Class of the Carinatæ.

If it could be shown that the lesser singing birds had come up directly from the low ancestral forms, they yet suggest a rather long spine for that ancestor. It might have possessed ninety vertebræ. But I have by me most satisfactory proofs that the highest singing birds came through a series of forms that are traceable towards the Struthious birds, until, at last, I have no doubt of their merging into them.

The Passerines from the Notogæa, both east and west, have among them various genera that come short of the excellence of the general Arctogæal types. This is seen in the structure of their skulls, and in their vocal organs, and in the lower grade of their intelligence, whilst in the "Pteroptochidæ," the sternum itself—a sort of *anchor* to the classifier, which is very safe and sure in all the Passerines except in two or three genera—gives way at last, and in those birds has *five metasternal processes instead of three.*

In the smallest of all birds—the Humming-birds—the actual number of vertebræ varies very little from what is found in the lesser Passerines, but they are generally disposed of in a different manner; they may have as many as four pairs of developed ribs in the fore part of the sacrum, as in the largest kind (*Patagona gigas*); in lesser forms, as *Heliostrypha parzudakii*, *Diplogenia hesperus*, and also in the long-

* See Owen, 'Osteol. Catal. Mus. Coll. Surg.,' vol. 1, p. 299, No. 1584.

billed *Docimastes ensifer*,* I find only *three.* In the lesser Passerines, as a rule, only *one* pair of sacral ribs are developed.

On the Articulation of the Vertebræ in Birds.

Special modification of the vertebral chain takes place to a greater extent in birds than in any other of the Vertebrata. Even in the intensely modified vertebræ of Serpents, with their *zygosphene* and *zygantrum*, we still have merely the "procœlous" articulation of the centra.

But in birds, as soon as the *short-tailed forms* appear, we have as in Marsh's gigantic *Hesperornis*, or feeble-winged Colymbine Grebe with pleurodont teeth—the highest known form of the vertebral articulation, namely, the "cylindroidal" or "heterocœlous." This most accurate mode of locking the vertebral segments together, in which the centra viewed from below seem to be procœlous, but seen endwise or laterally are opisthocœlous, is peculiar, as far as I know, to birds, and apparently was not always, or from the beginning, present in them. This seems to be shown by the fact that the other of Marsh's toothed birds, namely, *Ichthyornis*, has for the most part "amphicœlous" vertebræ, only one or two joints at the upper part of the neck showing the cylindroidal articulation, and that imperfectly.

Now this is a most puzzling fact in Palæontology, for *Ichthyornis* is a Carinate bird, and as far as I can see, is the parent form of the Gulls (Laridæ), although it possesses *thecodont teeth* in its long jaws. Everyone knows that the Loons and Grebes (*Colymbus*, *Podilymbus*, and *Podiceps*) are "Pygopods," and rather of a low type, but the Gulls are amongst the noblest and most intelligent of the Palmipeds, and are *semi-altricial* in their breeding.

Now it is a fact that modern Grebes and Loons disagree with the other Pygopods in having all their presacral vertebræ cylindroidal, whilst the Alcidæ and the Penguins (Spheniscidæ) and Gulls have their dorsal vertebræ opisthocœlous. More than this, by careful examination of the fore end of the first sacral (*dorso-sacral*) vertebra in the lesser Gulls (*Larus canus*, *L. ridibundus*, *L. tridactylus*), I find that this is not a *ball* to fit accurately into the *cup* of the last free dorsal, but that its facet is sinuous, and does leave some space inside the joint.

Hence I infer, cautiously, but with some considerable degree of confidence, that the modern Gulls have not quite perfected even the lower or "opisthocœlous" form of articulation of the vertebral centra for *all* their dorsals.

I call the opisthocœlous mode of articulation *lower*, because it certainly comes short in type of the cylindroidal, and is I believe the

* In that bird, which is much smaller than *Patagona*, the whole skull is $5\frac{1}{3}$ inches long (137 mm.), and the rest of the axis $2\frac{1}{4}$ inches (56 mm).

more common kind of articulation in Archaic Reptiles, whilst the "procœlous" mode is almost universal in the existing Reptiles.

But, in fact, Birds are very *eclectic* in the manner in which their vertebral centra are articulated, and any kind of articulation that happens to be the best for the particular region in which it is found is selected, so to speak.

I will show, 1st, in what families the dorsal vertebræ are opisthocœlous; 2ndly, the modification in Birds of that type of articulation; and then how many sorts of articulation they exhibit in this or that Family.

In his valuable memoir on the Penguins,* the late Professor Morrison Watson greatly understates the number of Families that have this peculiarity, namely, the Penguins and the Auks. Now amongst the Steganopods or Pelicanine types they are found in the Cormorants and Darters (*Plotus*).

Amongst the Old World Pygopods this structure occurs in all the Alcidæ—*Alca*, *Uria*, *Ciceronia*, &c., and in all the Charadriomorphæ or Shore birds (Limicolæ), and in the Gulls (Laridæ, Lestridæ, &c.), but not in the Petrel tribe—Procellaridæ.

But amongst the most highly specialised arboreal "Altrices" I have long been familiar with this peculiarity in the great Parrot Family—Psittacidæ—in which, strangely enough, it is combined with a very unlooked for character, namely, with terminal epiphyses—a structure which begins to show itself in the Ornithorhynchus, in the caudal region.

This is very remarkable in these high, hot-blooded birds, for in the whole class epiphyses are very rare, *only one* being constant; this is found in the *cnemial crest* of the tibia.

But the Parrots are not the only high kinds of birds in which the dorsal vertebræ are opisthocœlous; I have within the last two years found it in that remarkable type, the Oil Bird (*Steatornis caripensis*), an *archaic, frugivorous Goat-sucker*—a bird which has no near allies, a crepuscular Cave-dweller, found only in Cumana and a neighbouring island, and manifestly a waif from a nearly lost group.

On the Modification of the Opisthocœlian Articulation in Birds.

The *cup* and *ball* in these opisthocœlous dorsals of birds is very different from what is found in the procœlous vertebræ of the Ophidia; in them it is fairly circular or hemispherical, whilst in birds it is generally scarcely more than three-fifths of an ellipse, and the upper margin is emarginate, having a concave outline answering to the general concavity of the floor of the spinal canal.

That which shows such intense specialisation in the procœlous vertebræ of the Serpents is the remarkable manner in which an

* "Challenger" Reports, *Zoology*, vol. 7, p. 16.

additional upper pair of confluent pre-zygapophyses form what Owen calls a "zygosphene;" this fits into a double cavity—the "zygantrum."

Now the articulation of the opisthocœlous dorsals of the birds thus mentioned is a complication of the articulation of centrum with centrum, and not any special modification, in their case, of the neural arch from which the zygapophyses spring.

In Reptiles, as far as I can see, whether existing, or otherwise, there is nothing like what I am about to describe; if any Palæontologist will show me a similar structure I shall be most glad to know of it. Such a fact would tell us how carefully these highly metamorphosed types, the Birds, have kept along Reptilian lines; if not, if no such structure as this, any more than the cylindroidal articulation, is ever seen in Reptiles, then we have another instance of the manner in which the Birds have proceeded beyond the excellencies of their progenitors.

The greatest perfection of this complex opisthocœlian articulation of the dorsal vertebræ is best seen in some remarkable Charadrian birds; three of which are Neotropical, whilst one is found in Kerguelen's Island; I refer to *Chionis*, *Attagis*, and *Thinocorus*.

In *Attagis gayi*, a Neotropical bird of the Plover family, stouter than a Lapwing, but about the same size, a nearly extinct type, and very archaic, I find the best instance of this Ornithic modification of the opisthocœlian articulation of the dorsal vertebræ. On the hind face of the centrum the *cup* in its fresh state is heart-shaped; it is half a long ellipse, with its upper edge gently emarginate. There is a strong annular "meniscus," 1·5 mm. deep below, and 0·6 mm. wide for the rest of its extent. It is a very solid fibro-cartilage, except for a small extent above, where it is finished by a ligamentous part. When this meniscus, which partly divides the joint cavity into two spaces, is removed, the hollow cartilaginous tract is seen to be in three parts; below, a semicircular hollow, marked in its middle by the notochordal "suspensory ligament," and above, on each side, a flat ear-shaped additional facet. These two facets look equally downwards and backwards, and they lie obliquely on a similar pair of facets over the ball on the fore-end of the centrum of the next vertebra which looks upwards and forwards. These well-fitting oblique facets, fore and aft, are, indeed, additional zygapophyses, arising not from the neural arch, but from the centrum; and they check the movement of the cup-and-ball joint. For a bird needs not only a very long and absolutely ankylosed sacrum, it must also have a very strong dorsal series; not unfrequently all but the last of this series are also ankylosed together; this only takes place in birds which have their dorsals cylindroidal.

On the Presence of Procœlous Vertebræ in Birds, and of the Imperfect and Irregular Joints between the Centra.

The modern procœlous Reptilian form of vertebral articulation is not altogether wanting in Birds. The atlas, although devoid of its proper centrum, forms a more or less perfect joint of this kind in all birds; it is crescentic in many of the Precoces, and circular in most of the Altrices; and in the latter it is not notched above for the "odontoid or suspensory ligament," but perforated.

But in many of the higher or Altricial birds the last two movable joints in the caudal series become procœlous, and also acquire a joint-cavity. The rest of that series have a sub-concave joint, with an intervertebral fibro-cartilage filling in the slight interspace; these joints, however, retain the suspensory ligament like all the rest, and towards the end of this series the centrum is perforated by this remnant of the notochord, as in lower types.

The joint formed by the hind part of the *atlas* and fore-part of the *axis* is irregular; it cannot be classified with any of the other modes of articulation, but this arises from the fact that it is formed between the cortical or inferior part only of those two vertebræ. The two first vertebræ are greatly modified in all the "Amniota," an anticipation of which is found in the Urodelous Amphibia.*

There are two main varieties, in Carinate birds, of the articulation of the atlas with the occipital condyle, and of the atlas with the axis.

These correspond on the whole with the Natural Division of birds into "Altrices" and "Precoces"; the Piping Crow of Australia (*Gymnorhina tibicen*) may be taken as an example of the first, and the Australian Bustard (*Eupodotis australis*) of the second kind.

In *Gymnorhina* the atlantal (procœlous) cup is a perfect hemisphere, but near its upper rim the suspensory ligament passes through a small hole to reach the basi-occipital. This cup fits well under the hemispherical occipital condyle; it is in position intermediate between that condyle and the true atlantal centrum. The hind face of this imperfect vertebral body is scooped so as to form a crescentic groove, with the concavity upwards; the convex fore-end of the axis fits into this groove, and the atlas grows under the joint as a bilobate and carinate process; the joint is a *crescentic condyle* with its concavity looking upwards.

* In many of these there is an imperfect vertebra between that which is articulated with the two occipital condyles; it is evidently an *atlas* with an imperfect neural arch, and the median and lateral elements of which become fused to form the odontoid process. The perfect vertebra next following is evidently the *axis*, but has the atlantal function of carrying the skull. See Wiedersheim, 'On *Salamandrina perspicillata*,' Genoa, 1875, Plates 2—4, and my papers, "On the Skulls of the Urodeles," 'Linn. Soc. Trans.,' Ser. 2, Zool., vol. 2, Plates 14—21, and 'Zool. Soc. Trans.,' vol. 9, Plate 40.

In *Eupodotis* the procœlous facet of the atlas is a crescent with horns approximating, and between these the odontoid process, or true atlantal centrum, appears; it is embraced by these "horns," and, as in the other type, is tied to the basi-occipital by the suspensory ligament. In this bird, contrary to the rule, the atlas does not grow under the axis, and the joint between them is almost procœlous; and in this and still more in some other Precoces, the occipital articulation is transversely enlarged, *i.e.*, shows signs of being double, as in Amphibia, the *notochordal dimple* answering to the wide interspace between the condyles in these forms.

I shall explain these things more perfectly when I come to the "intercentra."

The *imperfect joints* are those of the sacrum and the coccygeal bones. The long general sacrum of a bird does not correspond to the special sacrum of a Reptile or a Mammal, and in the dorsal region of this long series the articulations are, at first, like those of the free dorsals in front of them; *i.e.*, they are cylindroidal or opisthocœlous, as the case may be. But as we approach the true sacral region, between the acetabula, the faces of the centra are roughly flat, and the centra themselves are transverse subcrescentic blocks, with all the intercentral structures aborted.

The same thing takes place in the *ploughshare* or coccygeal bone, which finishes the chain by a series of from *four to six*, more and more imperfect, segments, from which, for a time, in the embryo the notochord projects, uncovered, behind.

There are other ankylosed parts of the vertebral chain besides the sacrum and the coccygeal bone; in these the parts are normal at first, becoming afterwards fused together. It is very common for the last cervical (whose free rib does not unite by a short piece with the sternum) to be fused with the dorsals—all but the last, which remains free, as in Falcons, Pigeons, Fowls, &c. The same thing takes place in many of the Crane family, but generally with fewer bones. In the Hornbills (Buceridæ) the atlas and axis become ankylosed. In some other Altrices we have there found that which is normally the last free dorsal fused with the first of the dorso-sacral series; and in others the first dorsal sacral, covered by the iliac bones, remains free; this is, however, a very irregular modification, and is sometimes due to old age in one case, and to a somewhat immature condition in the other.

In the present paper I cannot go into details as to the various modifications of the neural arches, with their zygapophyses and spines, nor describe the various outgrowths below that arise from the centra. But there are *distinct parts* of the vertebra that must be mentioned; these are the "intercentra" and ribs.

On the Intercentra of Birds.

I have not spoken of the neural arches as actually distinct from the centra; they are, as bony tracts, for a time, but the great *heat* and *haste* of the development of an embryo bird causes many essentially distinct parts to be converted into hyaline cartilage continuously; such distinct morphological regions, however, are very apt to assert their independence for a few weeks during the growth of the young bird, and although separate osseous centres in a continuous tract of hyaline cartilage are apt to be very inconstant as to the share they take in the work, yet, on the whole, in default of the primary segmentation of the cartilage, they are very valuable landmarks.

In a survey of this subject from below upwards, it is well known that the neural arches come before the centra; that establishes their independence and importance.

It is very difficult to put this matter into a small compass, and to show throughout the whole of the Vertebrata what parts of a vertebra are important autogenous "elements" and what are mere *apophyses* or outgrowths. The *old pre-embryological nomenclature* fails us here, entirely.

Nothing *newer* and nothing *better* has been said upon this subject than by Baur, whose wide acquaintance with the extinct forms that lie between Birds above, and Fishes below, makes him, on the whole, an excellent guide.

In some "General Notes" [extracted from the 'American Naturalist,' October, 1887, pp. 942—945] Dr. Baur (p. 945) gives his "results" as follows:—

"1. That the ribs are *intervertebral.*

"2. The ribs are originally one-headed and connected with well-developed intercentra.

"3. All forms and connexions of the other ribs can be derived from that condition.

"4. The lower arches of the caudal vertebræ are either formed by true ribs, the oldest fishes (Ganoidei, Dipnoi), or by processes of the intercentra (Teleostei, Stapedifera).

"5. The connexion between the Dipnoi and the Stapedifera is still missing.

"6. Some remarks on the nomenclature of the elements of the vertebral column:—

"Owen's names, 'neurapophysis' and 'pleurapophysis,' are not correct; the neural and pleural arches are no processes of the vertebræ, but are distinct parts.

"The two elements composing the neural arch ought to be called the '*neuroids,*' the two elements composing the pleural arch the '*pleuroids.*'

"The spines connected with the neuroids ought to be called, as before, *neural spines*; those connected with the pleuroids, *pleural spines.*

"The real centrum of the vertebra ought to be called *centrum;* the lateral elements composing it *hemicentra* (Albrecht), not pleurocentra.

"The name intercentrum ought to be preserved.

"The part of the intercentrum, centrum, or neuroid to which the capitulum is articulated, may retain the name *parapophysis*; the part of the centrum or neuroid to which the tuberculum is articulated may retain the name *diapophysis.*"

If we consider the structure of a bird as compared with a Reptile or a long-tailed Mammal, it would seem to have no necessity for the development of "chevron-bones" or *intercentra*; yet these elements are constantly present at the two extremities of the vertebral chain, although in the hind-part they are often not more developed than those seen in the lumbar region of the Mole (*Talpa europœa*).

If all birds have come up to us through forms similar to the *Archœopteryx*, then there must have been a slow, secular degradation of these inferior arches: that view, however, places the Toothed Birds of the Cretaceous Period as far from those Saururous types as the Birds of the present time.

That the aquatic, gill-bearing forms from which, originally, the Reptile and the Bird both arose were long-tailed, I have not the least doubt. One thing, however, I never can see, and that is that there was any absolute necessity that there should be *just one pair* of those old quasi-larval Dipnoans (or Amphibians) that had, at that immeasurably remote epoch, "the promise and potency" of all those Reptiles and Birds that *we know* have arisen, and of all those myriads of others of which we know nothing.

As the times became ripe for the harvest of scaly and feathered forms, they did appear, but had they all one father and one mother?

Another question to be asked is, Were there ever any *per saltum* rises in the scale; did all those nobler and still nobler forms acquire their varying degrees of excellency, from a low Reptile to a high Singing-bird, by the slow accretion of growth, and almost imperceptible change of structure, and increase of faculty?

It would greatly relieve my mind if it could be shown that the most probable hypothesis is that the swarm of old Perennibranchiates in a thousand places, and at varying times, changed for the better; became sometimes rapidly, at other times more slowly, transformed as the occasions arose; when the dilemma was *transform or die.* That is the dilemma, now, to all our native Amphibia year by year, and that which takes place now in forms that rapidly rise to a great height above their former selves, may have taken place in the past on a grander scale, and with centuries for days.

However it came about, the *Saururous* (*long-tailed*) forms have become *Nothurous*, have a mere *bastard tail* or stump. Yet this morphological feat is performed in the transformation of any Tadpole in "a month of days," hence the real difficulty does not lie with Nature, but with us.

But in studying the abortive chevron-bones of birds we shall find that these high and marvellously transformed types are not short-tailed, if we consider *number* merely; it is the peculiar contraction and *packing*—consolidation—these segments have undergone that make them to differ so greatly from Reptiles and Saururous birds.*

In the Common Swan (*Cygnus olor*), behind the four true sacrals there are ten "urosacrals" fused with the long *post-ilia*; then come seven simple, and one compound, bone, composed in the *cygnet* of five bony segments and an unossified rudiment behind, six altogether. We thus get, even allowing for four sacrals, twenty-three vertebræ, more or less developed behind the outgoing sacral nerves, whilst the *Archæopteryx* appears to have had only twenty-one caudal vertebræ (See 'Zool. Soc. Proc.,' 1863, p. 517).

Now of these post-sacral vertebræ of the Swan nearly the hinder half have rudimentary intercentra. These are very small, those in the middle of the series being the largest. In the cygnet about a month after hatching, the first is beneath the third movable joint, and the last under the last cartilaginous interspace but one, in the series of imperfect segments that form the "ploughshare bone;" thus there are *eight* in all.

But there are intercentra at the other end of the chain; these I have studied in the Cygnet, in the ripe embryo of the Mooruk (*Casuarius bennettii*) and in various other birds, especially Carinatæ; whilst my son (T. J. Parker) has worked them out in the embryo of *Apteryx*. In these embyo, and young birds, there are always found the following osseous centres in the atlas and axis, namely, a pair for the neural arch of each vertebra, and one for the so-called "body" of the atlas, one for the odontoid process of the axis, and two for the body of the axis, not right and left, but *one before the other*.

The osseous centre in the cartilaginous odontoid process is strung upon the notochord, like the rest of the centra; it is the specialised

* This subject has long been on my mind; lately Dr. Baur unearthed an almost forgotten paper of mine on the tail of modern birds. See his "W. K. Parker's Bemerkungen über Archæopteryx, 1864, und seine Zusammenstellung der hauptsächlichsten Litteratur über diesen Vogel," 'Zool. Anzeiger,' No. 216, 1886. My earliest paper on this special point was read at the Zoological Society on December 8, 1863. See 'Zool. Soc. Proc.,' 1863, pp. 511—518. It was "On the Position of the Crested Screamer (*Palamedea* [*Chauna*] *chavaria*)."

and segmented centrum of the atlas, whilst the much larger bony centre to which it is attached, and which also is strung upon the notochord, is the centrum of the axis; they coalesce together, according to the rule, a *new* rule and part of the general transformation of an Amniotic type. But the so-called body of the atlas is in position between as well as below the occipital articulation, and is *cortical.* The lesser and foremost bone in the axis is also intermediate between as well as below the true centrum of the atlas and of the axis. I quite agree with Baur that these two bones are *intercentra,* although I am not ready with the "strong reasons" he can bring from every corner of Palæontology.

In considering both *intercentra* and *ribs,* there are two birds that have helped me most; these are the Swan and the Cormorant (*Phalacrocorax carbo*).

Whether faster or more slowly, the transformation of these two types from a Reptilian into an Avian form is certainly well worthy of our closest attention. The Ostrich tribe, a sort of half-way creatures, only help a little in this research; yet in tracing the stages of a Swan or of any other of the Anatidæ, there would appear to be nothing strange in the sudden arrest of one at the Struthious stage; we seem for a time to have before us a new kind of short-legged and web-footed bird of the Ostrich kind; *it does move,* however, it develops into a Carinate bird with a Desmognathous palate. I lay stress upon this, because, as I shall soon show, the Anatidæ hold with the Ratitæ in the matter of a perfect series of cervical ribs, as in the Crocodile, but more aborted, and soon fused with the vertebræ. Birds are very uniform, in all essentials, in their atlas and axis; but their caudal vertebræ differ just as much as the structures they support differ, *e.g.*, the "Rectrices," or tail quills, that form their double, fan-shaped, *third wing.*

The Cormorant puts its tail to a much greater variety of uses than the Swan; the component vertebræ of the former are stronger and have much larger intercentra to serve as levers to the *depressors* of the tail.

There are two movable caudals between the post-ilia in the Cormorant, and the second of these has a seed-like intercentrum that lies below and between the second caudal articulation. The next is much larger, and the rest are as long and twice as broad as the neural spines of the same vertebræ, and are ankylosed to the hinder bone; they lie well under the one in front, and form the lower third of the procœlous joint. The last or compound bone has *four* of these intercentra fused together and to the imperfect vertebræ to which they belong; thus this bone has a dilated and dentate base, the fore-part of which passes under three-fourths of the last simple vertebra, and is bilobate, whilst those in front are clavate. In some birds these intercentra have two crura, and these may meet below and form a *hæmal canal*; in the Cormorant they are solid, and are manifestly developed for *steering* purposes—as in the Kestrel or Windhover (*Falco tinnunculus*). The habits of that voracious, rapid, and powerful bird (the Cormorant) explain the teleology of these strong and solid intercentra of the tail.

Coming now to the *ribs*, my two chosen types, the Swan and the Cormorant, will be the best instances to show how thin the partition is between a hot-blooded bird, and a cold-blooded *generalised* Reptile, like the Crocodile. In my earlier papers on the Osteology of Birds, I wrote in a general and somewhat confused manner about *reptilian characters* in Birds; but Professor Huxley's inestimable paper "On the Classification of Birds" ('Zool. Soc. Proc.,' 1867, pp. 415—472), so thoroughly ventilated the relations of the two great classes, Reptiles and Birds, showing indeed that in a very true sense the two were one, a huge double class—base below and noble above—that if I am confused now, it is not the fault of my "guide."

It is perfectly true that the Ratitæ, on the whole, are the lowest, most generalised, and most reptilian of birds; but they have a high degree of ornithic specialisation in some parts, much beyond what is seen in some other birds that, on the whole, belong to a much higher level.

Now the Ratitæ are related to a large number of families of birds, that like themselves have cylindroidal vertebræ up to the sacrum; and there is an almost natural and complete series of these forms, Tinamous, Hemipods, Fowls, &c., &c. But as I showed many years ago, the Duck-tribe and the Fowl-tribe have a skull which is fundamentally alike in both groups, and is unlike that of any other kind of bird's skull, and yet is easily derivable from the Struthious type, by this and that gentle metamorphic alteration.

But if the Cormorant and his relatives were each derived from Ratitæ, *they* must have been quite unlike those now existing; a Swan, strange as the assertion may sound, is modified from an essentially Struthious embryo. I have traced it step by step.

But the Cormorant, and the Darter (*Plotus*), its nearest relative, seem more like a survival of transformed Plesiosaurs, and their Vertebral Chain is so intensely Reptilian that, among living forms, the Crocodile is the best guide to the morphologist in its interpretation.

On the Ribs of Birds.

I will first describe the ribs of the Swan, and then those of the Cormorant.

In *Cygnus olor*, as in all the normal "Chenomorphæ," the vertebral artery, right and left, runs inside a series of bridges, which, eked out by strong membrane, form a canal all along the neck. The piers of these small bridges are formed by the upper and lower transverse processes (*diapophyses* and *parapophyses*); the arches by arrested ribs—"pleuroids." As a rule, in the Carinatæ, these are not developed on the *axis* and *atlas;* but in the Anatidæ, as in the Ratitæ, generally, they are found in them also. The arch on the atlas is a strong but narrow bar; in the Cygnet of a month old there is in it a styloid bony rib, placed subvertically. The rest are larger, are horizontally placed, and have a free styloid end, which in many cases almost reaches to the end of the centrum of the next vertebra. These *riblets* have but little primary independence as cartilages; but they ossify separately; they are clavate, and this clubbed fore-end has thus no distinction of "capitulum" and "tuberculum," although the lower edge answers to the one, and the upper to the other.

In the twenty-second vertebra the styloid part is lost, and only a broad vertical bridge is developed by the "pleuroid;" in the twenty-third only a narrow bridge, like that on the *atlas*, but stouter. On the twenty-fourth and twenty-fifth the ribs are segmented off, have double heads, and remain free, although they do not form a perfect arch by reaching the sternum; indeed the last but one is very short. In these two vertebræ the facet for the capitulum is on the centrum, opposite the lower part of the facet of the centrum; that for the tuberculum is on the diapophysis. Thence along the five free dorsals and the two first dorso-sacrals, the joint for the tuberculum (the parapophysis) gets gradually higher, so that in the two last it lies over where the suture was between the centrum and neurapophysis. The developed ribs of the third and fourth sacral (dorso-sacral), have lost their capitulum, and articulate only by their tuberculum on the diapophysis.

The last three vertebræ of the seven that buttress the *pre-ilia*, have only a generalised mass, right and left; and on the next four, the true sacrals, these are either gone, or reduced to mere prickles. The twelfth and thirteenth have strong pleural bars, not segmented off in the cartilaginous condition, but they are ossified as distinct bars; these coalesce with the centrum and diapophysis. Behind these, in the Cygnet, there are no "pleuroids," but in a recently hatched Duckling (*Anas boschas, domesticus*), I find *five* pairs of these little *rib-bars* to the fore-half of the Urosacral series.

Thus there are *thirty-four* pairs of ribs, rudimentary or developed, without a break, in the Common Swan, and then an attempt at forming a new series behind the sacral nerves. Also, let it be noticed, that the first two pairs of pleuroids, or rib-rudiments, *arise from inter-centra*, whilst the last two of the twenty-nine have lost their capitulum, or primary head, and are articulated by their tuberculum or secondary head to the diapophysis, an outgrowth of the neural arch (neuroid).

Thus we have in a single vertebral chain an epitome of the history of the evolution of ribs. Towards the end of that chain, the vertebræ and ribs form the upper part of the most highly specialised thoracic cage in existence, it is the last consummation of the whole evolutional series, the furthest from the beginning made by the *Ammocœte*, when it has just been metamorphosed into a Lamprey.

In the Cormorant, one of the lower forms of the Pelecanine Family ("Steganopods," "Dysporomorphæ"), the vertebral chain is much more archaic than in either the Swan, or even the Ostrich and its kindred.

Here, indeed, we miss the atlantal rib, but rudiments are present on the *axis*, and these are attached to an ankylosed intercentrum.

On the whole, the greater number of the styloid cervical ribs are like those of the Swan, except that the upper edge of the free style is not connected with the neural arch by an ossified aponeurosis. There are only three presacral vertebræ that have developed ribs attached to sternal pieces, and thus forming perfect cinctures, finished below by the common inverted keystone or sternum. The ribs on the last two cervicals, the nineteenth and twentieth, have perfect heads, and have uncinate pieces attached and ankylosed to them, but their sternals are suppressed. In front of them there are three vertebræ, with non-segmented riblets, that have no retral style; these are mere *necks* of a developed rib, and run almost horizontally from the centrum to the large diapophysis; they are, in fact, similar to, but much stronger than, the atlantal rib of the Swan. The parapophysis in these three vertebræ stretches straight out from the centrum, which is also alate behind it, and these bars enclose a large foramen, 8 mm. wide and 4 mm. high. The nineteenth cervical, with its developed vertebral rib, forms for the capitulum of that rib a deep cup with two distinct facets, so that the head of the rib articulates in a manner similar to what is seen in Mammals. In them, however, the two facets are one in front of the other, and on distinct vertebræ; here they are one above the other, and near the fore-end of the same vertebra, one is on the centrum, and the other is on the neural arch. The facet on the centrum is higher than the junction of capitulum and centrum, in the non-segmented rib next in front. In the last cervical, the lower facet is still higher, but is on the centrum; both these pairs of ribs have a long neck and the normal articulation of the tuberculum with the under face of the end of the large diapophysis, an outgrowth of the neural arch.

In the three dorsals the parapophysial cup for the capitulum is entirely on the neural arch, and, from before, backwards, it keeps rising to a higher point in that arch. Thus in a few vertebræ we have the capitulum rising from a point *where the intercentrum would be* if it were developed, to a point quite clear of, and some height above, the centrum itself. The first general sacral vertebra is similar to the last free dorsal; its vertebral rib has a perfect sternal piece, and thus there are four complete cinctures to the thorax. The last developed rib is feebler, and its sternal piece does not quite reach the sternum. There is a dia-parapophysial facet for its feeble upper part; it is a cup nearly as large as the diapophysial facet in front of it, and the cartilage lining the cup is extended downwards on a narrow convexity of the transversely carinate outgrowth, and thus this rib, with a small head, and a neck less than half the normal size, articulates by one continuous facet belonging to both tuberculum and capitulum, and entirely on the neural arch.

The third vertebra in the general sacral series has a pair of ribs; these have lost their capitulum entirely; they are mere rods, 6 mm. long and 0·75 mm. thick, and are ankylosed by their inner twisted end to the diapophyses.

After these come three pairs of strong *pre-iliac buttresses*—generalised masses, from which all trace of rudimentary ribs has gone—in the old bird. Then come two vertebræ with the bodies nearly devoid of lower outgrowths; these are the true sacrals. These are followed by the urosacrals, the first of which has strong rib-bars that buttress the *post-ilia*, and that are ossified as distinct riblets, but are not segmented off as distinct tracts of cartilage in the embryo. But in old birds the buttresses of the second true sacral are not quite absorbed, but remain as prickles, for the clearing away of unnecessary parts goes on even after the bird is adult. This is only one among many instances that could be adduced in which the transformation of the skeleton is seen to be continued throughout life. In that transformation, from beginning to end, each individual bird repeats the story of its birth in the past ages, and each individual bird seems to be striving towards some goal, albeit in its present state, when adult, its structure is to the morphologist an absolutely perfect thing.

In birds, as a rule, the true sacrals abort, or even suppress, the pleuroid rudiments in the true sacrals; four of these block-like vertebræ form the sacrum proper of the Swan; two only in the Cormorant.*

* Professor Huxley (*op. cit.*, p. 416), in this *third* character of Birds as distinguished from Reptiles, says that, "Although all birds possess a remarkably large sacrum, the vertebræ, through the intervertebral foramina of which the roots of the sacral plexus (and, consequently, of the great sciatic nerve) pass, are not provided with expanded ribs abutting against the ilium externally, and against the bodies of these vertebra by their inner ends." Those true sacrals are called 'lumbo-sacral' by Professor Mivart ('Zool. Soc. Trans.,' vol. 10, p. 345, Plate 61, fig. 1), whilst the first two "uro-sacrals" are called "sacral." This is certainly an erroneous nomenclature.

Professor Mivart speaks of his examination of the skeleton of *P. bicristatus* and *P. brasiliensis*, as well as of *P. carbo*. His figure of the pelvis is probably one of these, and not of *P. carbo*; it differs from the two old specimens of the common

I shall finish this paper with one more instance. In most birds the true sacrals have only the upper transverse processes, or diapophyses; the lower bars, or arrested "pleuroids," are entirely gone in the adult, but small prickles remain, often more on one side than on the other. Thus the spaces for the large sacral nerves and their ganglia, and for the lobes of the kidneys, are not quite cleared. In the Tiger-Bittern (*Tigrisoma leucolophum*), a Neotropical member of the "Ardeidæ," there is no vertebra in the sacrum, until we come to the last three uro-sacrals, that has not its inferior or "pleuroid" bars.

The sacrum of this bird is composed of fifteen vertebræ, the first has developed ribs, with imperfect sternal pieces, the next two have small ankylosed ribs, separated for some distance from the diapophyses. Then come three with stout generalised pre-iliac buttresses. The next *six* have inferior rib-bars, those of the last four are strong, those of the first two weak. On the left side the second of these rods is membranous for a short extent; on the right side it is imperfect in its outer part, it is a mere prickle growing from the centrum. Except on the atlas this bird has ribs or rudiments of ribs up to the twelfth sacral. I suspect that if the ancestral form from which the Tiger-bittern arose could be put face to face with its stilted descendant, the two would differ as much as the vermiform larva of *Tipula oleracea* differs from its winged and stilted imago.

V. "Second Preliminary Note on the Development of *Apteryx*." By T. Jeffery Parker, B.Sc., C.M.Z.S., Professor of Biology in the University of Otago. Communicated by W. K. Parker, F.R.S. Received March 8, 1888.

The materials for the present investigation consist of embryos of the three common species of *Apteryx*, viz., *A. australis*, *A. oweni*, and *A. mantelli*. Most of them, including all the earlier stages, were collected for me by Mr. R. Henry, of Lake Te Anau; a nearly ripe embryo of *A. mantelli* was obtained from Mr. A. Reischek: and I am indebted to Mme. Müller for a half-ripe specimen of *A. oweni*, and to Sir Walter Buller for two, somewhat older, of *A. mantelli*.

I desire to record my sincere thanks to the Council of the Royal Society for the grant which has enabled me to defray the expenses of the investigation.

My observations are far from complete, and deal only with comparatively late stages. The eggs of *Apteryx* are at all times difficult to obtain, as evidenced by their high market value, and Mr. Henry is

bird dissected by me in having the pre-ilia buttressed by *seven* pairs of massive processes instead of *six* by having only *one* true sacral, and by showing strong costal bars on both the first and second "uro-sacral."

the only collector I have yet met with who was willing to give his time not only to collecting the eggs, but to removing and preserving the embryos. I have, unfortunately, never been able to leave Dunedin during the breeding season so as to try and procure the earlier stages, the removal and preservation of which could not be entrusted to a collector.

My first Stage (A) corresponds roughly with a chick embryo of the fourth day of incubation: the full number of mesoblastic somites —about 44—has already appeared. Stage B is apparently only a few hours older: Stage C corresponds very nearly with a fifth day chick, except that the limbs are in a less advanced condition.

Stage D is in about the same state of development as a chick of the 7th day; it was unfortunately damaged by the collector during removal from the egg, both fore-limbs being destroyed. Avian characters are now definitely assumed, the head being produced into a short beak very like that of a chick at the corresponding period. The hind-limbs are still in the primitive position, *i.e.*, stretched out at right angles to the long axis of the body, but their extremities are dilated into flattened paw-like feet which distinctly show the three principal digits and a small knob-like hallux.

Stage E is a little later than D, and is chiefly interesting for the condition of the fore-limb, which is terminated by a tridactyle paw with sub-equal digits. In the hind-limb the cnemial flexure has appeared, but not the mesotarsal flexure, so that the combined crus and pes are directed backwards.

In Stage F the characteristic features of the genus *Apteryx* are assumed, the beak having undergone a great increase in length and bearing the nostrils at the tip. The fore-limb is now a true wing, the manus being supported mainly by the second digit, but presenting blunt projections on the pre- and post-axial borders of the wrist which indicate respectively the positions of the reduced first and third digits. In the hind-limb the mesotarsal flexure has appeared, and the pes has nearly assumed its adult characters. The feather-papillæ appear first in this stage.

In Stage G the feather-papillæ have become larger and more widely distributed; the beak and the hind-limbs have further increased in length, and the wing shows no trace externally of either the first or the third digit. In all the remaining stages the adult form is assumed and the body covered with feathers.

Contrary to the usual statements as to the pterylosis of the Ratitæ, *Apteryx* has distinct apteria, which are especially noticeable in the earlier stages.* In Stage F, in which the feather-papillæ first appear, they are arranged in fairly distinct dorsal, humeral, and femoral

* Pterylæ and apteria are figured in the embryo ostrich and referred to in the description of the figures by Miss B. Lindsay, 'Zool. Soc. Proc.,' 1885, Pl. XLIII.

tracts; the ventral tract appears in Stage G. In the ripe embryo and even in the adult, besides the narrow ventral space recognised by Nitzsch, there are well-marked lateral spaces separating the dorsal and ventral, and the dorsal and femoro-crural tracts from one another.

In the full paper a table and diagrams will be given showing the length of important parts of the body (head, beak, limbs, &c.), in the various stages, expressed as percentages of length of vertebral column. The table shows that while the wings attain their maximum relative size in Stage F, the legs continue to increase in proportional length some time after hatching. The brain-case, also, undergoes from Stage G onwards a proportional diminution in size, while the beak increases steadily up to adult life.

The greater part of the full paper will deal with the skeleton: a detailed description will be given of the entire skeleton at about the time of hatching, when all the more important ossifications have appeared and but little ankylosis has taken place.

The vertebral formula is—

$$\text{Cv. } 16. \quad \text{Th. } 5+\overbrace{3 : \text{L. } 8 : \text{S. } 3 : \text{Cd. } 3}+6-8.$$

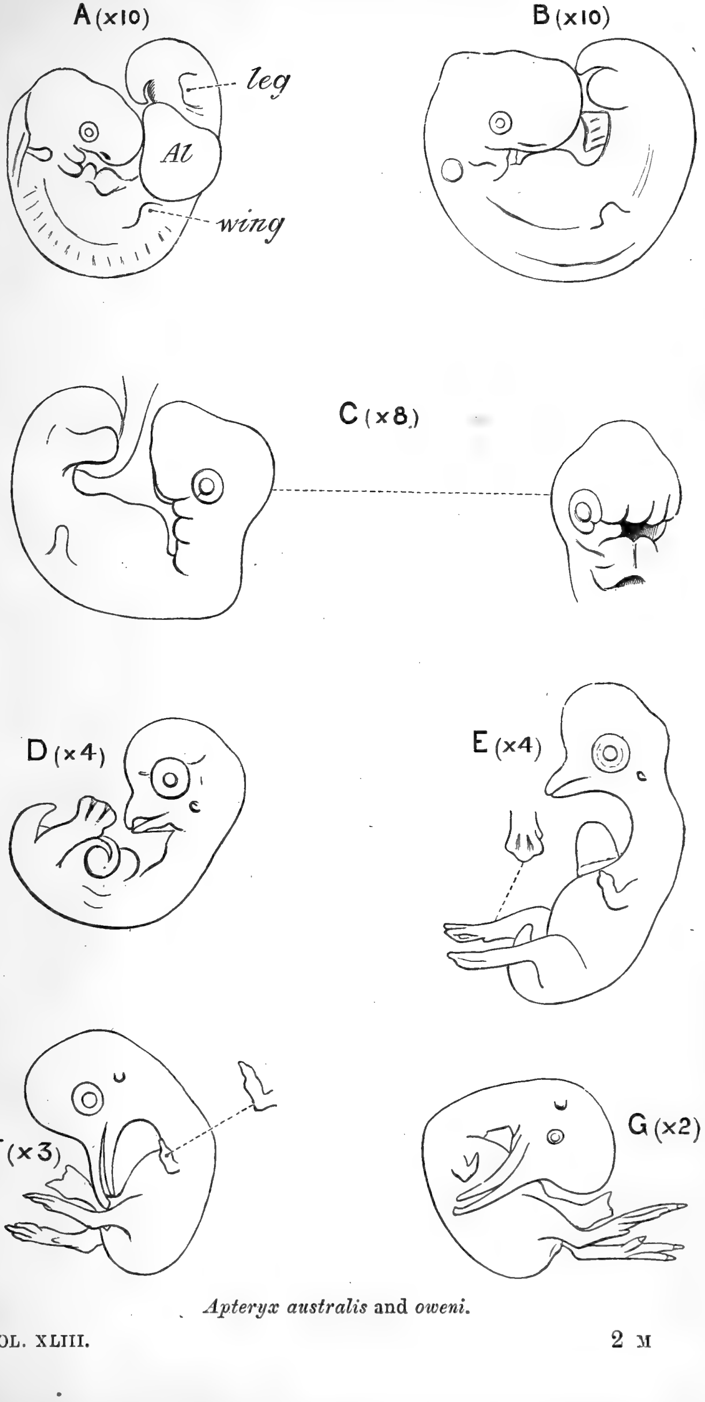
The bracket indicates that the last three thoracic, all the lumbar and sacral, and the first three caudal vertebræ are united to form the compound sacrum of the adult.

The axis vertebra is ossified by five centres, the additional one occurring in the antero-ventral region of the body, below the odontoid; this evidently represents an inter-centrum or inter-vertebral wedge-bone.

The cervical ribs appear to chondrify separately from the rest of the vertebræ; but further observations are needed on this point, as well as on the autogenously ossified transverse processes of the sacral vertebræ which in the youngest specimen hitherto examined are continuous with the vertebræ.

The skull differs so little except in details from that of other birds, that there is little to be said about it in an abstract. The chondrocranium of the ripe embryo and the separate membrane bones will be fully described and figured. As in other birds, I find no trace of Jacobson's organ; the capsule of the organ is, however, represented by a distinct (paired) rod of cartilage in the vomerine region, as in *Rhea*.

In Stages D—G the shoulder-girdle consists of a solid piece of cartilage having much the same shape as the adult bone. In Stage H an ossification appears in the scapular region, and another in the post-axial moiety of the coracoid region. In Stage I a fenestra appears, immediately pre-axiad of the coracoid ossification, dividing the ventral portion of the shoulder-girdle into procoracoid and coracoid



Apteryx australis and *oweni*.

regions. The resemblance at this stage to the shoulder-girdle of the ostrich is very close, but the late occurrence of the distinction between coracoid and procoracoid, and their formation by fenestration of a continuous cartilage, are remarkable. In Stage K the procoracoid has degenerated into a ligament which now forms the sole pre-axial boundary of the coracoid fenestra. Later, the coracoid ossification extends pre-axiad until the membrane of the fenestra is replaced by bone, but even in the adult the position of the fenestra is marked by the thin, often emarginate plate which forms the inner or pre-axial portion of the coracoid. The small aperture situated mesio-ventrad of the glenoid cavity, and sometimes described as the coracoid fenestra, serves for the transmission of a nerve.

In Stage E the manus contains three well-chondrified sub-equal digits; the carpals are not yet chondrified, and are only indicated by a concentration of nuclei in the blastema. In Stage F the second digit has increased out of all proportion to the first and third. In Stage G the pollex has degenerated, and its position is indicated only by a concentration of nuclei in the mesoblast; two well-marked carpals have appeared, one of which—the radiale—lies pre-axiad and slightly proximad of the other, which gives attachment to the second and third metacarpals. In Stage H the radiale lies entirely proximad of the second or distal carpal, which is closely applied to the proximal ends of the two metacarpals. In the newly-hatched bird the second and third metacarpals have ankylosed with one another and with the distal carpal, the radiale remaining separate.

In Stage G the pubis closely resembles that of a chick of the 6th day, the pubis being vertical and the ilium comparatively short. In Stage E the ilium has lengthened greatly, and the pubis forms an angle of 20° with the vertical. In G the adult form is assumed, and in H ossification has begun.

In Stage D the tarsus consists of three elements, tibiale, fibulare, and a single cartilage representing the five distalia. Besides the three functional digits of the adult, and the pollex, which at this stage has its normal connexion with the tarsus, there is a distinct vestige of the fifth metatarsal in the form of a rod-like cartilage, 0·4 mm. long. In Stage E the foot has elongated; the pollex has shifted distalwards, and is now attached to the pre-axial edge of the second metatarsal at about the middle of its length. The fifth metatarsal is still distinct, but has not increased in size; the tibiale and fibulare have united.

In Stage F the foot has nearly attained its adult form. The united tibiale and fibulare instead of being, as in the preceding stage, in close contact with the combined distalia, are separated from them by a narrow in-growth of connective tissue, the rudiment of the mesotarsal semilunar pad. The fifth metatarsal is still visible, but has undergone distinct retrogression, being only 0·15 mm. in length, and formed of indifferent tissue instead of hyaline cartilage. From this it would appear that the fifth metatarsal actually disappears in *Apteryx* instead of fusing with the fourth as in the chick.

In Stage G the proximal tarsals are closely applied to but have not yet united with the tibia; the distalia also are still distinct from the metatarsals. The rudiment of the mesotarsal semilunar pad has increased considerably, and in the centre of it a rounded nodule of hyaline cartilage has appeared, which I take to be the representative of the centrale tarsi, an element not hitherto recognised in birds.* In a recently-hatched specimen of *Apteryx australis*, it is a perfectly distinct cartilage about 2 mm. in diameter, imbedded in the fibrous tissue of the semilunar pad; in the adult it becomes ossified, attaining a diameter, in *A. oweni*, of about 5 mm.

As the scientific libraries to which I have access are small and imperfect, I take this opportunity of saying that I shall be extremely grateful to the authors of papers bearing upon the subjects of the present investigation who will favour me with separate copies.

[*Note.*—In no stage is there any trace of the hard knob on the beak which in birds generally assists the embryo to break the egg-shell.—March 21, 1888.]

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

Presents, March 22, 1888.

Transactions.

Baltimore:—Johns Hopkins University. Circular, No. 63. 4to. *Baltimore* 1888; Studies (Historical and Political Science). Ser. 5. No. 12. 8vo. *Baltimore* 1887; Report, 1887. 8vo. *Baltimore* 1888. The University.

Birmingham:—Free Libraries. Report, 1887. 8vo. *Birmingham* 1888. The Committee.

Edinburgh:—Royal Scottish Society of Arts. Transactions. Vol. XII. Part 1. 8vo. *Edinburgh* 1881. The Society.

Kew:—Royal Gardens. Bulletin. 1888. No. 15. 8vo. *London.* The Director.

London:—British Museum (Natural History). Catalogue of the Fossil Mammalia. Part V. 8vo. *London* 1887; Catalogue of

* It was figured, but not described, by Morse ("On the Intermedium," 'Anniversary Mem. Boston Soc. Nat. Hist.,' 1880, Plate I); and is figured and described as *naviculare vel* "centrale" by me in my paper "On the Morphology of Birds." (See Abstract, 'Roy. Soc. Proc.,' vol. 42, 1887, p. 58.)—W. K. P.

Transactions (*continued*).

the Birds. Vol. XII. 8vo. *London* 1888; Guide to the Shell and Starfish Galleries. 8vo. *London* 1887. The Trustees.

London Mathematical Society. Proceedings. Vol. XIX. Nos. 308–10. 8vo. [*London* 1888.] The Society.

Photographic Society of Great Britain. Journal and Transactions. Vol. XII. No. 5; List of Members. 8vo. *London* 1888. The Society.

Royal Institute of British Architects. Journal of Proceedings. Vol. IV. No. 10. 4to. *London* 1888. The Institute.

Royal Medical and Chirurgical Society. Proceedings. Vol. II. No. 7. 8vo. [*London* 1888.] The Society.

St. Bartholomew's Hospital. Reports. Vol. XXIII. 8vo. *London* 1887. The Hospital.

Society of Biblical Archæology. Proceedings. Vol. X. Parts 3–4. 8vo. *London* 1888. The Society.

Lund:—Universitet. Års-skrift. Tom. XXIII. 4to. *Lund* 1887–88. The University.

Manchester:—Literary and Philosophical Society. Memoirs. Ser. 3. Vol. X. 8vo. *London* 1887; Proceedings. Vol. XXV–XXVI. 8vo. *Manchester* 1887. The Society.

Tōkyō:—Imperial University of Japan. Calendar. 1887–88. 8vo. *Tōkyō* 1888. The University.

Turin:—R. Accademia delle Scienze. Atti. Vol. XXIII. Disp. 4–5. 8vo. *Torino* [1888]. The Academy.

Vienna:—K. Akademie der Wissenschaften. Anzeiger. 1888. Nr. IV, V. 8vo. *Wien.* The Academy.

Observations and Reports.

Bonn:—Sternwarte. Bonner Sternkarten. Ser. 2. Lief. 3–4. Obl. folio. *Bonn* 1887. The Director (Dr. Schönfeld).

Calcutta:—Meteorological Department, Government of India. Weather Charts of the Bay of Bengal. Obl. folio. *Calcutta* 1886–87. The Department.

Dorpat:—Sternwarte. Meteorologische Beobachtungen. 1887. October—December. 8vo. *Dorpat*; Privatbeobachtungen der Regenstation Alswig im Jahre 1886. Von Prof. Weirauch. 8vo. *Dorpat* 1887. The Observatory.

Melbourne:—Observatory. Monthly Record. October—December, 1887. 8vo. *Melbourne.* The Observatory.

Paris:—Service Hydrométrique du Bassin de la Seine. Résumé des Observations pendant l'Année 1886. 8vo. *Versailles* 1887; Observations sur les Cours d'Eau et la Pluie pendant l'Année 1886. Folio. *Versailles* [1887]. The Service.

Transactions (*continued*).

Pennsylvania:—Geological Survey. Report. 1886. Parts 1–2. 8vo. *Harrisburg* 1887. The Survey.

Burns (E.) The Coinage of Scotland, illustrated from the Cabinet of Thomas Coats, of Ferguslie, and other Collections. 3 vols. 4to. *Edinburgh* 1887. Mr. James Coats, Jun.

Dawson (Sir J. W.), F.R.S. The Geological History of Plants. 8vo. *New York* 1888. The Author.

Grensted (Rev. F. F.) A Theory to account for the Airless and Waterless condition of the Moon, with Geological and Physical Notes by T. M. Reade. 8vo. *Liverpool* 1888. The Author.

Monaco (Prince Albert de). Deuxième Campagne Scientifique de "l'Hirondelle" dans l'Atlantique Nord. 8vo. [*Paris*] 1887. With three other excerpts, in 4to. The Author.

Reynolds (J. E.), F.R.S. Experimental Chemistry for Junior Students. Parts 1–4. 12mo. *London* 1885–87. The Author.

Williamson (B.), F.R.S. An Elementary Treatise on the Integral Calculus. 8vo. *London* 1888. The Author.

"On the Voltaic Circles producible by the mutual Neutralisation of Acid and Alkaline Fluids, and on various related Forms of Electromotors." By C. R. Alder Wright, D.Sc., F.R.S., Lecturer on Chemistry and Physics, and C. Thompson, F.I.C., F.C.S., Demonstrator of Chemistry, in St. Mary's Hospital Medical School. Received January 18, —Read February 2, 1888.

(Abstract.)

The authors have examined a variety of cells analogous to Becquerel's "pile à oxygène;" *i.e.*, containing two platinum or other non-oxidisable plates, one immersed in an acid fluid, the other in an alkaline one, the two fluids being connected by a wet wick or siphon, and either some oxidising agent being also contained in the acid or some reducing substance in the alkali. In the first case continuous evolution of oxygen was brought about from the surface of the plate immersed in the alkali; in the second the converse phenomenon was observed, *i.e.*, hydrogen was continuously evolved from the plate in the acid; in each case the quantity of gas liberated was equivalent to the current passing as measured by a small silver voltameter. Thus the following figures were obtained in various experiments, the measurements being made after sufficient amounts

of current had passed to about saturate with oxygen or hydrogen respectively the fluid in the collecting tube, and so avoid loss by solution; in the first set the alkaline fluid was strong caustic soda solution; in the second somewhat diluted sulphuric acid (1 to 4 or 5 water) was the acid liquid. Carbon plates were used in experiments G and H; platinum ones in all the others.

I. *Cells in which Oxygen was evolved.*

A. Becquerel's "pile à oxygène." Concentrated nitric acid used.
B. Diluted sulphuric acid in which potassium permanganate had been dissolved.
C. Diluted sulphuric acid in which potassium dichromate had been dissolved.
D. Diluted sulphuric acid in which potassium ferricyanide had been dissolved.
E. Acid solution of ferric chloride.
F. A stronger acid solution of ferric chloride.
G. Hydrochloric acid saturated with chlorine.
H. Diluted sulphuric acid containing dissolved bromine.

Time in hours.	Milligrams of silver deposited.	Cubic centimetres of oxygen at 0° and 760 mm.	
		Equivalent to silver.	Actually collected.
A. 18	102	5·3	5·1
B. 10	85	4·41	4·30
C. 18	16	0·83	0·80
D. 42	12	0·62	0·45
E. 48	5	0·26	0·25
F. 48	18	0·93	0·85
G. 18	25	1·30	1·4
H. 18	46	2·38	2·3

II. *Cells in which Hydrogen was evolved.*

I. Concentrated solution of sodium hyposulphite (hydrosulphite Schützenberger) made strongly alkaline with caustic soda.
J. Strong caustic soda containing pyrogallol dissolved therein.
K. Alkaline fluid obtained by dissolving cuprous chloride in ammonia.
L. Similar fluid obtained from ferrous sulphate, ammonium chloride, and ammonia.

Time in hours.	Milligrams of silver deposited.	Cubic centimetres of hydrogen at 0° and 760 mm.	
		Equivalent to silver.	Actually collected.
I. 8	104	10·8	10·7
J. 22	76	7·9	7·8
K. 16	53	5·5	5·4
L. 20	36	3·7	3·6

Various reducing agents were found ineffective in causing hydrogen evolution in this way; thus no noticeable amount of hydrogen was produced when sodium sulphite or hypophosphite, potassium ferrocyanide, or manganous hydroxide and ammoniacal sal-ammoniac were used. Similarly, no oxygen evolution was observed when a mixture of sulphuric acid and barium dioxide, or hydrochloric acid containing iodine in solution, was the acid fluid. On the other hand, the oxygen absorbed by a platinum sponge aeration plate was sufficiently active to cause some four times as much permanent current to pass as was produced when a solid platinum plate was used immersed some centimetres below the surface of the acid.

By substituting various metals and caustic soda or ammonia solution for the platinum plate and alkaline solution containing a reducing substance, tolerably energetic cells were obtained; even in the case of metals not ordinarily regarded as belonging to the oxidisable class, solution was readily brought about when the alkaline fluid contained potassium cyanide. In all cases hydrogen was evolved from the surface of the opposed platinum plate immersed in sulphuric acid solution in quantity proportionate to the current passing, whilst a quantity of metal was dissolved usually sensibly equal to that representing the formation of the lowest oxide; tin dissolved to a somewhat less extent, indicating the production of some stannic oxide, from 3 to 7 per cent. of the metal being dissolved in this form, and the rest as stannous oxide; and mercury dissolved to form mercuric potassio-cyanide, 100 parts of metal dissolving for 108 of silver thrown down in the voltameter. Gold, silver, and palladium readily evolved hydrogen when immersed in cyanide solution; but platinum was ineffective, and iron gave only a faint action. Thus the following figures were obtained in various experiments:—

Metal used.	Alkaline fluid.	Time.	Milligrams of silver precipitated in voltameter.	Hydrogen liberated in cubic centimetres at 0° C. and 760 mm.	
				Equivalent to silver deposited.	Actually collected.
Tin	Caustic soda.	3 hrs.	107	11·08	11·0
,, ...	,,	2 ,,	137	14·2	14·2
Lead	,,	16 ,,	244	25·3	25·1
Copper	Ammonia.	42 ,,	11	1·14	1·0
,, ...	Potassium cyanide and caustic potash.	10 ,,	120	12·4	12·0
,, ...	,,	4 ,,	115	11·9	11·2
Silver	,,	6 ,,	69	7·15	6·9
Mercury	,,	24 ,,	78	8·08	8·0
Palladium........	,,	18 ,,	106	11·0	11·0
Gold	,,	14 ,,	141	14·6	14·55
Nickel	,,	18 ,,	45	4·7	4·5
Cobalt	,,	4 ,,	15	1·56	1·55

In all the cells examined, the ultimate action may be expressed by the scheme—

$$\begin{cases} X \mid H_2SO_4 \mid Na_2SO_4 \mid 2NaOH \\ XH_2 \mid SO_4Na_2 \mid SO_4Na_2 \mid H_2O+O, \end{cases}$$

where oxygen is evolved, X being some substance capable of combining with hydrogen (nitric acid, chromic anhydride, chlorine, &c.); and by the scheme—

$$\begin{cases} H_2SO_4 \mid Na_2SO_4 \mid 2NaOH \mid Y \\ H_2 \mid SO_4Na_2 \mid SO_4Na_2 \mid H_2O+OY, \end{cases}$$

where Y is some substance capable of uniting with oxygen (pyrogallol, ferrous oxide, copper, gold, &c.). For every gram-equivalent of silver thrown down in the voltameter, consequently, a gram-equivalent of both acid and alkali must disappear by mutual neutralisation during the passage of the current. This disappearance was verified quantitatively by titration in various experiments with no more lack of precision than the nature of the observation would lead one to anticipate.

We find that by combining two fluids, one alkaline and containing a powerful reducing agent, the other acid and containing an energetic oxidiser, with platinum plates immersed in each (*e.g.*, caustic soda solution of pyrogallol, and sulphuric acid solution of chromic anhydride), continuous currents of very considerable power may be obtained when the internal resistance is diminished sufficiently by using cells of considerable magnitude; *e.g.*, when made of the stoneware and inner porous vessels usually employed for Grove's cells, the porous vessel being cemented into the outer stoneware vessel (by paraffin wax or other unattacked material) in such a fashion as to divide it into three compartments separated one from the other by porous dividing walls; the acid and alkaline fluids being placed in the two outermost compartments, and the innermost one being filled with a solution of a neutral salt, *e.g.*, sodium sulphate. A large variety of analogous cells of more or less power can thus be formed by using different organic and inorganic reducing substances soluble in alkali, *e.g.*, ferrocyanides, hydrosulphites, opianates, &c.

"On Kreatinins. I. On the Kreatinin of Urine as distinguished from that obtained from Flesh Kreatin. II. On the Kreatinins derived from the Dehydration of Urinary Kreatin." By George Stillingfleet Johnson, M.R.C.S., F.C.S., F.I.C. Received May 5,—Read June 16, 1887.

Part I.

I was induced to undertake this investigation by a careful observation of the action of picric acid and potassium hydrate upon normal human urine at the boiling temperature.

The introduction of picric acid as a test for sugar in urine is due to my father, Dr. George Johnson, who accidentally discovered the production of a very dark colour on the addition of picric acid to a portion of saccharine urine which had been previously boiled with potassium hydrate. He made this observation in November, 1882. The dark colour was found to be due to reduction of potassium picrate to potassium picramate by glucose at the boiling temperature, and in presence of potassium hydrate. The reaction had been described by C. D. Braun nearly twenty years previously ("Ueber die Umwandlung der Pikrinsäure in Pikraminsaüre und über die Nachweisung des Traubenzuckers," 'Fresenius, Zeitschrift,' 1865), but it had not hitherto been applied to any practical purpose.

Having observed the extreme delicacy of the test, and the ease with which the reaction is effected, my father determined to introduce it to the notice of the medical profession, not merely as a trustworthy qualitative test of extreme delicacy, but also as a means of estimating quantitatively the actual amount of glucose in diabetic urines. In this latter object I did my best to assist him, and our united efforts resulted in the elaboration of a new, easy, and accurate method for determining sugar in urine, which is fully described in Dr. G. Johnson's volume of 'Medical Lectures and Essays,' and also in the last edition of Dr. Roberts' work on 'Urinary and Renal Diseases.'

Reaction of the Picric Acid and Potash Test in Normal Human Urine.

As soon as the picric acid and potash test was introduced to my notice, I applied it to normal human urine, and found that it gave indication of the presence therein, if not of glucose, at all events of a reducing substance capable of reducing yellow potassium picrate to red potassium picramate in presence of potassium hydrate at the boiling temperature.

After the elaboration of the test as a quantitative one, many specimens of urine from men in perfect health were examined by its means, and we never met with a single specimen which gave no reducing action, though, as would be expected, the reduction was more marked in concentrated specimens, and the smallest quantity of reducing substance was found in the urine of patients suffering from diabetes insipidus, in which cases the specific gravity of the secretion was very low.

In most urines from healthy individuals, of the average specific gravity (1·020), the amount of reduction exerted upon picric acid in presence of potassium hydrate at the boiling temperature corresponded with that which would have been produced by a solution of glucose containing 0·6 *grain per fluid ounce;* whilst the quantity of reducing agent indicated by the method of Fehling or Pavy is always slightly greater, the cupric oxide reduction of normal human urine expressed in terms of glucose averaging 0·75 *grain per fluid ounce.*

But is this normal reducing agent glucose? and if not, what is it? The object of the present research is to answer these questions.

Observations which negative the Hypothesis that the Reducing Agent of Normal Urine is Glucose.

Dr. Robert Kirk ('Lancet,' June 16, 1883) was, I believe, the first to publish the fact that healthy urine gives some indication of reducing picric acid in presence of potash *at the ordinary temperature*, whereas glucose effects no reduction until the temperature approaches the boiling point. This is an important distinction.

Again, after repeated experiments, I have never once succeeded in producing alcohol and carbon dioxide from normal human urine by the action of yeast, even when the solution was artificially concentrated before introducing the yeast, and the liquid containing the ferment was kept under the most favourable conditions of temperature, &c.

Repeated Observations have confirmed the Existence of Reducing Agents in Normal Human Urine.

It was probably owing to the impossibility of accounting otherwise for the reducing action which normal urine invariably exerts upon cupric oxide in boiling alkaline solutions, that physiological chemists were led to assert that dextrose is present in the urine of healthy men.

In a paper published in the Medico-Chirurgical Society's 'Transactions' (vol. 63, p. 222), Dr. F. W. Pavy, F.R.S., writes as follows:—"The reducing action before the addition of acetate of lead is due partly to uric acid and partly to the small amount of sugar naturally present in urine. It is doubtful if there is any other body worthy of consideration to exert any sensible reducing effect." And Dr. Pavy finds that one-fourth of the total reduction of copper oxide by normal urine is due to uric acid. This result Dr. Pavy arrived at by estimating the cupric oxide reduction of the urine before and after precipitation by lead acetate, which removes from solution the uric acid, but not the other reducing agent to which three-fourths of the total reducing effect must be ascribed, and which Dr. Pavy concludes is sugar. Brücke's views on this subject are too well known to need comment.

Amongst the known substances other than glucose, which reduce potassium picrate to picramate in boiling alkaline solutions, are—

(1.) Potassium ferrocyanide, a salt which is not likely to be found in urine, since it is devoid of medicinal properties, and is therefore not likely to be administered.

(2.) Sulphides of the alkali metals. I have shown ('Chemical News,' vol. 47, 1883, p. 87) that in boiling *dilute* solutions of potassium hydrate, albumen yields potassium tetrathionate, and not potassium sulphide, and as potassium tetrathionate does not reduce potassium picrate in boiling dilute solutions of potassium hydrate, and as a very dilute solution of potassium hydrate is employed in the quantitative estimation of glucose by picric acid, viz., 30 minims of the liquor potassæ of the British Pharmacopœia, diluted to 4 drachms, there is no possibility of reduction of picric acid by alkaline sulphides, formed by the action of potassium hydrate upon unoxidised sulphur compounds in the urine at the boiling temperature.

Kreatinin has been suggested by Dr. Oliver as the reducing agent of normal urine ('Bedside Urinary Testing'). The sample of kreatinin which he examined was sold by Messrs. Hopkin and Williams. I have examined a portion of this sample, and found that it was very deficient in reducing power. Allowing most liberally for its presence in the urine, only about one-twentieth of the total reduction of cupric oxide effected by that secretion in its normal condition could be accounted for by this substance.

The reducing action of kreatinin upon cupric oxide in boiling solutions containing caustic alkali has been long known to German chemists. Thus, Kühne draws attention to this fact at page 505 of his 'Lehrbuch der Physiologischen Chemie.'

As regards the *amount* of cupric oxide reduction usually attributed to kreatinin by German physiological chemists, I may quote a recent paper by Professor E. Salkowski, in the 'Centralblatt für die Medicinischen Wissenschaften,' March, 1886, in which the reduction due to uric acid and kreatinin combined is estimated as varying from one-fifth to one-sixth of the total cupric oxide reduction effected by normal urine, the remainder being attributed to other substances, and probably to compounds of glycuronic acid (Glykuronsäureverbindungen).

The Reducing Agent of Normal Urine is disintegrated by prolonged Boiling with Dilute Solution of Potassium Hydrate.

Whilst examining the reactions of the reducing agent in normal urine, I found that by prolonged boiling with dilute potassium hydrate solution, about three-fourths of the copper oxide reducing power of the urine is lost, the remaining one-fourth being due to survival of the uric acid. The urine which had been subjected to this treatment did not reduce picric acid at all, for uric acid has no reducing action upon picric acid in boiling alkaline solutions.

The reducing agent of normal urine is therefore disintegrated by prolonged ebullition with potassium hydrate. On comparing the behaviour of solutions of glucose in the same circumstances, I have since found that such solutions lose their reducing action upon cupric oxide with far greater rapidity than the reducing agent of normal urine.

In the 'British Medical Journal,' (March 17th, 1883), will be found a table of results of some determinations of the reducing action of normal urines upon picric acid and cupric oxide respectively, in which the effect of prolonged boiling with diluted potassium hydrate solution upon the reducing agent of normal urine is visible

I. Total indication by picric acid.	II. Total indication by ammonio-cupric method.	III. Indication by ammonio-cupric method, after boiling with potash.	IV. Difference between II and III, normal reducing agent.
gr. per 1 fluid oz.	gr. per 1 fluid oz.	gr. per 1 fluid oz.	gr. per 1 fluid oz.
(1.) 0·6	0·909	0·276	0·63
(2.) 0·5	0·607	0·09	0·517
(3.) 0·35	0·546	0·145	0·401
(4.) 0·8	1·245	0·437	0·808

at a glance. In this table all the reductions are expressed in terms of glucose.

Summing up all the evidence, there is no doubt that a reducing agent is present as a normal constituent of the urine of healthy men, and that it confers upon normal urine the property of reducing cupric oxide to the same extent as if it held in solution (on the average) 6 grains of glucose in every 10 fluid ounces of urine, or 1·34 grams *per litre.* But, considering the impossibility of causing alcoholic fermentation to take place in solutions of this reducing agent, and its property of reducing picric acid to some extent in presence of potassium hydrate at the ordinary temperature, its identity with glucose appears to be very doubtful. Further doubt is thrown upon this identity by the fact that if a solution of mercuric chloride be added to normal urine and afterwards potassium hydrate, the yellowish precipitate which forms, becomes grey in a few minutes by reduction at the ordinary temperature, whereas glucose does not effect reduction of mercuric oxide in presence of potassium hydrate without application of heat in less than one hour.

Endeavours to Isolate the Reducing Agent of Normal Urine.

Having examined qualitatively the reactions of the reducing agent of normal urine, and having found reasons to doubt its identity with diabetic sugar, I next proceeded to employ various precipitants with a view to its removal from the complex fluid in which it is dissolved.

Normal lead acetate, basic lead acetate, baryta-water, solution of ammonia, alcohol, were all employed in turn, but the filtrates in each case reduced picric acid as strongly as before precipitation, showing that the precipitants employed had failed to remove the reducing agent from solution.

Finally I succeeded in removing the whole of the normal reducing agent from urine by complete precipitation with strong aqueous solution of mercuric chloride.

Previous Researches on the Action of Mercuric Chloride upon Normal Human Urine.

It has long been known that an aqueous solution of mercuric chloride produces in *all* specimens of normal urine a flocculent precipitate, when added in sufficient excess. But the state of our knowledge as to the nature of the substance or substances thus precipitated is very unsatisfactory.

Dr. John Greene, of Birmingham, published in the 'British Medical Journal' (May 10th, 1879) a research in which he describes the isolation from this precipitate, produced by mercuric chloride in normal urine, of a white flocculent albumen-like substance, precipitable by lead acetate as well as by mercuric chloride, and refusing to dialyse through animal membranes. Dr. Greene analysed this substance, with the following results:—

Carbon	34·52
Hydrogen	5·71
Nitrogen	12·58
Oxygen	47·19
	100·00

The empirical formula indicated by these numbers is $C_{16}H_{30}N_5O_{16}$, which requires—

Carbon	35·03
Hydrogen	5·47
Nitrogen	12·77
Oxygen	46·73
	100·00

The main object of Dr. Greene's research was to prove that the precipitate produced by mercuric chloride in normal urine contains a large proportion of organic nitrogen, and that corrections might be made, in the determination of the urea in urine by the hypobromite method, by deducting so much nitrogen for every gram of mercury precipitate yielded by the original urine.

As, however, Dr. Greene rejected everything in the precipitate by mercuric chloride in normal urine which is not also precipitated by lead acetate, he of course missed the normal reducing agent altogether, and must have also considerably under-estimated the total nitrogen contained in the mercury precipitate, for the reducing agent of normal urine is highly nitrogenous, as will presently appear.

Maly ('Ann. Chem. Pharm.,' vol. 159, p. 279) describes a method for obtaining kreatinin hydrochloride from the urine of man or the horse by precipitation with mercuric chloride. His method consists in concentrating the urine to one-third of its original bulk, then precipitating by lead acetate and filtering, whereby the uric acid and Greene's substance are removed. The filtrate is freed from lead by sodium carbonate or sulphuretted hydrogen, neutralised after a second filtration by acetic acid or sodium carbonate, and then precipitated by mercuric chloride. The precipitate is washed and then decomposed by hydrogen sulphide under water, and the filtrate is decolourised by animal charcoal and evaporated. The residue, on being re-crystallised once or twice from alcohol, yields pure kreatinin hydrochloride in hard shining prisms (*vide* Watts's 'Dictionary,' Suppl. 2, p. 393).

In spite of these researches, mercuric chloride has recently been again recommended as a precipitant for *albumen* in urine, and the precipitate produced by this reagent in normal urine has been attributed to *urea*. Enough, however, has been said to prove that the precipitate produced by mercuric chloride in normal urine is of a very complex nature.

Reaction of Normal Human Urine with Strong Solution of Mercuric Chloride.—The Author's Researches.

In all my experiments with a view to separate the reducing agent from normal urine, I have employed a solution of mercuric chloride in water, saturated at the temperature of the laboratory (16° C.).

In my first experiments I filtered the urine immediately after adding to it one-fourth of its volume of the cold saturated mercuric chloride solution, and always found that the filtrates still contained the reducing substance in solution.

I soon observed, however, that after separating the flocculent amorphous precipitate first produced by mercuric chloride, the filtrate did not long remain clear, but after about half an hour a second precipitate began to form, having a granular appearance, and continuing to separate out for many days. On filtering from time to time, and examining the reducing action of the filtrate, it was found that the reducing power progressively diminished as more and more of the mercury salt separated out from the solution. In short it soon became evident that the reducing agent of normal urine may be completely removed from that liquid by precipitation with mercuric chloride.

Although the complete precipitation of the reducing substance is very slow if mercuric chloride be added alone, if we add to fresh unconcentrated urine one-twentieth of its volume of a cold saturated solution of sodium acetate, then one-fourth of its volume of cold saturated solution of mercuric chloride, and filter immediately, the filtrate deposits the whole of the normal reducing agent as mercury salt in about forty-eight hours. The precipitation is known to be complete when the filtrate from the second mercury precipitate remains clear, even on the addition of more solution of sodium acetate and mercuric chloride. It will then be found that the permanently clear filtrate is without reducing action upon both potassium picrate and cupric oxide in boiling alkaline solutions.

The first precipitate produced by mercuric chloride in normal urine is amorphous and flocculent, and has some resemblance to coagulated albumen. Finding that it contained no reducing agent, I have deferred the study of this precipitate for the present, but a cursory examination of it showed that it contained uric acid, probably as mercuric urate, besides the substance described by Dr. Greene, which is precipitated by lead acetate. When decomposed by hydrogen sulphide under water, the filtrate from mercuric sulphide is acid in reaction, contains much chlorine, but no phosphoric acid, and yields a gummy mass on evaporation. It exerts no reducing action either upon cupric oxide or potassium picrate in boiling alkaline solutions.

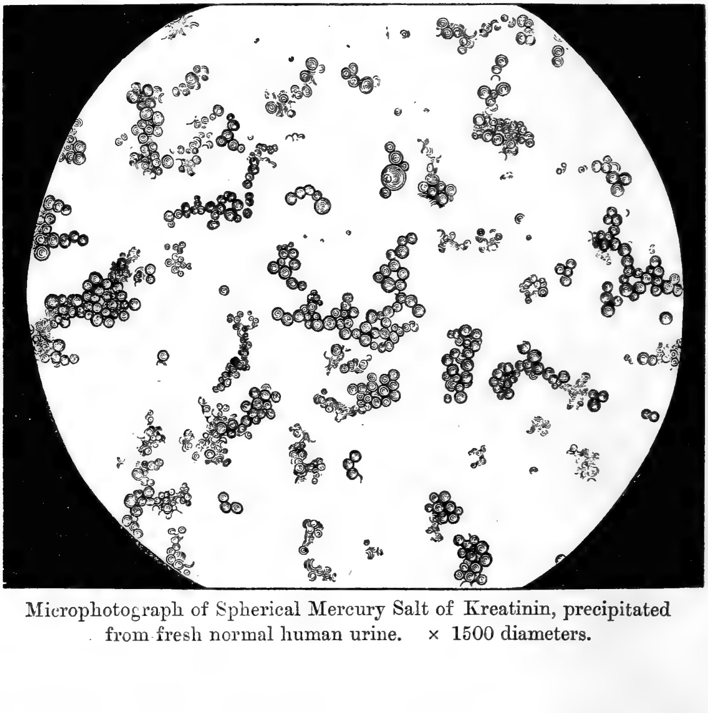
Satisfied, therefore, that the granular precipitate which gradually separated out from the filtrate from the above amorphous substance contains the whole of the normal reducing agent of urine, I confined myself to the examination of that compound.

Physical Properties of the Mercury Salt of the Reducing Base of Urine.

I have described this precipitate as granular, and it certainly appears upon superficial observation to be a crystalline substance, as indeed might be expected from its gradual formation, but microscopic examination reveals some curious facts in connexion with it.

Examined under a quarter-inch object glass, the substance is at once seen to be perfectly homogeneous, and often has the appearance of minute crystals united together in stellate groups; but under a one-sixteenth inch object glass these stellate groups are seen to be composed of a number of very minute and perfectly spherical masses. I have watched the growth of these spherules from a minute point to a little globe resembling an oil-globule. The

FIG. 1.



Microphotograph of Spherical Mercury Salt of Kreatinin, precipitated from fresh normal human urine. × 1500 diameters.

mode of formation of this compound appeared to me so remarkable that I have prevailed upon my friend Mr. Herbert Jackson to execute some microphotographs of the spherical mercury salt of the reducing base of urine.

This spherical mercury salt is very sparingly soluble in cold water. When recently precipitated it dissolves with great ease in hydrochloric acid, but is insoluble or nearly so in acetic acid.

Chemical Reactions and Decompositions of the Spherical Mercury Salt.

Aqueous solution of ammonia does not blacken the spherical compound, but if the salt be thrown into boiling water, the sediment which remains undissolved is immediately blackened by ammonia-water, so that a portion of the mercury in this compound is reduced to the mercurous condition by contact with water at 100° C. Therefore, the spherical mercury salt must be washed with cold water, and must be dried *in vacuo* over sulphuric acid, in order to avoid this decomposition.

Moistened with solution of potassium hydrate at the ordinary temperature, the compound gradually becomes black by reduction, a compound ammonia escaping at the same time in abundance.

Suspended in cold water and treated with hydrogen sulphide, the compound first becomes yellow and finally black (mercuric mercury), and if the stream of gas be continued, nothing remains undissolved but mercuric sulphide. The solution is acid in reaction, and reduces both potassium picrate and cupric oxide in presence of potassium hydrate at the boiling temperature (hydrochloride of the reducing base).

Having convinced myself that the spherical compound is a definite substance, I proceeded to ascertain the weight of it which is yielded by a known volume of urine, and to subject the salt to analysis with a view to ascertain its composition.

Whilst determining the quantity of the spherical compound obtained by precipitation from known volumes of urine, I was of course accumulating a store of material from which to prepare large quantities of the reducing base itself for subsequent examination. The method which I adopted was as follows :—

A quantity of fresh human urine is examined for albumen and sugar, and, if found normal, its volume is exactly noted. The reaction to litmus is then ascertained, and also the amount of reduction of picric acid (or cupric oxide) which it is capable of effecting. Next the specific gravity of the specimen is observed, after which I add to it one-twentieth of its volume of a cold saturated solution of sodium acetate, and from one-third to one-fourth of its volume of cold saturated mercuric chloride solution. There is no need to concentrate the urine by evaporation. The mixture is now immediately filtered to separate the flocculent amorphous mercury precipitate, and the filtrate is received into a large glass vessel capable of holding about 60 litres. It is better not to operate with more than a litre of urine at a time, because if a larger volume be taken there is risk of formation of spherical mercury salt before the filtration is complete, and of its retention upon the filter. But the mercuric chloride acts so effectually in preventing putrefaction that sample after sample may be treated as above directed, and filtered into the same receptacle, in which the spherical compound accumulates and awaits the convenience of the operator. When a sufficient quantity has accumulated, it is thrown upon a filter, washed with cold distilled water till the washings are only slightly troubled by silver nitrate, and the washed compound is then dried over sulphuric acid *in vacuo* and weighed.

The following tables will give an idea of the scale upon which I have operated, and will also show how the weight of mercury salt obtained is proportional to the amount of reduction exerted by the specimens examined:—

Table I.

No. of men contributing.	Volume of urine.	Specific gravity at 16° C.	Picric acid reduction expressed as glucose.	Reaction to litmus.
	c.c.		gr. per fluid oz.	
4	5,600	1·021	0·6	Feebly acid
3	1,600	1·023	0·65	Neutral
3	900	1·023	0·87	Feebly acid
4	1,200	1·018	0·75	Feebly acid
2	1,100	1·023	0·75	Acid
2	1,000	1·027	0·88	Feebly acid
2	790	1·025	0·87	Acid
3	1,100	1·023	0·87	Acid
3	750	1·031	1·00	Acid
1	900	1·031	1·00	Acid
1	1,000	1·026	0·85	Acid
1	2,000	1·026	0·85	Acid
1	1,300	1·027	0·90	Acid
Many........	15,400	1·017	0·62	Acid

Table II.

Volume of urine in c.c.	Specific gravity at 16° C.	Picric acid reduced (as glucose).	Reaction to litmus.	Vol. of sat. $HgCl_2$ added.	Vol. of sat. NaAg added.
c.c. 1,690	1·017	gr. p. fluid oz. 0·7	Acid	c.c. 430	c.c. 60
1,350	1·025	0·85	Acid	400	50
2,180	1·022	0·75	Acid	600	100
1,755	1·025	1·2	F. acid	450	60
1,150	1·022	0·9	Acid	320	60
1,560	1·025	0·9	Acid	400	50
1,730	1·026	0·85	Acid	450	50
1,430	1·022	1·0	Acid	375	45
1,910	1·020	0·9	Acid	500	70
1,420	1·016	0·7	F. acid	410	50
1,000	1·020	0·85	Acid	300	40
1,380	1·020	0·8	Acid	350	50
1,000	1·025	1·1	Acid	300	40
1,715	1·025	1·0	Acid	450	70
1,760	1·025	1·0	Acid	500	80
1,410	1·018	0·75	Acid	400	60
1,785	1·024	0·7	Acid	500	70
1,000	1·022	0·8	Acid	300	50
1,140	1·019	1·0	Acid	310	50
1,535	1·022	1·0	F. acid	500	70
1,495	1·022	0·85	Acid	500	70
1,500	1·026	0·75	Acid	500	70
1,410	1·025	1·0	Acid	500	70
1,950	1·020	0·7	Acid	800	100
770	1·025	1·0	Acid	300	40
2,000	1·019	0·9	Acid	800	100
1,600	1·020	0·8	Acid	700	100
40,625	..	..	..	12,345	1,725

The total volume operated on (Table I) was thus 34,640 c.c. The mean specific gravity was 1·020, and the mean reduction of picric acid (expressed in terms of glucose) was equivalent to 0·67 grain of glucose per fluid ounce.

The spherical mercury salt obtained from the above urine, after being washed with cold water and dried *in vacuo* over sulphuric acid, weighed 198 grams. This is at the rate of 5·7 grams of the spherical compound to each litre of urine.

The mean specific gravity at 16° C. of the above samples (Table II) of normal human urine is 1·022, and the mean reduction of picric acid is equivalent to that which would be effected by a solution containing 0·86 grain of glucose per fluid ounce.

The washed and dried spherical mercury salt from the above 40,625 c.c. of normal urine weighed 293·13 grams, which is equivalent to 7·19 grams per 1 litre of urine.

It is apparent from the above tables that the weight of mercury salt obtained from an equal volume of urine is proportional to the amount of reduction effected by the secretion, and that both have a tendency to increase with the specific gravity of the latter.

The following observations made upon my own urine during the past winter, illustrate some interesting points in connexion with the precipitation of normal urine by mercuric chloride:—

My weight at the time of the experiment was 70·08 kilos. I was in good health, in active work both mental and bodily, and consuming an ordinary mixed diet. All the samples had the normal acid reaction to litmus, and were free from albumen and sugar.

The total urine of each twenty-four hours, having been measured, was mixed with a cold saturated solution of sodic acetate, and then precipitated with one-fourth of its volume of cold saturated solution of mercuric chloride. Both first and second precipitates were collected on filters previously counterpoised, washed with cold water, dried *in vacuo* over H_2SO_4, and weighed.

The total volume of urine during the six days amounted to 8930 c.c.

Average per 24 hours	....	1488 c.c.
Maximum in ,,	....	1790 ,,
Minimum in ,,	....	1230 ,,

	Volume of urine.	Weight of 1st Hg. ppt.	Weight of 2nd Hg. ppt.
	c c.	grams.	grams.
December 29 to 30.......	1790	5·355	9·076
„ 30 to 31.......	1360	9·238	9·930
„ 31 to January 1	1575	7·300	8·758
January 1 to 2...........	1230	6·715	8·176
„ 2 to 3...........	1700	4·985	8·932
„ 4 to 5...........	1275	7·735	7·955
Six days..........	8930	41·328	52·827

Hence it appears that the mercury salt of the reducing base is far more constant in quantity than the amorphous mercury-compounds first precipitated. Also in my case the average weight of *kreatinin* passed in twenty-four hours amounted to 1·77 grams, or 25/1000000 of the body-weight.

Analysis of the Spherical Mercury Salt from Urine.

As precipitated from urine, the spherical mercury salt has a fawn colour, due to enclosure of urinary colouring-matter. It was therefore deemed advisable to re-precipitate the compound in order to obtain it pure for analysis. With this object in view, the thoroughly washed compound is suspended in water and subjected to a stream of hydrogen sulphide until completely decomposed. The deep yellow filtrate from the mercuric sulphide is digested for some days with purified animal charcoal (which must be quite free from lime salts), and is then mixed with solutions of sodium acetate and mercuric chloride. A very pure compound is obtained, if the decolourised solution is largely diluted before adding the mercuric chloride, in which case any precipitate which forms at once may be separated by filtration, after which the filtrate gradually deposits the pure spherical compound in a colourless condition. Having been washed with cold water and dried *in vacuo* over sulphuric acid, the pure mercury salt was analysed with the following results:—

By Combustion with Copper Oxide.

(*a.*) 1·1920 gram of the Hg salt gave 0·1500 gram of H_2O.
and 0·3755 „ CO_2.

Equivalent to 0·01666 gram hydrogen.
and 0·10241 „ carbon.

Hence Hg salt contains $\begin{cases} \text{hydrogen, 1·39 per cent.} \\ \text{carbon, 8·592 „} \end{cases}$

(*b.*) 1·3302 grams of Hg salt gave 0·1522 gram H_2O.
and 0·4187 „ CO_2.
Equivalent to 0·016811 gram of hydrogen.
and 0·114191 „ carbon.
Hence Hg salt contains { hydrogen, 1·26 per cent. carbon, 8·584 „

The *nitrogen* was estimated in this compound and in all the analyses recorded in this research by a modification of Dumas' method. The substance to be analysed is introduced in a boat behind the copper oxide, instead of being mixed with the latter. By this means the copper oxide can be ignited strongly in a stream of carbon dioxide, before adjusting the measuring tube for the nitrogen. Residual carbon left in the boat at the end of the combustion is burnt off by oxygen generated by heating potassium chlorate in a second boat placed behind that which contains the substance to be analysed.

The following results were obtained:—

(*a.*) 0·7728 gram of Hg salt gave 44·83 c.c. of nitrogen, measured at 0° C. and 760 mm. P., equivalent to 0·0560385 gram of N.
Hence the Hg salt contains 7·25 per cent. nitrogen.

(*b.*) 0·6228 gram Hg salt gave 36·29 c.c. of nitrogen, measured at 0° C. and 760 mm., equivalent to 0·0453625 gram of N.
Hence the Hg salt contains 7·28 per cent. of N.

The *mercury* was determined in two ways: 1st, by dissolving the Hg salt in HCl and precipitating the solution by H_2S; and 2ndly, by suspending the Hg salt in water and decomposing by H_2S. The close concordance between the results proves how very completely the mercury salt may be decomposed by hydrogen sulphide under water.

(*a.*) 1·0882 gram of Hg salt dissolved in boiling HCl gave by precipitation with H_2S 0·7868 gram of HgS, equivalent to 0·67827 gram of metallic mercury.
Hence the salt contains 62·33 per cent. of Hg.

(*b.*) 0·8232 gram of Hg salt, suspended in water and decomposed by H_2S, gave 0·5996 gram HgS, equivalent to 0·5169 gram of mercury.
Hence the salt contains 62·79 per cent. of Hg.

The *chlorine* was determined (1) by decomposing the Hg salt under water with H_2S and precipitating the filtrate with $AgNO_3$; (2) by igniting the Hg salt with pure sodium carbonate, dissolving in water, acidulating with HNO_3, and precipitating with $AgNO_3$. The following results were obtained:—

(*a.*) 0·8070 gram of the Hg salt gave 0·5226 gram of silver chloride, equivalent to 0·1285 gram of chlorine, or 15·92 per cent. of Cl.

(*b.*) 0·8232 gram of Hg salt gave 0·5356 gram of AgCl, equivalent to 0·1325 gram of chlorine.

The salt therefore contains 16·09 per cent. of chlorine.

The empirical formula derived from the results of these analyses is $C_{16}H_{28}N_{12}O_6Hg_7Cl_{10}$.

The simplest rational formula is $4(C_4H_5HgN_3O{\cdot}HCl){\cdot}3HgCl_2{\cdot}2H_2O$.

	Theory.	Found.	
Carbon	8·575	8·588	8·592, 8·584
Hydrogen	1·251	1·325	1·39, 1·26
Nitrogen	7·503	7·275	7·26, 7·28
Oxygen	4·288	4·247	(By difference).
Mercury	62·528	62·560	62·79, 62·33
Chlorine	15·855	16·005	16·09, 15·92
	100·000	100·000	

The apparent molecular weight of the mercury salt (confirmed subsequently) is 2239, and it contains 20·19 per cent. of the base $C_4H_7N_3O$.

The *crude* spherical mercury salt, as obtained directly from urine, yielded on combustion—

Carbon, 8·46 per cent.
Hydrogen, 1·17 „

Preparation and Properties of the Hydrochloride of the Reducing Base of Urine.

In order to prepare the hydrochloride of the reducing base of urine, the crude spherical mercury salt is suspended in water and completely decomposed by hydrogen sulphide. The deep yellow acid filtrate from the resulting mercuric sulphide is then decolourised as completely as possible by very pure animal charcoal, and the nearly colourless filtrate from the charcoal is left to evaporate spontaneously *in vacuo* over sulphuric acid. Large well-formed prisms with pyramidal apices separate out, which only need washing with a little strong alcohol to separate a small quantity of dark tarry matter, which usually adheres to the bottom of the vessel. They need no re-crystallisation.

This ready crystallisation of the hydrochloride of the reducing base is doubtless due to the great purity of the mercury salt which is attained by separating the amorphous compounds first precipitated from the urine by mercuric chloride by immediate filtration.

I have worked Maly's process (*vide supra*) exactly as he directs, and found that, notwithstanding the preliminary precipitation of the concentrated urine with lead acetate, a tarry mass is obtained after the H_2S treatment of the mercury precipitate, which stands much in need of the re-crystallisation from alcohol recommended by Maly, before it will yield distinct crystals. If, however, the precipitate first formed by mercuric chloride in Maly's process be separated by immediate filtration, and only the precipitate produced in the filtrate on standing be decomposed by H_2S, all goes smoothly and well. The preliminary precipitation by lead acetate and the artificial concentration of the urine are quite useless.

The crystals of the hydrochloride of the reducing base of urine, obtained as above, are quite permanent in the air, and their weight is unaltered by exposure to a temperature of 100° C.

0·3708 gram of the crystals (not re-crystallised) gave 0·3512 gram of silver chloride, equivalent to 0·086881 gram of chlorine, or 23·43 per cent. Cl.

The formula $C_4H_7N_3O{\cdot}HCl$ requires 23·74 per cent. Cl.

The crystals are excessively soluble in water, and also to a great extent in alcohol.

Mixed in aqueous solution with mercuric chloride, there is no precipitate, but on adding solution of sodium acetate, the spherical mercury salt separates out.

Mixed in aqueous solution with mercuric chloride and potassium hydrate, there is first a whitish precipitate, which dissolves in excess of the potassium hydrate, but the solution becomes turbid again after a few seconds with formation of a yellowish precipitate, which immediately afterwards becomes black by spontaneous reduction.

Mixed in aqueous solution with Nessler reagent, a bright yellow precipitate separates out, which rapidly becomes black by spontaneous reduction, a compound ammonia being evolved at the same time.

When an alcoholic solution of the hydrochloride of the reducing base of urine is mixed with excess of an alcoholic solution of platinic chloride, a yellow crystalline platinum salt is immediately precipitated. These crystals appear dendritic under the microscope. They are anhydrous, and permanent in the air.

When dissolved in water, and spontaneously evaporated, a fine orange-coloured prismatic platinum salt is formed, which loses water at 100° C., becoming opaque and of a lemon-yellow tint. The same orange-coloured prisms are obtained by spontaneous evaporation of a mixture of the hydrochloride of the reducing base with platinic chloride in aqueous solution.

The determination of the *water of crystallisation* in these orange-coloured prisms shows that they contain (on the average) 5·32 per cent. of H_2O.

Thus 0·4960 gram of Pt salt, crystallised from aqueous solution and air-dried, lost 0·0260 gram at 100° C., equivalent to 5·24 per cent.

5·1396 grams of Pt salt, prepared as above, lost 0·2706 gram at 100° C., corresponding with 5·27 per cent.

0·8332 gram of Pt salt, re-crystallised from aqueous solution and air-dried, lost 0·0454 gram at 100° C., corresponding with 5·44 per cent.

The *anhydrous platinum salt of the reducing base* was submitted to analysis with the following results:—

0·4866 gram of platinum salt gave 0·1188 gram of water, equivalent to 0·0132 gram of hydrogen, or 2·71 per cent. of H, and

0·2680 gram of CO_2, equivalent to 0·0736 gram of carbon, or 15·12 per cent. carbon, and left

0·1514 gram of platinum, corresponding with 31·11 per cent. of Pt.

0·4172 gram of Pt salt gave 0·1060 gram of water, equivalent to 0·01177 gram of hydrogen, or 2·82 per cent., and

0·2336 gram of CO_2, equivalent to 0·06371 gram of carbon, or 15·27 per cent. of C.

The *chlorine* was determined by mixing the platinum salt with pure caustic lime, igniting, dissolving in diluted nitric acid, filtering from the spongy platinum (which is weighed), and precipitating the chlorine in the filtrate as AgCl.

0·5535 gram of Pt salt gave 0·7460 gram of AgCl, equivalent to 0·1845 gram of chlorine, corresponding with 33·333 per cent. Cl, and left

0·1715 gram of platinum, corresponding with 30·984 per cent. of Pt.

0·5430 gram of Pt salt gave 58 c.c. of nitrogen, measured at 0° C. and 760 mm., equivalent to 0·072 gram of nitrogen, or 13·25 per cent.

0·3060 gram of platinum salt gave 32·85 c.c. of nitrogen, measured at 0° C. and 760 mm., equivalent to 0·04106 gram of nitrogen, or 13·41 per cent.

These results agree well with the formula $2\{C_4H_7N_3O.HCl\}.PtCl_4$.

	Required.		Found.	
Carbon.....	15·047	....	15·190	15·12 15·27
Hydrogen...	2·507	....	2·760	2·71 2·82
Nitrogen....	13·166	....	13·330	13·25 13·41

	Required.		Found.	
Oxygen	5·017	....	4·340 (By difference)	
Platinum ...	30·878	....	31·047	{ 30·984, 31·11
Chlorine	33·385	....	33·333	
	100·000		100·000	

The above being the formula of the anhydrous platinum salt of the reducing base of urine, it appears that the formula of the orange-coloured prisms obtained by spontaneous evaporation of the aqueous solution, is $2(C_4H_7N_3O.HCl).PtCl_4.2H_2O$. This formula requires 5·34 per cent. of water of crystallisation, whilst the mean of the three determinations given above indicates the presence of 5·32 per cent.

Preparation and Properties of the Reducing Base—Kreatinin of Urine.

The reducing base or kreatinin of urine is obtained in the free state by treating the concentrated aqueous solution of its hydrochloride (prepared as above from the spherical mercury salt) with excess of very pure hydrated lead oxide at the ordinary temperature.

The crystallised hydrochloride should be dissolved in about fifteen times its weight of cold water, and an excess of perfectly pure recently precipitated lead hydrate added to the solution, which should be kept stirred constantly for about twenty minutes at the ordinary temperature. After the liquid has acquired a strongly alkaline reaction, it will be found to filter clear through paper. The filtrate is perfectly free from chlorine and colouring matter, and yields the urinary kreatinin on spontaneous evaporation *in vacuo* over sulphuric acid in the crystalline condition. It is better not to dilute the filtrate by adding the washings from the lead oxychloride to it, but these may be evaporated separately by heat.*

The same sample of the spherical mercury salt of the urinary kreatinin may be made to yield three different substances according to the treatment adopted in the separation of the kreatinin from it. The following details of an experiment conducted upon the large scale will best exemplify this statement.

260 grams of the spherical mercury salt of the urinary kreatinin

* The lead hydrate employed for liberating the reducing kreatinin of urine from its hydrochloride must be free from *basic lead nitrate.* When this compound is present, the alkaline filtrate, though free from chlorine, is found to hold in solution both lead and the radicle of nitric acid; moreover, the lead cannot be removed from solution by animal charcoal, even at the boiling temperature, but must be separated by hydrogen sulphide, after which the filtrate from the lead sulphide exhibits an acid reaction, and deposits crystals of kreatinin nitrate on evaporation.

I have found it best to employ lead hydrate precipitated from the *acetate* by ammonia.

was decomposed by hydrogen sulphide in presence of water, special care being taken to avoid too great a rise of temperature by constant agitation in a closed bottle during the action of the gas upon the precipitate.

After the action was complete the solution was set aside for three days in a tall glass cylinder, to allow the mercuric sulphide to settle. As much as possible of the clear and supernatant liquor was then decanted off from the precipitate, and set to evaporate *in vacuo* over sulphuric acid. Hard anhydrous crystals of kreatinin hydrochloride resulted. After being washed with a little cold alcohol, these crystals were dissolved in about fifteen times their weight of cold water, the solution was well stirred with pure lead hydrate, and filtered. The filtrate when evaporated *in vacuo* over sulphuric acid, deposited needle-shaped crystals of a base for which I propose the name of *efflorescent kreatinin*, whose properties and composition will be described shortly. This efflorescent kreatinin, being obtained from the spherical mercury salt of urine as far as possible without any application of heat, I believe to be the true natural kreatinin of urine.

The washings from the lead oxychloride, &c., after the above treatment were evaporated at 60° C. on a copper plate kept hot by steam. A number of *anhydrous* square tables were obtained on cooling; which when dissolved in cold water yielded an alkaline solution which deposited efflorescent kreatinin on evaporation *in vacuo* over sulphuric acid.

The third substance is obtained as follows:—

The washings from the mercuric sulphide in the above experiment were concentrated over steam. The concentrated liquor was diluted, decolourised by animal charcoal, and filtered. The filtrate was again concentrated over steam, and finally evaporated *in vacuo* over sulphuric acid. The resulting crystals of kreatinin hydrochloride did not differ in outward appearance from those obtained from the solution to which no heat had been applied; but, when decomposed by pure lead hydrate after solution in fifteen parts of cold water—in short, under conditions as nearly as possible identical with those present in the case of the hydrochloride crystallised from cold aqueous solution—instead of efflorescent kreatinin, anhydrous square or oblong tabular crystals are obtained, similar in form to the tabular crystals resulting from the evaporation of an aqueous solution of the efflorescent kreatinin of urine at 60° C., but more transparent than the latter, and differing from them in that their cold aqueous solution deposits anhydrous tabular crystals again on spontaneous evaporation, instead of efflorescent crystals.

This tabular anhydrous kreatinin, which re-crystallises unaltered from its cold aqueous solution on evaporation *in vacuo* over sulphuric acid, I shall speak of in this paper as *tabular kreatinin α of urine,* whilst the tabular crystals which yield efflorescent kreatinin under similar treatment will be alluded to as *tabular kreatinin β of urine,* the term *efflorescent kreatinin* being reserved for the natural base.

It will be apparent from the following details of experiments that these three varieties of urinary kreatinin are convertible into one another at the will of the operator.

Suppose we start with *efflorescent kreatinin.* This substance crystallises in splendid transparent square prisms, often upwards of an inch in length, which, however, begin to lose their transparency when freed from extraneous moisture within half an hour in summer weather, and very rapidly even in winter upon mere exposure to common air. After complete efflorescence the crystals retain their original form without any tendency to crumble, and then resemble porcelain in appearance.

Now if these effloresced crystals are re-dissolved in the smallest possible quantity of *cold water,* the solution deposits efflorescent kreatinin again on evaporation *in vacuo* over sulphuric acid.

But if the effloresced kreatinin be dissolved in water at 60° C., and the evaporation be continued at that temperature till the crystallising point is reached, the crystals deposited on cooling are *tabular kreatinin β of urine,* anhydrous crystals, which yield efflorescent kreatinin on spontaneous evaporation of their cold aqueous solution over sulphuric acid. And if the effloresced kreatinin be dissolved in water at 100° C., the solution, evaporated *in vacuo* over sulphuric acid (in case the water present is in sufficient quantity to hold all the kreatinin in solution at the ordinary temperature), deposits crystals of *tabular kreatinin α of urine,* which re-crystallises unchanged from cold aqueous solution.

Again, if tabular kreatinin α of urine be dissolved in a larger volume of water at 60° C. than is necessary to hold it in solution at the ordinary temperature, and if the solution be kept at 60° C. for one hour, then allowed to cool and evaporated *in vacuo* over sulphuric acid, efflorescent kreatinin crystallises out.

Analysis of Urinary Kreatinin.

The determination of the water of crystallisation in the efflorescent kreatinin is not easy, on account of the rapidity of the efflorescence when the crystals are quite free from extraneous moisture.

The results of analysis, however, prove that the composition of the crystals is represented by the formula $C_4H_7N_3O.2H_2O$.

The mean of the two determinations gives 23·87 per cent. H_2O, whilst the formula $C_4H_7N_3O.2H_2O$ requires 24·17 per cent.

Weight of efflorescent kreatinin taken.	Loss of weight at 100° C.	Loss per cent.	Required for $C_4H_7N_3O.2H_2O$.
gram.			
0·3195	0·0770	24·10	24·17
0·4230	0·1000	23·64	24·17

When a weighed quantity of the effloresced kreatinin of urine is dissolved in boiling water, and the solution evaporated to dryness *in vacuo* over sulphuric acid, the weight of tabular kreatinin α obtained is identical with that of the effloresced kreatinin taken.

This fact indicates that the percentage composition of the two bodies is identical; a view which is fully supported by the results of their ultimate analysis.

Description of substance.	Weight of substance in grams.	Weight of hydrogen found.	Weight of carbon found.	H in 100 parts.	C in 100 parts.
Tabular kreatinin α of urine........	0·3170	0·02015	..	6·35	..
Tabular kreatinin α of urine........	0·2080	..	0·088036	..	42·32
Effloresced kreatinin of urine....	0·3968	0·0260	0·16541	6·55	41·68

Description of substance.	Weight of substance in grams.	Vol. of nitrogen at 0° C. and 760 mm.	Weight of nitrogen in grams.	N in 100 parts.
		c.c.		
Tabular kreatinin α of urine	0·1754	52·64	0·066032	37·64
Effloresced kreatinin of urine	0·1540	45·73	0·05736	37·24

The following table shows that these results agree well with the formula $C_4H_7N_3O$:—

	Found in tabular K.		Found in effloresced K.		Required for $C_4H_7N_3O$.
Carbon.........	42·32	....	41·68	....	42·478
Hydrogen.......	6·35	....	6·55	....	6·194
Nitrogen........	37·64	....	37·24	....	37·168
Oxygen (difference)	13·69	.... (difference)	14·53	....	14·160
	100·00		100·00		100·00

Supported by the analyses of mercury and platinum salts, and of the hydrochloride, these analyses leave no doubt that the empirical formula of the reducing base of urine is $C_4H_7N_3O$.

Gold Salt of the Kreatinin of Urine.

Both the tabular kreatinin α of urine and the efflorescent kreatinin of urine, when dissolved in dilute hydrochloric acid and mixed with a concentrated aqueous solution of auric chloride, yield large thin plates of kreatinin-auric chloride. This salt is remarkably stable. The crystals are permanent in the air, possess a brilliant golden-yellow lustre, and undergo no change at 100° C., except a temporary darkening of colour, which disappears again on cooling.

The analysis of this salt confirms the view that the molecular weight of the reducing kreatinin of urine is 113.

Analysis of Gold Salt of the Efflorescent Kreatinin of Urine.

Weight of kreatinin-auric chloride taken in grams.	Weight of gold found.	Weight of chlorine found.	Weight of gold in 100 parts.	Weight of Cl in 100 parts.
0·2670	0·1165	0·08324	43·63	31·17

These results agree with the formula $C_4H_7N_3O.HCl.AuCl_3$.

	Found.		Theory.
Au	43·63	......	43·43
Cl	31·17	......	31·37

Determination of the Solubility in Water of the Efflorescent Kreatinin and of Tabular Kreatinin α of Urine.

Some solution of efflorescent kreatinin of urine, which was depositing crystals over sulphuric acid at 14° C., was poured off from these crystals into a tared dish, weighed, and evaporated to dryness *in vacuo* over sulphuric acid.

The residue was further dried at 100° C. and then weighed.

4·934 grams of the aqueous solution saturated at 14° C. left a residue (effloresced K.) which dried at 100° C. weighed 0·329 gram.

Therefore 4·605 grams of water at 14° C. held in solution 0·329 gram of effloresced kreatinin, corresponding with 0·4338 gram of efflorescent kreatinin ($C_4H_7N_3O.2H_2O$).

Hence the efflorescent kreatinin ($C_4H_7N_3O.2H_2O$) requires 10·6 times its weight of water at 14° C. for complete solution.

And the effloresced kreatinin ($C_4H_7N_3O$) requires 14 times its *weight of water for complete solution at* 14° C.

A very pure specimen of the *tabular kreatinin* α of urine was dissolved to saturation in water kept at 40° C. The saturated solution was then kept at 17° C. for 12 hours. Crystals separated out.

3·9828 grams of the resulting solution saturated at 17° C. was evaporated to dryness *in vacuo* over sulphuric acid. The residue weighed 0·3376 gram. Therefore 3·6452 grams of water at 17° C. held in solution 0·3376 gram of the tabular kreatinin, or 1 gram of tabular kreatinin α of urine is soluble in 10·78 grams of water at 17° C.

Allowing for the different temperatures, it appears, therefore, that the solubility in water of the *efflorescent kreatinin* ($C_4H_7N_3O.2H_2O$) *before efflorescence* is practically identical with that of the *tabular kreatinin* α of urine ($C_4H_7N_3O$).

The solubility of kreatinin prepared from the kreatin of flesh is given by Liebig as 1 part in 11·5 parts of water at 16° C.

Solubility of Tabular Kreatinin α of Urine in Alcohol.

A fine specimen of the *tabular kreatinin* α was digested with alcohol of specific gravity 0·795 at its boiling point in a continuous extraction apparatus for some hours. The hot alcoholic solution deposited a number of dendritic crystals on cooling. The solution was left at the ordinary temperature for 20 hours in a well-corked bottle. 10 c.c. of the solution, whose temperature was 17° C., was then drawn off in a pipette and evaporated to dryness over steam. The residue weighed 0·0220 gram. A second 10 c.c. left a residue weighing 0·0216 gram. 7·95 grams of absolute alcohol at 17° C. therefore dissolved 0·022 gram of the tabular kreatinin.

Therefore 1 part by weight of the kreatinin dissolves in 362 parts by weight of absolute alcohol at 17° C.

The solubility of kreatinin prepared from the kreatin of flesh is given by Liebig as 1 part in 102 parts of absolute alcohol at 16° C.

Determination of the Weight of Cupric Oxide reduced by Efflorescent Kreatinin of Urine and by Tabular Kreatinin α of Urine in Boiling Alkaline Solutions.

My object in making these determinations being to ascertain how much of the reducing action of normal urine is to be ascribed to the kreatinin which is present in that secretion, I employed the same method therein as in estimating the cupric oxide reduction effected by the urine itself, viz., Pavy's ammoniacal cupric method.

11·2 c.c. of a solution of the *efflorescent kreatinin of urine*, containing

0·1346 gram of the anhydrous base ($C_4H_7N_2O$) in 60 c.c. were required for the complete reduction of 40 c.c. of Pavy's ammoniacal cupric solution.

Now 40 c.c. of Pavy's solution = 0·02 gram of glucose.

And 11·2 c.c. of the kreatinin solution contain 0·0251 gram of $C_4H_7N_3O$.

Therefore 12·5 grams of the effloresced kreatinin are equivalent to 10 grams of glucose in reducing action upon cupric oxide.

Again 0·5 gram of the *tabular kreatinin α* of urine was dissolved in 250 c.c. of water.

6 c.c. of this solution was required to decolourise 20 c.c. of Pavy's ammoniacal cupric solution (= 0·01 gram of glucose). And 11·9 c.c. was required for 40 c.c. of the cupric solution.

Hence 12 grams of the *tabular kreatinin α* of urine are equivalent to 10 grams of glucose in reducing action upon cupric oxide.

Now $$\underset{=226}{2CH_7N_3O} : \underset{=180}{C_6H_{12}O_6} :: 12 : 9{\cdot}5.$$

From which it appears that 2 mols. of the reducing kreatinin of urine are about equal to 1 mol. of glucose in reducing action upon cupric oxide.

Reduction of Cupric Oxide by Kreatinin from Kreatin of Flesh prepared by Liebig's Method.

62 lbs. of fresh beef, treated by Liebig's process for the extraction of the kreatin, gave some fine crystals of that substance.

3·685 grams of the kreatin obtained in this way was converted into kreatinin hydrochloride by the action of dry hydrogen chloride in Liebig's drying tube.

The resulting hydrochloride was dissolved in 24 parts of water as recommended by Liebig, boiled, and treated with pure lead hydrate added a little at a time. In short, Liebig's directions for preparing kreatinin from the kreatin of flesh were followed as exactly as possible. The kreatinin thus prepared was re-crystallised six times from the smallest possible quantity of cold water by evaporation *in vacuo* over sulphuric acid.

Well-formed anhydrous tabular crystals resulted. In order to compare the cupric oxide reduction of this substance with that of the *natural kreatinin of urine*, a solution was made containing 0·1 gram of the substance in 100 c.c.

27·5 c.c. of this solution decolourised 30 c.c of Pavy's ammoniacal cupric solution (= 0·015 gram glucose).

Hence 180 parts by weight of glucose are equivalent to 329·94 parts by weight of kreatinin.

Or 1 mol. of glucose is equal to 3 mols. of kreatinin from kreatin of flesh in reducing action upon cupric oxide.

Whereas 1 mol. of glucose is equal to 2 mols. of the natural kreatinin of urine in cupric oxide reducing power.

But the important practical deduction which I draw from the above data is this—that the natural kreatinin of urine is responsible for the bulk of the reducing action exerted by normal human urine, and further that, in my belief, the whole cupric oxide reduction effected by that secretion may be accounted for by uric acid and kreatinin.

Taking 1500 c.c. (= 52·8 fluid ounces) as the average volume of urine passed by a healthy man in the twenty-four hours, and 6 to 7 grams per litre as the average weight of the spherical mercury salt of kreatinin yielded by the normal secretion, then, as the spherical mercury salt contains 20 per cent. of kreatinin ($C_4H_7N_3O$), the weight of anhydrous kreatinin passed in twenty-four hours by a healthy man will vary between 1·8 and 2·1 grams. And as 12 grams of the reducing kreatinin of urine are equivalent to 10 grams of glucose in reducing action upon cupric oxide, it follows that the cupric oxide reduction effected by the urinary kreatinin in twenty-four hours is equivalent to that which would be produced by 1·5 to 1·75 grams of glucose, *i.e.*, by from 23 to 27 grains of glucose in 52·8 fluid ounces of urine.

Accordingly cupric oxide is reduced by the kreatinin of normal urine in the same degree as if the secretion contained from 0·43 to 0·51 grain of glucose in each fluid ounce.

But the total cupric oxide reduction effected by normal human urine is equivalent to from 0·6 to 0·8 grain of glucose per 1 fluid ounce, and of this total reduction one-fourth has been shown by Dr. Pavy to be due to uric acid. Therefore the total reduction is accounted for by the conjoined action of the uric acid and kreatinin.

I can only attribute the little importance which has hitherto been attached to the reducing action of urinary kreatinin, and the low estimates which have been made thereof, to the fact that only one substance having the formula $C_4H_7N_3O$ has hitherto been recognised by physiological chemists, and that therefore the properties of the kreatinin obtained by Liebig's process from kreatin of flesh have been supposed to be identical with those of the *natural kreatinin of urine.*

I shall prove in Part II of this paper that the properties of the kreatinins obtained from the kreatin of urinary kreatinin itself, by treating it according to Liebig's direction, are in many respects different from those of the natural base. It will be sufficient at present to emphasise some of the differences in properties which I have observed between the natural kreatinin of urine and the kreatinin described by Liebig.

1st. As regards *reducing action.*

I have prepared kreatinin from flesh kreatin by Liebig's process, and have found differences in the reducing action given by the products. But the specimen which agreed best with the description given by Liebig was destitute of reducing action.

Professor W. N. Hartley has compared the absorption spectrum of this specimen with that of a specimen of *tabular kreatinin α of urine* for me, and his results are published in an Appendix to this paper.

2nd. The *platinum salts* differ.

In all the descriptions of "kreatinin platinic chloride" that I have found, the salt is described as *anhydrous.* I have subjected the platinum salt of urinary kreatinin to complete analysis, and find that it contains 2 mols. of water of crystallisation.

3rd. The *solubilities in water and alcohol* differ.

The solubility in alcohol of kreatinin (Liebig) is 1 part in 102 parts of alcohol at 16° C.

The solubility in alcohol of *tabular kreatinin α of urine* is 1 part in 362 parts of alcohol at 17° C.

Kreatinin (Liebig) dissolves in 11·5 parts of water at 16° C.

Tabular kreatinin α of urine dissolves in 10·78 parts of water at 17° C.

It appears, therefore, that the tabular kreatinin of urine is slightly more soluble in water, and vastly less soluble in alcohol than the artificial base described by Liebig.

I will conclude this part of my work by laying stress upon the importance of the precipitation of kreatinin from urine as spherical mercury salt, not only as a convenient method of obtaining the natural base in a state of purity, but also as a means of estimating with accuracy the *quantity* of kreatinin in a given volume of the secretion; and at the same time the amount of reduction exerted by the natural base. In Part II will be found an account of the bases obtainable from the kreatin of urinary kreatinin.

Part II.

On the Kreatinins derived from the Dehydration of Urinary Kreatin.

When the kreatinin of urine is boiled in dilute aqueous solution in proportion 1 : 1000 by weight of water, it is gradually converted into kreatin, which may be crystallised out by concentrating at the boiling temperature. If the first crop of kreatin be separated from the mother-liquor, the latter, diluted largely and again concentrated, yields a further crop of crystals, and this may be repeated as long as any kreatin crystallises out. The whole of the crystals thus obtained are then dissolved together in boiling water, the solution kept boiling with animal charcoal till colourless, filtered, and concentrated. On cooling the pure urinary kreatin crystallises out.

Determinations of Water of Crystallisation in Urinary Kreatin.

Weight of air-dried crystals in grams.	Weight lost at 100° C.	Weight lost by 100 parts.	H_2O in 100 parts of $C_4H_9N_3O_2.H_2O$.
3·547	0·4415	12·45	12·08
2·955	0·363	12·28	12·08

These results agree with the formula $C_4H_9N_3O_2.H_2O$.

Conversion of Urinary Kreatin into Kreatinin Hydrochloride by Liebig's Process.

Weighed portions of the air-dried urinary kreatin were introduced into a counterpoised Liebig's drying tube, connected at one extremity with apparatus for the delivery of dry air or dry hydrogen chloride at will, and at the other extremity with a counterpoised bulb apparatus containing sulphuric acid. In some experiments the kreatin was first dried at 100° C. in dry air, and then subjected to the action of dry HCl at 100° C.; in others the crystals were at once acted on by HCl, without previous removal of their water of crystallisation. The final products were found to have similar properties in all the experiments. Dry air was, of course, kept passing in all cases at 100° C., until no more water was expelled, *i.e.*, until the weight of the sulphuric acid bulbs in front of the kreatin tube became constant.

The following are some of the quantitative results:—

Weight of kreatin taken in grams.	Weight of H_2O collected.	Weight of kreatinin hydrochloride found.	Theoretical weight of H_2O expelled.	Theoretical weight of kreatinin hydrochloride.
(Cryst.) 1·066	0·276	1·076	0·259	1·0696
(Anhydrous) 3·1055	0·412	3·507	0·426	3·544
(Anhydrous) 2·592	0·364	2·950	0·356	2·957
(Cryst.) 2·659	0·594	2·708	0·642	2·661
(Cryst.) 5·063	1·220	5·078	1·223	5·0799

The above results sufficiently prove that when urinary kreatin is acted upon by dry HCl at 100° C., the change is expressed by the equation $C_4H_9N_3O_2.H_2O + HCl = C_4H_7N_3O.HCl + 2H_2O$.

Comparison between Kreatinin Hydrochloride prepared by Liebig's Process from Urinary Kreatin and the Kreatinin Hydrochloride obtained from the Spherical Mercury Salt of Urine.

When the contents of the tube, in which urinary kreatin has been converted into kreatinin hydrochloride by dry HCl at 100° C., are dissolved in cold water, three times its weight of which is sufficient for complete solution, there are obtained on evaporation *in vacuo* over sulphuric acid transparent flattened prisms, which rapidly become opaque on exposure to common air by efflorescence.

1·26 gram of these crystals (air-dried and weighed at once) lost 7·06 per cent. after 48 hours' exposure to common air at 15° C. Being finally dried at 100° C., they were found to have lost altogether 10·9 per cent.

The formula $C_4H_7N_3O.HCl.H_2O$ requires 11·04 per cent. H_2O. In this respect, kreatinin hydrochloride obtained by Liebig's process from urinary kreatin, resembles the kreatinin hydrochloride obtained from kreatinin of flesh by the same process.

Thus 0·323 gram of air-dried transparent square plates of kreatin hydrochloride, prepared by me from beef by Liebig's process, and crystallised by spontaneous evaporation from cold aqueous solution, lost 12·4 per cent. of their weight when dried at 100° C., which is rather more than is required by the formula $C_4H_7N_3O.HCl.H_2O$. The occurrence of water of crystallisation in the crystals obtained by spontaneous evaporation of cold aqueous solutions of kreatinin hydrochloride obtained artificially by Liebig's process from kreatin (whether urinary or sarcous in its origin) is an important means of distinguishing these artificial hydrochlorides from the natural hydrochloride of urinary kreatinin, the crystals of which I have invariably found to be anhydrous, even when the utmost care is taken to avoid heating their solution (*vide* Part I of this paper).

Description of the Preparation of Kreatinins from the Kreatinin Hydrochloride produced from Urinary Kreatin.

The same process was adopted in liberating the kreatinin from the artificial hydrochloride as in the case of the hydrochloride of the kreatinin of urine itself. The concentrated aqueous solution was well stirred with excess of pure lead hydrate, at the ordinary temperature, till the reaction to litmus became strongly alkaline. The liquid was then filtered and evaporated *in vacuo* over sulphuric acid. In some cases this treatment produced efflorescent kreatinin, exactly resembling in crystalline form and in composition the efflorescent kreatinin of urine ($C_4H_7N_3O.2H_2O$); whilst in other cases anhydrous tabular crystals were formed, which were found to be crystallographically identical with the tabular natural kreatinin of urine. I have been unable to ascertain the cause of the formation of these two bodies in different experiments, but I shall show presently that they are very easily convertible the one into the other.

For the crystallographic examination of the kreatinin crystals prepared by me from urine and from the kreatin of urine, I am indebted to the kindness and patience of Mr. L. Fletcher of the Mineralogical Department of the British Museum. He has found that the tabular crystals of kreatinin, both natural and artificial, are identical in their angles with those measured by Kopp and Heintz. He has also made for me (with the aid of Mr. H. A. Miers) what, I suppose, are the first measurements of the efflorescent kreatinin. Mr. Fletcher's report is as follows :—

Kreatinin (measured by Fletcher).

Anhydrous tabular K. from urine and from urinary kreatin by Liebig's process. Thin, rectangular, nearly square tables.

System—Monosymmetric.
Elements—$a : b : c :: 1{\cdot}235 : 1{\cdot}116$; $y = 69° 47'$.
Forms observed—$a\{100\}$, $c\{001\}$, $m\{110\}$, $d_1\{\bar{1}01\}$.

The development of the forms is illustrated by Figs. 2 and 3.
The form d was only observed in one of the crystals.
There is an easy cleavage parallel to a_1 $\{100\}$.

FIG. 2.

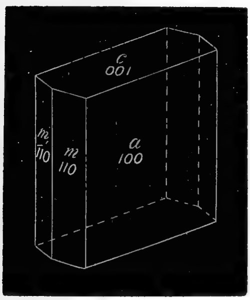
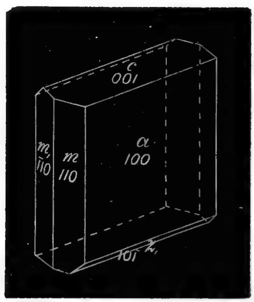


FIG. 3.



Observed.			Kopp.	Heintz.
Angles.	Mean.	Limiting values.		
ac....	69° 47′	69° 35′—70° 3′	69° 24′	69° 57′—70° 30′.
cd_1 ...	50° 58′	50° 50′—51° 6′		
am ...	49° 13′	48° 48′—49° 34′		
mm_1..	81° 34′	81° 12′—81° 53′	81° 40′	81° 40′, sometimes 37′ or 38′.

The crystals were thus identical in their angles with those measured by Kopp and Heintz.

Efflorescent Kreatinin, ($C_4H_7N_3O.2H_2O$). *Measured by L. Fletcher and H. A. Miers.*

Long prisms.
System—monosymmetric.
Elements—$a : b : c :: 0·6114 : 1 : ? ; y = 80° 18'$.
Forms observed—$b\{010\}$, $c\{001\}$, $m\{110\}$, $h\{120\}$, $f\{130\}$.

Angles found.	Observed.	Calculated.
bm	58° 55½′	58° 55½′
cm	81° 42′	81° 42′
bh	39° 40½′	39° 41′
bf	28° 7′	28° 57′
ch	83° 45′	83° 49′
cf	85° 13′	85° 19′

Analyses of Kreatinins from Urinary Kreatin by Liebig's Process. Determinations of Water of Crystallisation in the Efflorescent Kreatinin from Urinary Kreatin.

The same difficulty was experienced in making these determinations as in the case of the efflorescent kreatinin obtained directly from urine, on account of the extreme rapidity of the efflorescence.

Weight of efflorescent kreatinin taken (in grams).	Loss of weight at 100° C.	Loss of weight in 100 parts.	Theoretical loss for $C_4H_7N_3O.2H_2O$.
0·726	0·1736	23·91	24·16
0·465	0·114	24·51	24·16

The formula $C_4H_7N_3O.2H_2O$ receives additional support from the results of the ultimate analysis of the efflorescent kreatinin, before it has undergone efflorescence.

Combustion of Efflorescent Kreatinin ($C_4H_7N_3O.H_2O$) from Urinary Kreatin.

Weight of substance taken in grams.	Hydrogen found.	Carbon found.	Hydrogen in 100 parts found.	Carbon in 100 parts found.
0·284	0·0203	0·0917727	7·16	32·31

	Found in 100 parts.		Required for $C_4H_7N_3O.2H_2O$ ($C_4H_{11}N_3O_3$).
Carbon	32·31	......	32·21
Hydrogen	7·16	......	7·30

The ultimate analysis of the effloresent kreatinin after efflorescence proves that the composition of the effloresced substance is identical with that of the anhydrous tabular kreatinin from urinary kreatin.

Ultimate Analysis of Effloresced Kreatinin from Kreatin of Urine and of Tabular Kreatinin from the same source.

Description of substance.	Weight of substance in grams.	Weight of hydrogen found.	Weight of carbon found.	H in 100 parts.	C in 100 parts.
Effloresced kreatinin from kreatin of urine	0·215	0·0137	0·09136	6·404	42·49
Tabular kreatinin from kreatin of urine	0·312	0·0195	0·1342	6·26	43·01
Tabular kreatinin from kreatin of urine	0·1515	0·0100	0·0641	6·60	42·47

Description of substance.	Weight of substance in grams.	Vol. of nitrogen at 0° C. and 760 mm.	Weight of nitrogen in grams.	Nitrogen in 100 parts.
		c.c.		
Effloresced kreatinin from urinary kreatin	0·154	45·574	0·057171	37·123
Tabular kreatinin from urinary kreatin	0·1505	44·53	0·055862	37·117

These results agree well with the formula $C_4H_7N_3O$, as shown by the following table:—

	Found in tabular kreatinin from urinary kreatin.		In effloresced kreatinin from same source.	Required for $C_4H_7N_3O$ in 100 parts.
		Mean.		
Carbon	42·31 43·01	42·66	42·49	42·478
Hydrogen............	6·60 6·26	6·43	6·404	6·194
Nitrogen	..	37·117	37·123	37·168
Oxygen (by difference) ..	..	13·793	(differ.) 13·983	14·160
	..	100·000	100·000	100·000

Platinum and Gold Salts of Kreatinins from Urinary Kreatin.

When a solution of tabular kreatinin, prepared from urinary kreatin by Liebig's process, and which has re-crystallised in the tabular form from cold aqueous solution, is acidulated with hydrochloric acid, mixed with aqueous solution of platinic chloride and evaporated *in vacuo* over sulphuric acid, a crystalline platinum salt is formed. The crystals are paler in colour than the platinum salt of the tabular kreatinin of urine, and require nearly twice as much water to dissolve them. The salt is easily purified by washing away the excess of platinic chloride from it with strong alcohol, and then re-crystallising from watery solution.

Like the platinum salt of the kreatinin of urine, it contains two molecules of water of crystallisation.

0·162 gram of the salt became opaque at 100° C., losing 0·009 gram of water.

This corresponds with 5·55 per cent.

The formula $2\{C_4H_7N_3O.HCl\}PtCl_4.2H_2O$ requires 5·34 per cent.

Comparison between the Platinum Salts of Tabular Kreatinin α of Urine and Tabular Kreatinin α from the Urinary Kreatin, as regards solubility in Water.

Weight of solution (in grams) saturated at 15° C.	Name of salt taken.	Weight of dried at 100° C.	Parts by weight of water which dissolve 1 part of salt at 10° C.
9·837	Platinum salt of tabular Kα of urine	0·652	14·1
3·641	Platinum salt of tabular Kα from urinary kreatin	0·143	24·4

Thus the platinum salt of the natural kreatinin is nearly twice as soluble in water as that of the artificial base.

Neither the efflorescent kreatinin of urine, nor the efflorescent kreatinin from urinary kreatin form platinum salts of any definite nature. After treatment with alcohol the crystalline matter which remains upon spontaneous evaporation of mixed chlorides is found to be a mixture of a little yellow granular matter with transparent colourless needles, probably the hydrochloride of the kreatinin.

With auric chloride, however, the efflorescent kreatin, and also the tabular anhydrous kreatinin from urinary kreatin, give fine gold salts, having the formula $C_4H_7N_3O.HCl_4.AuCl_3$, and crystallising in thin yellow lustrous plates.

The gold salt of the efflorescent kreatinin from kreatin is darker in colour than that of the efflorescent kreatinin of urine, but it has the same composition, as shown from the following analysis:—

Gold Salt of Efflorescent Kreatinin from Kreatin.—0·520 gram gave 0·226 gram Au, equivalent to 43·461 per cent.

	Found.	Required for $C_4H_7N_3O.HCl.AuCl_3$.
Au	43·461	43·43

The gold salts of natural kreatinins from urine may be readily distinguished from those of artificial kreatinins obtained from the urinary kreatin, by the action of ether, which decomposes the latter, but has no action upon the former. When applied to the crystals of artificial kreatinin auric chloride, ether renders them opaque, dissolving out auric chloride and leaving the hydrochloride of the kreatinin.

The crystals of gold salt of the natural kreatinin on the contrary remain unchanged under ether.

Also, if ether be added to the alcoholic solution of the natural kreatinin in auric chloride, no change takes place, but it causes a precipitate of the kreatinin hydrochloride when added to the alcoholic solutions of the gold salts of the artificial kreatinins.

Solubility in Water and Alcohol of Tabular Kreatinin α from Urinary Kreatin.

Weight of solution in water, saturated at 17° C.	Weight of residue dried at 100° C.	Parts by weight of water at 16·5° C. required to dissolve 1 part of base.
5·0412 grams	0·4314 gram	10·68 parts

This agrees very closely with the solubility of natural tabular kreatinin in water, which requires 10·78 parts at 17° C.

The solubility of the artificial tabular kreatinin in alcohol is, however, greater than that of the natural base. At 18·5° C., 324 parts of absolute alcohol dissolve 1 part by weight of tabular kreatinin α from urinary kreatin, whereas the natural tabular kreatinin of urine requires 362 times its weight at 17° C.

Effects of Re-crystallisation from Aqueous Solution at Different Temperatures of Kreatinins obtained from Urinary Kreatin.

There appear to be four varieties of kreatinin obtainable from urinary kreatin.

(1.) Efflorescent kreatinin α, which, having effloresced, recrystallises in the same form after spontaneous evaporation of its cold aqueous solution.

(2.) Efflorescent kreatinin β, which, having effloresced, recrystallises in the tabular anhydrous form from cold aqueous solution.

(3.) Tabular kreatinin α, which recrystallises in the same form when its cold aqueous solution is evaporated *in vacuo* over sulphuric acid.

(4.) Tabular kreatinin β, which recrystallises in the efflorescent form when its cold aqueous solution is spontaneously evaporated.

There is no apparent difference between the crystals of (1) and (2), nor any between those of (3) and (4). Yet, when the conditions of solution and evaporation are kept as similar as possible, there is the remarkable difference described in the products of re-crystallisation. These four substances may be obtained as follows:—

(1.) Efflorescent kreatinin α is the product usually obtained after the treatment of the kreatinin hydrochloride, prepared from urinary kreatin, by Liebig' process, with pure lead hydrate at the ordinary temperature. After it has effloresced, we may obtain from it either (3) or (4).

The tabular kreatinin α is formed if the solution of the effloresced crystals is made at 100° C., even though the subsequent evaporation be conducted at the ordinary temperature.

The tabular kreatinin β is the product if the effloresced crystals are dissolved in water at 60° C.

The weight of both these forms of crystals is always identical with that of the effloresced kreatinin from which they were obtained.

(2.) Efflorescent kreatinin β is obtained by dissolving tabular kreatinin α in water at 60° C., and then subjecting the solution to evaporation *in vacuo* over sulphuric acid. Efflorescent kreatinin is never obtained by deposition on cooling from a hot solution.

From tabular kreatinin α tabular kreatinin β is obtained by dissolving in boiling water, and then evaporating the solution at the ordinary temperature.

The following are some results of re-crystallisation of artificial urinary kreatinin:—

Nature of crystals.	Dissolved in	Evaporated.	Product.
Tabular K......	Water at 60° C.	In vacuo ...	Efflorescent Kβ.
Efflorescent Kβ..	Cold water....	Do. ...	Tabular Kβ.
Tabular Kβ	Do.	Do. ...	Efflorescent Kα.

In this case alternate crystallisation of tabular and efflorescent kreatinin took place twice, the kreatinin becoming finally permanently efflorescent. If repeated re-crystallisations by spontaneous evaporation from cold aqueous solution be made, the kreatinin tends ultimately to assume the efflorescent form.

By redissolving at 60° C., we may, at any time, cause the production of temporarily tabular kreatinin β, which swings back to the permanently efflorescent state after one or two re-crystallisations from cold aqueous solution; or by redissolving in water at 100° C., we may produce tabular kreatinin α, which re-crystallises in the same form indefinitely from cold aqueous solution.

It is extremely remarkable, however, that the efflorescent kreatinin is obtained by dissolving tabular kreatinin α in water at 60° C., and then evaporating spontaneously.

Condition of the Two Molecules of Water in the Efflorescent Kreatinin.

Although this water behaves like the water of crystallisation in an efflorescent salt in many respects, and although it does not appear in the gold salt formed by the efflorescent kreatinin, yet there are reasons for supposing that either the kreatinin in the efflorescent kreatinin differs from the tabular anhydrous kreatinin; or else that the water is more closely combined with the kreatinin than ordinary water of crystallisation. For instance—

1st. The solubility in water of the efflorescent kreatinin *before efflorescence* is the same as that of the anhydrous tabular kreatinin.

2nd. When re-crystallised by evaporation of its *cold solution in vacuo* after being dissolved at the boiling temperature, anhydrous tabular kreatinin is produced.

3rd. There is difficulty in forming a platinum salt of efflorescent kreatinin, none in forming the platinum salt of the tabular kreatinin.

4th. When re-crystallised from boiling alcoholic solution, the efflorescent kreatinin forms needle-shaped crystals exactly resembling the original base, and entirely different from those which are produced in the alcoholic solution of the tabular kreatinin.

Cupric Oxide Reduction effected by the Tabular Kreatinin from Urinary Kreatin.

10·8 c.c. of a solution containing 0·1785 gram of tabular kreatinin α from kreatin of urine in 60 c.c., were required to decolourise 40 c.c. of Pavy's ammoniacal cupric solution (= 0·02 gram of glucose), *i.e.*—

10 parts by weight of glucose reduce as much CuO as 16 parts by weight of kreatinin (tabular α), or

2 mols. of glucose are equivalent to 5 mols. of kreatinin in reducing action upon CuO.

A specimen of *tabular kreatinin* β from urinary kreatinin was found to have exactly the same reducing action as the above.

Tabular Synopsis of Different Kreatinins.

	Efflorescent K. of urine. $C_4H_7N_3O.2H_2O$.	Tabular Kreatinin of urine. $C_4H_7N_3O$.	Efflorescent K. from urinary kreatin. $C_4H_7N_3O.2H_2O$.	Tabular K. from urinary kreatin. $C_4H_7N_3O$.	" Kreatinin." (Liebig).
Solubility in water ..	1 in 10·6 at 14° C.	1 in 10·78 at 17° C.	......	1 in 10·68 at 16·5° C.	1 in 11·5 at 16° C.
Solubility in alcohol .	......	1 in 362 at 17° C.	......	1 in 324 at 18·5° C.	1 in 102 at 16° C.
Platinum salt......	Indefinite or decomposed by alcohol	$2(C_4H_7N_3O.HCl)PtCl_4.2H_2O$	Indefinite, or decomposed by alcohol	$2(C_4H_7N_3O.HCl)PtCl_4 \cdot 2H_2O$	$2(C_4H_7N_3O \cdot HCl)PtCl_4$
Solubility of platinum salt in water	......	1 in 14·1 at 15° C.	......	1 in 24·4 at 15° C.	
Gold salt..........	Unchanged by ether	Unchanged by ether	Decomposed by ether	Decomposed by ether	
Reduction of CuO compared with that of glucose	4 molecules $C_4H_7N_3O$ = 2 mols. glucose	4 molecules $C_4H_7N_3O$ = 2 mols. glucose.	5 molecules $C_4H_7N_3O$ = 2 mols. glucose	5 molecules $C_4H_7N_3O$ = 2 mols. glucose.	

Reducing Action of a Specimen of Kreatinin from Kreatin of Beef.

The kreatin from which this kreatinin was prepared was extracted by myself from beef (62 lbs.) by Liebig's process, and I followed Liebig's directions as exactly as possible in converting it into kreatinin. The product, however, required 390·2 times its weight of alcohol at 15° C. for solution, and its platinum salt contained water of crystallisation, so that the base does not agree in properties with that described by Liebig.

The reducing action of this base was feebler than that of the artificial kreatinin from the kreatin of urine. It required 3 mols. of this kreatinin to reduce as much cupric oxide as 1 mol. of glucose.

Thus, comparing the reducing action of kreatinins, we have—

Tabular and efflorescent	Kreatinins from urine.	4 molecules	= 2 mols. glucose.
Tabular α and β	Kreatinins from urinary kreatin........	5	" " "
Kreatinin from kreatin of flesh......		6	" " "

It is evident, therefore, that the reducing action of the natural kreatinin is much greater than any of the artificial kreatinins which I have examined.

In conclusion, I think it is proved—

(1st.) That the most active reducing agent in normal urine is the urinary kreatinin.

(2nd.) That the properties of kreatinins artificially prepared cannot be considered as identical with those of the natural base; and

(3rd.) That there is strong presumptive evidence against the existence of sugar in normal human urine.

In this paper I have confined my attention to the differentiation of kreatinins by the study of their physical properties, reactions, &c. I hope shortly to study the substitution products of the various substances I have described, with a view to the construction of rational formulæ, &c.

Appendix by Professor W. N. Hartley, F.R.S.

On the Absorption-spectrum of a Base from Urine.

The *base* which Mr. G. S. Johnson has separated from urine is regarded as isomeric with kreatinin artificially prepared by Liebig's process; it has therefore been considered of interest to ascertain the character of its absorption-spectrum and more especially to compare the specific absorptive power of the two substances. The method of examination was that described in the 'Proceedings of the Royal

Society,' vol. 33, p. 1, with the modifications described in the 'Journal of the Chemical Society,' vol. 47, pp. 685—757. The absorption-curves have been drawn in the same way as those figured in the latter publication.

The two bodies, kreatinin and the *base* from urine, are compounds with a very similar constitution, as is apparent from the two curves accompanying this note. The absorption-bands are not caused by the condensation of the carbon atoms as in aromatic derivatives of benzene, but by the condensation of the numerous oxygen and nitrogen atoms, as is the case with uric acid. The absorption-bands, though definite, have no great persistency, but the intensity of the total actinic absorption is great, the band of kreatinin becoming visible when 2 mm. of a solution were examined containing little more than 1 part of the substance in 1000 of water. With the *base* from urine 2 mm. of solution show a band when the same quantity is contained in 5000 of water.

The Absorption Spectra of Kreatinin.* 0·0113 gram dissolved in 10 c.c. of water, or a milligram-molecule in 100 c.c.

Thickness of layer of liquid	20 mm.		15 mm. and 10 mm.		5 mm. and 4 mm.	
Description of spectrum	Oscillation-frequencies.	Wave-lengths.	Oscillation-frequencies.	Wave-lengths.	Oscillation-frequencies.	Wave-lengths.
Continuous, strong, to	3080	3245	3080	3245	3080	3245
,, fairly strong, to	3832	2609	3832	2608	3890	2568
Line visible at	..	..	3890	2568	..	..
Rays extend to	..	..	..	..	3936	2538
Thickness of layer of liquid	**3 mm.**		**2 mm.**		**1 mm.**	
Continuous, strong, to	3080	3245	3080	3245	3080	3245
,, fairly strong, to	3890	2568	3890	2568	3890	2568
Rays extend to	4055	2466	4141	2415	4141	2415
ABSORPTION BAND	..	..	4141 to 4331	2415 to 2310	Absorption	weaker.
Spectrum ends at	..	..	..	..	4426	2259

* This specimen of kreatinin was prepared by the action of dry HCl at 100° C. upon kreatin (Liebig's process). It gave all the reactions described by Liebig, but no reduction of picric acid or mercuric oxide at the ordinary temperature. With Nessler's reagent, it gave a yellow precipitate, becoming *red* on standing.—G. S. J.

Kreatinin—*continued*. 0·0113 gram dissolved in 50 c.c. of water, or a milligram-molecule in 500 c.c.

Thickness of layer of liquid	5 mm.	4 mm. and 3 mm.		2 mm. and 1 mm.	
Description of spectrum	The same spectrum as with 1 mm. of the more dilute solution.	Oscillation-frequencies.	Wave-lengths.	Oscillation-frequencies.	Wave-lengths.
Strong and fairly strong, to	......	3890	2568	3890	2568
Rays extend to	......	4566	2190	4660	2146

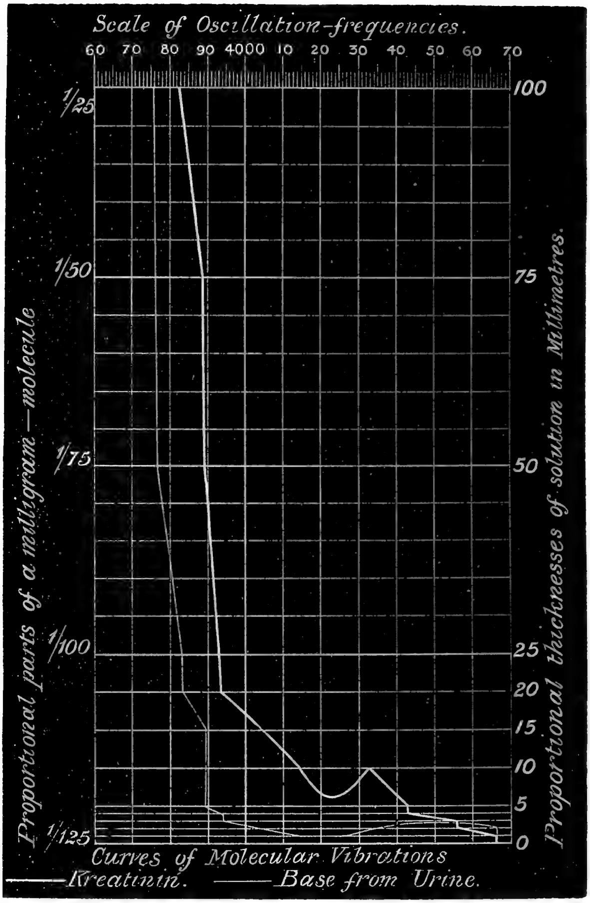
Absorption Spectrum of the Base from Urine. 0·0113 gram. dissolved in 10 c.c. of water, or a milligram-molecule in 100 c.c.

Thickness of layer of liquid	20 mm., 15 mm., and 10 mm.		5 mm. and 4 mm.		3 mm., 2 mm., and 1 mm.	
Description of spectrum	Oscillation-frequencies.	Wave-lengths.	Oscillation-frequencies.	Wave-lengths.	Oscillation-frequencies.	Wave-lengths.
Continuous, strong and fairly strong, to	3647	2740	3768	2653	3832	2609
Spectrum extends	3768	2653	3832	2609	3890	2568

Base from Urine—*continued*. 0·0113 gram dissolved in 50 c.c. of water, or a milligram-molecule in 500 c.c. 5 mm. of this yield the same spectrum as 1 mm. of the more dilute solution.

Thickness of layer of liquid	4 mm. and 3 mm.		2 mm.		1 mm.	
Description of spectrum	Oscillation-frequencies.	Wave-lengths.	Oscillation-frequencies.	Wave-lengths.	Oscillation-frequencies.	Wave-lengths.
Continuous, strong and fairly strong, to	3890	2568	3890	2568	..	..
Spectrum extends to	3936	2538	..	..	..	..
Weak spectrum to	..	..	4055	2466	..	..
ABSORPTION BAND from	..	..	4055 to 4331	2466 to 2310	..	..
Spectrum extends to	..	..	4660	2146	4660	2146

FIG. 4.


Scale of Oscillation-frequencies.
60 70 80 90 4000 10 20 30 40 50 60 70
100
75
50
25
20
15
10
5
0
1/25
1/50
1/75
1/100
1/125
Proportional parts of a milligram—molecule
Proportional thicknesses of solution in Millimetres.
Curves of Molecular Vibrations
——Kreatinin. ——Base from Urine.

OBITUARY NOTICE OF FELLOW DECEASED.

Henry Mangles Denham, son of the late Henry Denham, Esq., of Sherborne, was born on the 28th of August, 1800.

He commenced his career in the Royal Navy at a very early age, and continued an almost uninterrupted course of service afloat in the surveying branch of the profession for the long period of fifty years.

He was one of the most able and eminent of our nautical surveyors, and was considered a high authority on all questions relating to hydrographical engineering; he was intimately connected with the improvement of our great commercial ports, upon which his counsel and advice were frequently sought, almost to the close of his life.

During his early service, between the years 1810 and 1827, he was employed under that distinguished surveying officer, Captain Martin White, on the surveys of the Channel Islands, and in the English and Irish Channels. In the latter year he was appointed to the command of the "Linnet," of ten guns, and, during the next seven years, he conducted the surveys of the Bristol Channel and of the ports of Liverpool and Milford.

He was promoted to the rank of Commander in 1835; and his next service in command of the "Lucifer" was on the surveys of the coast of Lancashire and Cumberland. In 1845 he was appointed to the command of the "Avon," and was sent to the West Coast of Africa on special surveying duties, which he conducted with so much ability, under very unfavourable circumstances, that he was rewarded with post rank in the following year—1846.

He then returned to England, and was again employed on hydrographical duties connected with the home coasts.

Early in the year 1852 Captain Denham was appointed to the command of an expedition, consisting of H.M. ships "Herald" and "Torch," for exploration and survey in the Western Pacific, where he was actively employed until the close of the year 1860.

During this protracted voyage the "Herald" and her consort added greatly to the hydrographical knowledge of this extensive region. Various surveys were made on both the eastern and western coasts of Australia, but the region of Captain Denham's special exploration was the Coral Sea, where he thoroughly examined and defined the route outside the Great Barrier Reefs, by Torres Strait, to the Dutch possessions in the Java Sea, Singapore, and India; likewise among the various groups of islands eastward and northward of Australia, where the salient points of New Caledonia, the New

Hebrides, Fiji and Tonga groups, with those of the Solomon and Louisiade Archipelagos, were accurately determined, and numerous doubtful dangers, long the cause of perplexity and anxiety to the navigator, were either correctly placed or proved to have no existence.

These examinations, and the improved conditions of the charts which resulted from them, much facilitated the intercourse between our great Australasian colonies, the islands of the Western Pacific, and the coasts of China and Japan.

During this voyage a considerable portion of the Fiji group was also surveyed in detail, but the "Herald's" long absence from England caused the completion of this work to be left to other hands.

In 1861 the "Herald" returned to England by Torres Straits and the Cape of Good Hope, arriving home in the month of May, and this memorable voyage terminated Captain Denham's active service afloat.

During the short intervals of active service under the Admiralty, Captain Denham executed several commissions connected with the mercantile steam marine for the Lords Committee of the Privy Council for Trade, and also with reference to harbour improvements at Liverpool, Swansea, and Bideford.

He received votes of thanks from various National and Local Boards, and in 1834 was presented with the freedom of the borough of Liverpool, and elected a member of the Literary and Philosophical Society of that city.

In 1839 he was elected a Fellow of the Royal Society, and, in 1841, a brother of the Corporation of Trinity House, and a corresponding member of the United States National Institute for the Promotion of Science.

Sir Henry Denham was knighted for his long and important services in the "Herald" on the Pacific Station, and especially for the assistance he rendered by his counsel and otherwise to the New South Wales Government during the Russian War of 1854–55.

He was promoted to the rank of Rear-Admiral in March, 1864, of Vice-Admiral in January, 1870, and Admiral in August, 1877.

He died at his residence, Carlton Gardens, on July 3rd, 1887.

G. H. R.

Mr. John Arthur Phillips, F.G.S., F.C S., and M.I.C.E., who died at his home in London through a sudden attack of illness on the 5th of January, 1887, was one of those devotees of scientific inquiry who deserve to have a longer portion of life blest with that leisure which is needed for research.

He was born in the neighbourhood of Polgooth Mine, near St. Austell, Cornwall, and appears to have taken as a boy a hereditary interest in mining and metallurgical matters, for his grandfather was the manager of that noted old tin mine, and was quoted as "its very intelligent director," by Mr. J. Hawkins, F.R.S., and as his chief informant in 1791, on the phenomena of that remarkable locality which was described by Mr. Hawkins in the first volume of the 'Transactions of the Royal Geological Society of Cornwall' in 1818.

Mr. Phillips' attention was drawn in early youth to observations and experiments connected with electricity and the deposition of metallic copper. We may trace this in part to the influence of the Polytechnic Society of Cornwall and its useful annual exhibitions at Falmouth, as well as to the example and studies of the late Robert Were Fox, F.R.S., one of its founders. Young Phillips soon becoming desirous of a more thorough grounding in the metallurgical sciences, entered as a student at the École des Mines, passed its curriculum with credit, and for a short time was intrusted with the charge of a coal mine in the South of France.

On returning to England Mr. Phillips was employed in taking a practical part in the evaporation experiments on steam coals carried out with a Cornish boiler at the Civil Engineers College at Putney, for the Admiralty, the results of which were embodied in the well-known "Report." This work was followed up by various papers on chemical and metallurgical subjects, among which one of the most generally interesting was a "Chemical Examination of the Metals known to the Ancients" ('Journal of the Chemical Society,' 1852, and 'Liebig's Annalen,' vol. 81).

For many years past the rich silver-lead lodes of Pontgibaud in the Auvergne have been worked by a partly English Company, and Mr. Phillips was engaged for a considerable time, between 1855 and 1860, on behalf of the Messrs. Taylor of London, the managers, in experimenting and erecting furnaces for the treatment of those ores. He also for a few years acted as a consulting mining engineer, taking the opportunity of visiting and describing the singular gold-bearing deposits of Nova Scotia, and the more important gold fields of California, his notes on which were published in the 'Proceedings of the Royal Society,' 1868.

But it was not until his metallurgical aptitude was proved by success as a manager that Mr. Phillips obtained the leisure to take up independent studies. The profitable results which attended the co-operation of Mr. Claudet and himself in the conduct of a work at Widnes, in which copper, silver, and gold were extracted from "burnt" Spanish pyrites, placed him in a more favourable position.

He became a Fellow of the Geological Society, and in the 'Quarterly Journal' of that Society, in the 'Philosophical Magazine,' as also in our own 'Proceedings' and elsewhere, he brought out a long series of papers descriptive of results of chemical analysis and of microscopic work as bearing on mining and geological subjects. Among these some of the more notable were those on the phonolite of the Wolf Rock, on the salt spring at Wheal Seton Copper Mine, on the "greenstones" of Cornwall, in which he confirmed the views of Mr. Allport as to the great extent to which rocks, originally augitic, have been converted into varieties of a hornblende character. This he followed up by observations on chemical and mineralogical changes which have taken place in the igneous rocks of North Wales; and in a generally interesting paper in 1875 he showed reasons for calling in question the startling generalisation of Mr. Sorby as to the depth and pressure under which the granitic rocks had been formed.

Numerous observations had before been made on the subject of the dark enclosures—whether rounded or angular—which so often occur in granite; but Mr. Phillips seems to have been the first to apply (1879) a series of analyses and microscopical investigations to the question, although only confirming, after all, the old opinion that some of them are concretionary, others only fragments.

Meanwhile, at various intervals several larger works had issued from his pen—an elementary, and then a fuller Manual of Metallurgy, a treatise on the Mining and Metallurgy of Gold and Silver, a very full compilation from well-selected sources on Ore Deposits; and at the very last he was occupied in bringing out a new edition of his Metallurgy.

Mr. Phillips, on coming from Lancashire to reside in London, added greatly to his circle of friends by his generous and outspoken character, and his good common sense and special knowledge will be seriously missed at the council tables of Societies at which he was a frequent attendant.

W. W. S.

Joseph Baxendell was born in Manchester in 1815, and died at Southport on October 7th, 1887. Having to make his way in the world he was sent to sea at an early age. It is believed that the circumstances of his profession led him to recognise the immense importance of the two great branches of observational science astronomy and meteorology, and to interest himself in their cultivation.

With this object in view he was assiduous in supplementing the deficiencies of a limited education, and ultimately acquired a knowledge of mathematics which was of much service to him in his scientific investigations.

Mr. Baxendell was what this training made him. He became a thoughtful and retiring student of nature rather than one who cared to take a prominent place in general scientific society. But he was much esteemed by those whose tastes were similar to his own, and a meeting of such students usually took place once a fortnight during the winter months at the rooms of the Manchester Literary and Philosophical Society. At first Mr. Baxendell was a regular attendant at these meetings, and ultimately he was chosen to be Secretary of the Society and Editor of its publications. The duties of these offices were discharged by Mr. Baxendell in a very intelligent and conscientious manner.

In astronomy Mr. Baxendell contributed observations of various kinds. Of these perhaps the most important are embodied in his Catalogue of Variable Stars, a work which is highly esteemed by all astronomers.

In meteorology his contributions are of conspicuous importance, and in one branch of this science he may claim to be the pioneer.

In 1871, after having discussed eleven years' observations of the Radcliffe Observatory, Oxford, he came to the conclusion that the forces which produce the movements of the earth's atmosphere are most energetic in those years when there are numerous spots on the surface of the sun. This conclusion was, like that of many similar pioneers, derived from perhaps a somewhat limited series of observations, but the sagacity of Mr. Baxendell is justified by the fact that many other men of science have since followed in his footsteps.

Mr. Baxendell was likewise an independent discoverer of the fact that the faculæ which accompany sun-spots are thrown more behind them than before—the word *behind* having reference to the direction of rotation of the sun upon its axis.

Again he entertained the opinion, which has since spread, that the behaviour of sun-spots is connected in an intimate manner with that of meteoric matter round the sun.

Mr. Baxendell foretold the long drought of 1868, and persuaded the city of Manchester to take precautionary measures which had the effect of mitigating the inconvenience arising from want of water.

He was a Fellow of the Royal Astronomical Society, a corresponding member of the Royal Society of Königsberg, of the Scientific and Literary Academy of Palermo, and of the National Observatories of France, Germany, and Italy.

He held for many years the office of Astronomer to the Manchester

Corporation, and was residing at the Observatory, Southport, at the time of his death.

B. S.

Sir George Burrows, who died December 12th, 1887, was born in 1801 in Bloomsbury Square. His father, Dr. George Man Burrows, a member of a family of Kentish yeomen, who had lived for at least two centuries at Chalk, near Gravesend, was at that time a general practitioner, and one of the most energetic. His early education was at a school of good renown at Ealing, kept by Dr. Nicholas; and among his teachers was Professor Huxley's father, to whose lessons he ascribed the love of mathematics which led to much of his success in later life. In 1819 and 1820, being destined for the medical profession, he attended Mr. Abernethy's lectures and dissected at St. Bartholomew's Hospital, and attended the lectures of Brande and Faraday at the Royal Institution. In 1821 his father determined to send him to Edinburgh, that he might there take his doctor's degree, and the day for his leaving London was fixed; but, on the urgent advice of Dr. Latham, who pointed out the far greater value of an English degree to one who was to practise in London, the plan was changed, and he went to Cambridge and entered at Caius College.

There he worked hard, did well in the annual college examinations, was active in athletics, a good rower and cricketer, but in social life was deemed quiet and reserved. In 1825 he took his B.A. degree, passing as tenth wrangler, and was soon after elected a Fellow of his College. During his undergraduate time he had been appointed to a Tancred Studentship, which involved the necessity of his taking the M.B. within the year after the B.A.; but he obtained some respite from this rule, took pupils, was a junior mathematical lecturer, studied what he could of medicine with the University professors, and passed the M.B. examination at some time in 1826. Soon after this he returned to St. Bartholomew's, was for twelve months one of Lawrence's dressers, and was a constant worker with Latham and Watson. Thus he went on till, having a good opportunity of travelling, he visited and studied at the Universities of Paris and Pavia and some of those in Germany. In 1829 he obtained at Cambridge a licence to practise, and was admitted an inceptor candidate at the College of Physicians. In 1831 he took his M.D., and was appointed with Dr. Roupell to the Lecturership on Forensic Medicine, then first instituted at St. Bartholomew's. In 1832 he was admitted a Fellow of the College of Physicians, and was put in charge of wards prepared for cholera patients in the epidemic of that year, the first time of its occurrence in England. In 1834 he was appointed the first Assistant-Physician, and took charge of medical out-patients, who were then, for the first time, dealt with as a separate class.

From this time onwards Sir George Burrows's career was one of constantly increasing success and professional distinction. It may be indicated by the offices to which he was appointed.

At the College of Physicians he was Gulstonian Lecturer in 1834; Croonian in 1835 and 1836; Lumleian in 1843 and 1844; Censor in 1839–40–43 and –46; Councillor for five periods of three years between 1838 and 1870; President from 1871 to 1875. In the General Medical Council he represented the College, and was one of the Treasurers from 1860 to 1863, and was President from 1864 to 1869.

In the Hospital he became in 1834 sole Lecturer on Forensic Medicine, in 1836 joint Lecturer on Medicine with Dr. Latham, in 1841 sole Lecturer and full Physician,—appointments which he held till 1863, when, on his retirement, he was elected Consulting Physician.

In 1870 he was appointed Physician-Extraordinary to the Queen; in 1873, Physician-in-Ordinary. In 1874 he was made a Baronet.

He was President of the Medico-Chirurgical Society in 1869–71; President of the British Medical Association in 1862; was elected a Fellow of the Royal Society in 1847, and Honorary LL.D. of Cambridge and D.C.L. of Oxford, a Member of the Senate of the University of London, and an Honorary Fellow of Caius College, Cambridge. He was a very active member, as his father had been, of the Society for the Relief of Widows and Orphans of Medical Men, and was for many years its President, as he was also of the British Medical Benevolent Fund.

This brief and swift recital of the appointments which Sir George Burrows filled may tell the general character of his professional life, and may be sufficient evidence of the esteem with which he was always regarded, and of the assurance that was felt that, whatever duties were assigned to him, he would do them well. All the high offices, all the honours conferred on him, seemed to come quite naturally and of course; he never asked for one, or did anything on purpose to obtain one; his having them excited neither jealousy nor surprise; and herein may be at once the explanation and the chief lesson of his life. He had excellent mental power. He showed it in his University career, and always afterwards; but that which was yet more admirable and characteristic was his steadfast, resolute use of his power straight to the work he had to do. More enthusiasm or more enterprise might have made him a more impressive or more popular teacher, might have made him more keen in research, more successful in acquiring new knowledge; but they might not have added to the general utility or the good influence of the long life which he spent in learning and teaching what seemed directly useful, in treating disease in the methods generally regarded as the best, and in discussing all manner of questions relating to his profession in senates, councils, and committees.

Sir George Burrows was not a frequent writer on medical subjects. The only book he wrote was 'On the Disorders of the Cerebral Circulation,' 8vo., 1846. The substance of it had been given in the Lumleian Lectures at the College of Physicians in 1843 and 1844, and its chief value was in the evidence which it gave of the error of the belief, then generally held, that the cranium being a complete case of bone, completely filled by the brain and its membranes, and excluding from them all atmospheric pressure, the quantity of blood circulating in the brain cannot be materially increased or diminished by posture, bleeding, changes in the heart or breathing, or by any such means. The belief thus held was not only general, but was influential in the treatment of disease, leading some to hold that, so long as the skull was entire, no abstraction of blood, by any manner of bleeding, could have any effect on the blood-vessels of the brain, so as to lessen the absolute quantity of blood contained within them.

In opposition to this, Sir George Burrows showed, in careful experiments, testing those of Dr. Kellie on which chiefly the belief had rested, that the quantity of blood in the brain is materially altered by bleeding largely, and by posture and by suffocation; and that, admitting that the contents of the cranium must be always nearly the same, the variations in the blood may be balanced by those of the cerebro-spinal fluid.

As one reads this book one cannot but regret that he did not give himself more frequently to original research, for it is clear, critical, and definite, and it greatly helped to the correction of serious errors. But he was not fond of research; he preferred the daily business of practical life, and in it the use of the best knowledge he could gain from others' and his own attentive observation. The only other essays that he published were two papers in the 'Medico-Chirurgical Transactions,' one "A Case of Extensive Carcinoma in the Lungs," in vol. 27, the other on "Tubercular Pericarditis," in vol. 30, and the articles on measles, scarlet fever, and hæmorrhage in Tweedie's 'Library of Medicine.' Besides these he published some clinical lectures in the 'Medical Gazette;' and his first lecture on Forensic Medicine, which was also separately printed, is in the 'London Medical and Surgical Journal' for February 4th, 1832.

From all this I think it may justly be said that that which most marked Sir George Burrows's mental character, and contributed most to his professional success and to his influence and utility, was that, having a strong will and a strong, clear intellect, he applied them steadfastly to the plain daily duties of his life.

J. P.

Astley Cooper Key was the second son of Mr. Charles Aston Key, Surgeon-in-Ordinary to H.R.H. the late Prince Consort, and was born in the year 1821; he was educated at the Royal Naval College at Portsmouth, and from his boyhood he manifested a scientific bent of mind, which he cultivated and followed up in after life, so far as the duties of a most active and unremitting professional career afforded him the leisure to do so.

At the Naval College he gained the prize which carried with it a Lieutenant's commission, and he was consequently promoted to that rank as soon as he became eligible in point of age.

In 1843 he was appointed to the "Gorgon," Captain, afterwards Sir Charles, Hotham, and served in her on the South-east Coast of America; he was the junior Lieutenant of this ship in 1844 when, during a severe *pampero*, she was driven from her anchors at Monte Video, and cast upon the beach; when the waters had subsided, which during this storm had risen 20 feet above the usual sea-level, the "Gorgon" was left literally on the dry land, from which very few, save her gifted Captain—who never doubted but that she must float again—believed that she would ever be moved; Mr. Key was among those few, and by his zeal and untiring exertions added in no small degree to the successful result. After many months of persevering efforts, under great difficulties and undiscouraged by frequent failure, the "Gorgon," by the united exertions of the English squadron in these waters, was again, uninjured, upon her proper element.

The writer of this notice was present, and well remembers the jokes and jeers of the foreign ships of war at her expense; the French Admiral remarking "that no one but a pig-headed Englishman would have persevered in such a hopeless task." He was the first, however, in his flag-ship to give the "Gorgon" three hearty cheers as she steamed round the squadron after her remarkable release.

Mr. Key wrote a narrative of the means employed in this most successful operation entitled, "The Recovery of the 'Gorgon,'" which added much to his professional reputation. In the following year (1845) the "Gorgon" took part in the combined attack by the English and French squadrons on the forts and forces of General Rosas, President of Buenos Ayres, at Obligado, in the Parana.

Captain Hotham, who commanded the English squadron, gave Mr. Key the command of an armed brig (the "Fanny") on this expedition, and in her he was present at the capture of the forts and during the subsequent operations which were undertaken with the view of opening the upper waters of the River Plate, and establishing commercial intercourse with Paraguay. These operations were continued until the close of the year 1846; and for his share in them

Mr. Key was promoted to the rank of Commander on the day of the action at Obligado—viz., the 18th of November, 1845.

Commander Key's next service was in command of the "Bull Dog," from 1847 to 1850, with the Mediterranean Fleet, under the late Admiral Sir William Parker, Bart. During the Sicilian Revolution of 1848 he was despatched for the protection of British subjects at Palermo, where by his energy and tact the Neapolitan troops were prevented from attacking the English quarter of the city; he was afterwards sent on a delicate mission to Civita Vecchia, and placed his ship at the disposal of the Pope should it have become necessary for him to embark from his dominions—Rome being in a very disturbed state. His Holiness, however, from various causes decided to escape by land to Gaeta, which he did in disguise.

Commander Key's services were so highly appreciated by the Commander-in-Chief on these occasions that he was especially recommended, and was promoted to the rank of Captain in 1850.

Captain Key next served in command of the "Amphion" during the Baltic Campaign of 1854, when he took part in the capture of the forts of Bomarsund, and other operations.

In 1855 he was appointed to the command of the "Sans Pareil" screw line-of-battle-ship, and was one of the Captains selected to command a flotilla of gun- and mortar-boats then preparing for the attack on Cronstadt, in the summer of that year; in the meantime, however, peace was concluded with Russia, when for his services during the war he was nominated a C.B.

On the breaking out of the Indian Mutiny, in 1857, he was sent in the "Sans Pareil" with a squadron of gunboats to Calcutta, and for his services there received the thanks of the Indian Government.

In 1858 he was ordered to China, and commanded a battalion of seamen at the capture of Canton. On the signing of the Treaty of Peace at Tientsin in June, 1858, Captain Key returned to England and served as the naval member on the Royal Commission which was appointed to consider the condition of our coast defences.

In 1860 he was appointed to the command of the Steam Reserve at Devonport, and after three years' service in that capacity he was transferred to the command of the "Excellent," the gunnery ship at Portsmouth, and was also Superintendent of the Royal Naval College at that port, where he served until 1865.

About this time, the great change in the size and power of naval guns, brought about by the introduction of armour-plated ships, necessitated the creation of a new department at the Admiralty, and Captain Key was appointed Director-General of this new Naval Ordnance Department, which he held as Captain and Rear-Admiral until 1869, having been promoted to flag rank in 1866.

In the latter part of 1869 he was appointed Admiral Superintendent of Portsmouth Dockyard, but was shortly transferred to a similar position at Malta, when he became second in command of the Mediterranean fleet.

Soon after vacating this position, he was at the end of 1872 appointed President of the newly-established Naval College at Greenwich for the higher education and study of naval officers of all ranks. In 1873 he was promoted to the rank of Vice-Admiral, and in January, 1876, was appointed Commander-in-Chief on the North American and West Indian Stations. In 1878 he became Admiral, and received the appointment of First and Principal Naval Aide-de-Camp to the Queen.

In the year 1879 he went to the Admiralty as Principal Naval Lord, where he served under two administrations until 1885. During Lord Northbrook's absence on his mission to Egypt in 1884, Sir Cooper Key was sworn of the Privy Council, and conducted the administration of the Admiralty.

In 1866 he came under the Age Retirement Scheme, and was placed on the Retired List of Admirals. He had been nominated a K.C.B. in 1873, and was raised to the dignity of a G.C.B. in 1882. The University of Oxford had, in 1880, conferred upon him the honorary degree of D.C.L.

There have been few naval officers who have enjoyed so long and uninterrupted a career, or who have held positions of so important and responsible a character as Sir Cooper Key. He was always a most successful and popular officer, and during his whole course of service had displayed qualities and abilities of a high order, whether as a commander or an administrator; he was an earnest and generous supporter of many benevolent institutions, especially of those connected with the moral and religious training of seamen.

He died at his residence, Laggan House, Maidenhead, on the 3rd of March, 1888.

G. H. R.

Vice-Admiral Thomas A. B. Spratt, C.B., the eldest son of the late Commander James Spratt, who served with much distinction on board H.M.S. "Defiance" at the battle of Trafalgar, was born in 1811, and entered the navy in 1827. As midshipman he joined the surveying branch of the naval service on board H.M.S. "Mastiff" in the Mediterranean, on which station he served all but continuously until 1863.

In 1847 he was appointed as a lieutenant to the command of the surveying vessel "Volage," and in the following year succeeded as commander to the command of the "Spitfire," the principal surveying ship of the station.

Employed mainly in the Archipelago, Commander Spratt worked steadily at charting those intricate seas, whilst his archæological and geological knowledge enabled him to make and publish many scientific observations on the places visited. In 1847 he published with Professor E. Forbes a work on 'Travels in Lycia.'

During the Crimean war the "Spitfire" was attached to the fleet in the Black Sea, and Commander Spratt's services were in constant requisition. Besides surveys of all the places required for the anchorage or operations of the fleet, some of them made under the enemy's fire, he planned the attacks on Kertch and Kinburn, and led the combined fleet to their position before the latter place. He repeatedly received the acknowledgements of the Commander-in-Chief, Admiral Sir E. Lyons, for his exertions on these and similar occasions, and was finally promoted for his services in January, 1855. He received the distinction of C.B. and of officer of the Legion of Honour at the close of the war.

On peace being proclaimed, Captain Spratt resumed his hydrographical surveys in the Archipelago, and continued them until the close of 1863.

Amongst papers and works published by Captain Spratt may be mentioned—

'A Report on the Geology of Malta and Gozo,' 1854.

'On the Movements of Teignmouth Bar,' 1856.

'Deep Soundings in the Mediterranean,' 1856–7.

'On the Comparative Conditions on the Different Mouths Branches of the Danube,' 1858.

'Investigation of the Effect of the Prevailing Wave Influence on the Nile Deposits,' 1859.

'On the Evidences of Rapid Silting in progress at Port Said,' 1870.

'Travels and Researches in Crete,' in two volumes, 1865.

This last work eminently illustrates his powers and versatility in different branches of scientific observation, and contains much valuable information on geological, archæological, and other subjects.

Captain Spratt became a Rear-Admiral on the retired list in 1872, and a Vice-Admiral in 1878. He was a Fellow of the Geological, Zoological, and Geographical Societies, and of the Society of Antiquaries, and was elected a Fellow of the Royal Society in 1856.

He was a Commissioner of Fisheries from 1866 to 1873, and held the appointment of Acting Conservator of the Mersey from 1879 to his death, which occurred on the 18th March, 1888.

W. J. W.

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