PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From January 10, 1878, *to June* 20, 1878.

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ERRATUM IN VOL. XXVI.

Page 511, for $y = Dd$, read $y = Dd^x$.

ERRATA IN VOL. XXVII.

Throughout Plate 4 the letter $\varkappa$ ought to match in size the letter α.

Page 78, lines 13 and 27, and page 79, line 16, *for* Greenwich-Kew *read* Kew-Greenwich.

Page 119, line 8 *for* hypochloric *read* hydrochloric.

OBITUARY NOTICES OF FELLOWS DECEASED.

Karl Ernst von Baer, one of the ten children of Magnus von Baer, was born at the family estate of Piep, in Esthonia, on the 28th of February, 1792. The first seven years of his life were spent in the house of his childless uncle, Karl von Baer, who seems to have had a wholesome dread of premature teaching, for when the young nephew returned in his eighth year to his father's house in order to be educated with his sisters, he did not so much as know his letters, though he had so far grown in mind as to be heartily ashamed of his backwardness. He speedily, however, made up for lost time, and in 1807 was entered at the High School of Reval, which he left in 1810 to join the University of Dorpat. Already drawn towards natural history, and especially towards botany, he became a student of medicine, and on the 29th of August, 1814, took his degree of doctor. The medical teaching at Dorpat was at that time far from satisfactory; from none of his masters save from Burdach, the physiologist, can Baer be said to have learnt anything, and when he became doctor he felt that his medical training had yet to begin. He accordingly moved to Vienna; but the more he tried to throw himself into purely medical studies, the more he felt that fate had not destined him for an active professional life. In the spring of 1815, happening to make with a friend a visit to the neighbouring Schneeberg, the Alpine flora he there found so powerfully revived his repressed love of botanical studies, that he determined at all risks to devote himself as far as possible to science. An accidental interview with von Martius determined him to place himself in the same summer under Döllinger, at Wurzburg, and the merit of having made von Baer an anatomist must be reckoned to the credit of that worthy teacher. Thither also came Baer's friend and countryman, Pander, to begin under Döllinger those studies on the development of the chick in which Baer was destined afterwards to take so great a part. At Wurzburg, however, Baer showed nothing more than a friendly interest in these investigations, and when in 1817 he moved to Königsberg to become prosector to Burdach, who had in 1814 been transferred thither from Dorpat, he threw himself with zeal into the ordinary anatomical and zoological studies. He had spent the previous winter in Berlin in order to fit himself more fully for his new post. His activity soon made itself felt; besides his purely anatomical lectures and demonstrations he found time to deliver an anthropological course, thus early indicating the tendencies to which he gave up so many of his later years, and in 1819 to establish a new

zoological museum. In the same year he was made Professor Extraordinarius, and, looking upon himself as now settled at Königsberg, married Fräulein von Medem. In 1822 he became Professor Ordinarius, and in 1826 succeeded Burdach as Professor of Anatomy and Director of the Anatomical Institute.

Ever since his arrival at Königsberg he had devoted himself with great energy to zootomy, and published several zoological and anatomical papers, notable among which is one containing the first exposition of his views on animal types. In 1818, however, he received from his friend Pander a copy of the monograph on the development of the chick, the study of which led him in the spring of 1819 to commence those embryological investigations with which his name will ever be connected. These were carried on with unwearied diligence for seven years, and it was not until 1827 that he made his results known in the form of a contribution to Burdach's Physiology. In his capacity as editor, however, Burdach saw fit to make so many changes and omissions as greatly to dissatisfy Baer, who determined to publish his researches in an independent form. This he did in 1828 under the title of "Ueber Entwickelungsgeschichte der Thiere, Beobachtung und Reflexion." Meanwhile, in the spring of 1827, he had made the important discovery of the existence of the mammalian ovary in the ovum.

The "Entwickelungsgeschichte" consists, in the first place, of a detailed chronological description of the development of the chick in the egg; and, in the second place, of general deductions and reflexions in the form of scholia and corollaries. Almost immediately afterwards Baer began a more general and systematic account of development under the title of "Vorlesungen über Zeugung und Entwicklung der organischen Körper." The first sheets of this were printed in 1829, but the work was then interrupted for several years, and was finally published without the author's permission in 1834 as the second part of the "Entwickelungsgeschichte."

In 1827 Baer received an invitation to become a member of the Academy of Sciences at St. Petersburgh. This he did not immediately accept; but in 1829, without resigning his professorship at Königsberg, made a journey to St. Petersburgh to see whether it would be possible for him to carry on his investigations in that city. He found, however, the hindrances to work so many and great, that he declined the call and returned to his old post, throwing himself with renewed vigour into his studies. A few years later these incessant labours began to tell upon his health. He grew enfeebled, his nights became sleepless, and he began to fear that his work was ended. Just at this juncture his elder brother, who was in possession of the family estate, died, and his sisters strongly pressed him to undertake the charge of the property which would eventually pass to his own son. These

seignorial duties would be perfectly compatible with a residence at St. Petersburgh, and accordingly in the summer of 1834 he bade good-bye to the scene of his labours and triumphs, and at the end of the year entered upon his new duties at St. Petersburgh as zoological member of the Academy of Sciences. He soon afterwards became one of the librarians of the Academy's library, and in 1841 was appointed Professor of Comparative Anatomy and Physiology in the Medico-Chirurgical Academy; this latter post, however, he resigned after a few years.

With his departure from Königsberg Baer's labours as an embryologist may be said to have closed; the rest of his life he devoted to anthropology, using that word in its widest sense. He took advantage of his position in the Academy to employ the resources of the Russian Empire in collecting materials for the study of the natural history of man. "Das Studium der Bildungsgeschichte des menschlichen Geschlechtes, die höchste aller Wissenschaften." Not content with setting out others on travels of inquiry, with drawing up instructions as to what they should observe and collect, with directing and superintending the publication of their results, he must needs himself undertake long voyages; and these were at intervals continued until he had reached an advanced age. In 1837 he journeyed, not without dangers and hardships, to Nova Zembla, and again in 1840 to the North Cape. In 1851 he began with the assistance of the Imperial Government a series of voyages in order to investigate the conditions of the fisheries of the Russian Empire. Besides shorter visits to the northern seas he spent nearly the whole of four years, from 1853 to 1857, in the neighbourhood of the Caspian Sea, returning to St. Petersburgh twice only during the interval. And in 1860 he again journeyed south, this time to the Sea of Azov. In all these wanderings he had in view the solution of problems not only of national economy but of the distribution and conditions of life of animals, plants, and man, of natural history, in fact, in its widest sense. It was chiefly in the interests of anthropology that in 1858–61 he visited the museums of the Continent and of London.

In 1864 he celebrated the jubilee of his doctorate, on which occasion was published, in a handsome volume, an account of his life, written by himself, at the request of the Ritterschaft of his native province. The same year, however, brought sorrow as well as joy, for it took from him his wife; and feeling himself now weighted with the burden of years, he resigned his post as ordinary member of the Academy, becoming an honorary member instead, and in 1866 removed to Dorpat, where he could live more quietly than in the imperial city, and where he was nearer to the family estate. But not even here did he altogether rest, devoting much time in these later years to an exposition of his general views, and especially to a criticism of

Darwinism, of which he remained to the end an opponent. His activity was at last broken by the increasing infirmities of old age, and on the 28th November, 1876, at five in the afternoon, he passed away. Only ten days before his death had he prepared for publication a communication to the "Archiv. für Anthropologie."

In 1854 he was elected a foreign member of the Society, and in 1867 he received the Copley medal.

Thus, as with so many other great men, Baer's intellectual life passed through two phases: an embryological phase to which the strength of his manhood was devoted, and an anthropological phase which absorbed the energy of his later years. And, as with other great men, it is by virtue of the earlier phase that his name is destined never to be forgotten. It is no disrespect to his later labours to say that they cannot, in importance, be for one moment compared to the work of those seven years which produced the "Entwickelungsgeschichte." When, in 1819, Baer put his hand to the plough, sixty years had passed since Wolff published his "Theoria Generationis," and during the whole of that long interval there had been no worthy embryological work, save Pander's tract, which, though admirable, was but a fragment; the skeleton which Wolff had put together had as yet to be clothed, and the views to which Wolff's genius had by instinct led him, needed still to be made sure by detailed proofs. When in 1828 Baer sent his sheets to the press, the story of the growth of the chick was in its main features complete. The varied manifold labours of embryologists since that day have filled up gaps and rounded off angles in Baer's edifice, but they have hardly touched the structure itself.

But it is almost the least of Baer's merits to have made known a mass of new facts touching the formation of the bird and other animals. Facts were to him useless, save as bricks wherewith to build up true views of nature. All through his slow toilsome study of the folds and twists, the thickenings and thinnings of the growing chick, he was supported by the sure hope that in the flitting shadows of embryonic forms was to be found the key to the laws of animal organisation. He wrote to Pander, "Gleich einem leuchtenden Strahle schoss es mir durch die Seele, dass der Typus im Bau der Wirbelthiere sich allmählig im Embryo ausbildet." It is not on account of the extent and accuracy of his work, for others, such as Rathke, have been as laborious and accurate, that Baer's inquiries mark an epoch. It is because he was the first, if not actually to see (for Merkel had some twenty years before laid hold of the same truth), at least clearly to enunciate, and indeed to demonstrate, the important law that the embryonic phases of the individual are tokens of the relations of kind and race.

Working as we do now in the light of the doctrine of natural selec-

tion, we can see that Baer stopped short when he ought to have gone on. He was satisfied when he had applied his law of "progress from the general to the special," so far as to make it clear that the manifold forms of animal life were educts of a few general types. He did not see, and to the end refused to admit, that these types were themselves the educts of an evolution. And the antagonism which in his later years he manifested towards modern views of evolution and recent embryology was based on the feeling that the new doctrine swept away the necessity for ultimate abstract types. Like the theory of epicycles in the old astronomy, Baer's views have succumbed before a simpler conception, of the truth of which the results of his own labours afford some of the strongest supports. Like the epicyclists, Baer largely prepared the way for the wider doctrine which has swallowed up his own.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

January 10, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

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

The following papers were read :—

I. "Magnetic Observations taken during the Transit of Venus Expedition to and from Kerguelen Island." By the Rev. S. J. PERRY, S.J., F.R.S. Received October 13, 1877.

In a previous paper, on the elements of terrestrial magnetism observed at Kerguelen, the reason is mentioned why it was not deemed advisable to take magnetic observations at sea during the voyage; the results, therefore, in this paper are entirely confined to the determinations of the Dip, Intensity, and Declination of the earth's magnetic force at the several places at which we landed. On our outward journey we had no opportunity of using our instruments, except at the Cape of Good Hope, but on our return we were much more fortunate, as H.M.S. "Volage" made a stay of at least two days at Point de Galle, at Bombay, Aden, Port Saïd, and Malta, and we utilised all these opportunities except the first. From Malta, the Rev. W. Sidgreaves and myself returned to England by Sicily, Italy, and France, and, taking our magnetic instruments with us, we were able to observe at Palermo, Naples, Rome, Florence, and Moncalieri. As we had previously made a complete survey of France in 1868 and 1869, we thought it too early to repeat the observations. The instrumental corrections and constants have already been given in the former paper on Kerguelen, it will, therefore, suffice to refer to that communication for any necessary details.

TABLE I.

Station.	Place of Observation.	Latitude.	Longitude.	Authority.
		° ′ ″	h. m. s.	
Cape Town	Observatory	33 56 3 S.	1 13 55·0 E	N.A.
Bombay	Colaba Observatory	18 54 25 N.	4 51 39·63	C. des Temps
	The College	18 56 7	4 51 43·63	„
Aden	Near Church at entrance to town	12 46 15	3 0 40·63	„
Port Saïd	Garden of Monastery	31 13 0	2 9 12	Atlas.
Malta	Garden of Governor's Palace, S. Antonio	35 53 50	0 58 4·63	C. des Temps
Palermo	Botanical Gardens	38 6 44	0 53 24·17	N.A.
Naples	Observatory	40 51 46·6	0 56 58·86	„
Rome	Observatory Coll. Rom.	41 53 52·2	0 49 54·7	„
Florence	Garden	43 46 4·1	0 45 1·46	„
Moncalieri	Garden of Palace	44 59 53	0 30 48·77	Observatory.

The Magnetic Dip.

The long sea voyage, with its great variation of temperature, was very trying for the delicate portions of the instruments, and as we found that No. 1 Needle had been slightly injured by rust, it was never used during our home journey. In the last column of Table II the daily mean values of the dip are entered, but it was only at Cape Town and at Bombay that observations could be taken on more than a single day. Needle No. 2 seems to read about 30″ higher than No. 3, but no correction has been applied to either, as the amount is considerably less than the usual errors of observations.

TABLE II.

Station.	Date.	Needle No. 1.	Needle No. 2.	Needle No. 3.	Daily Means.
		° ′ ″	° ′ ″	° ′ ″	° ′ ″
Cape Town	1874. July 27	−56 6 7	−55 57 47	− 56 3 30	− 56 2 28
	August 19	..	−56 4 0	− 56 0 30	− 56 2 15
Bombay Coll.	1875. April 7	..	..	20 4 16	..
„ Observatory				20 9 15	20 6 46
	„ 10	..	..	19 14 5	19 14 5
	„ 18	..	19 15 15	..	19 15 15
Aden	„ 27	..	5 28 9	..	5 28 9
Port Saïd	May 15	..	42 33 35	42 30 12	42 31 54
Malta	„ 24	..	..	51 36 42	51 36 42
Palermo	„ 29	..	54 17 47	54 18 28	54 18 8
Naples	June 3	..	57 0 13	57 0 33	57 0 23
Rome	„ 8	..	58 51 53	58 49 56	58 50 55
Florence	„ 19	..	60 13 0	..	60 13 0
Moncalieri	„ 21	..	62 55 1	62 56 23	62 55 42

As previous observations of the magnetic dip, taken at different times at several of the above stations, have been collected and reduced

to the epoch 1840–45, by Sir Edward Sabine (Cf. Phil. Trans., 1868), it may be well to compare the values for the two epochs, in order to ascertain the secular variation.

Table III.

Station.	1842–5.	1875.	Annual change.
Cape Town	− 53°·5	− 56° 2′ 22″	− 4′·69
Bombay.....	18 ·4	19 14 40	+ 1 56
Aden.....	5· 1	5 28 9	+ 0 63
Malta	53· 5	51 36 42	− 3 49
Naples (1845) ..	58° 43′ 45″	57 0 23	− 3 45
Rome (1847) ..	59 49 4	58 50 55	− 2 09
Florence (1844) .	61 54 30	60 13 0	− 3 28

The values of the dip at the Cape of Good Hope, from 1751 to 1851, have been collected in vol. i of the Cape Magnetic Observations, and these give, for the yearly increase in the south dip of the needle, − 6′·94 from 1751 to 1840, − 5′·45 from 1841 to 1846, and − 5′·58 from 1843 to 1851. The quantity in Table III shows that this decrease in the acceleration is still continuing.

The inclination observed at the College in Bombay is very extraordinary, and would at first sight appear to have been entered incorrectly. The result, however, agrees perfectly with the observed angles, and as two complete observations were taken with Needle No. 3, and these fairly agree, it may be supposed that the instrument was standing at the time over an unsuspected mass of iron capable of deflecting the suspended needle, through an angle of nearly a degree. The paper by Mr. C. Chambers, in the Phil. Trans. for last year, gives 19° 8′·5 as the most probable value of the dip for January 1st, 1871, at the Colaba Observatory, with a secular change of + 1′·9. This magnetic element would, therefore, appear to be altering more rapidly at present than its mean rate of change during the last 30 years. The angle observed in 1875 is very slightly in excess of that computed for the same epoch from the data of Mr. Chambers.

The low value of the secular change at Rome is quite exceptional, but if we compute this quantity for the epoch 1849 from the figures given in Sir E. Sabine's report, we again obtain − 2′·09, showing that, if there be an error, it is due to local causes.

The Magnetic Intensity.

At each station the horizontal component of the magnetic force was determined absolutely by vibrations and deflections, and the total intensity deduced from the horizontal force combined with the dip. The magnets employed throughout were the same as those used at Kerguelen, and no departure from the usual methods was ever found necessary.

TABLE IV.

Station.	Date.	Temperature.	Time of one vibration.	Log $m\,x$.	Distances of centres of magnets	Temperature.	Observed Deflection.	Log $\frac{m}{x}$.
		° Fahr.				°	° ′ ″	
Blackheath	1874. May 1	51·8	5·14838	0·25146	1·0	51·8	13 22 36	9·06691
					1·3	51·8	6 3 41	9·06700
Cape Town	July 28	55·5	4·90230	0·29368	1·0	60·0	12 4 52	9·02414
					1·3	59·3	5 28 55	9·02418
	August 8	62·7	4·90700	0·29347	1·0	61·5	12 4 53	9·02429
	„ 18	64·2	4·92380	0·29162	1·0	62·0	12 3 34	9·02356
					1·3	62·4	5 28 13	9·02355
Bombay College	1875. April 8	80·4	3·60950	0·56212				
	„ 9	..	..	..	1·0	82·8	6 23 28	8·75212
					1·3	83·4	2 54 40	8·75209
„ Observatory	„ 8	85·7	3·59850	0·56526				
		85·7	3·59883	0·56518				
	„ 10	85·8	3·59617	0·56553	1·0	85·1	6 20 59	8·74954
					1·3	85·1	2 53 39	8·75029
Aden	„ 27	89·6	3·70630	0·53974	1·0	89·9	6 43 39	8·77484
		89·7	3·70992	0·53830	1·3	89·1	3 3 50	8·77479
Port Saïd	May 15	75·8	4·03929	0·46381	1·0	74·8	8 2 51	8·85070
Malta	„ 24	83·1	4·31417	0·40758	1·0	80·8	9 7 9	8·90518
					1·3	80·3	4 8 51	8·90523
Palermo	„ 29	101·2	4·41500	0·38939	1·0	88·2	9 31 4	8·92453
Naples	June 3	81·7	4·51458	0·36785	1·0	77·2	10 0 50	8·94523
					1·3	77·6	4 32 46	8·94483
Rome	„ 8	87·2	4·62927	0·34652	1·0	81·1	10 26 17	8·96348
		87·2	4·61750	0·34873	1·3	79·2	4 44 48	8·96371
Florence	„ 19	72·2	4·67625	0·33654	1·0	72·2	10 49 20	8·97814
Moncalieri	„ 21	73·2	4·83625	0·30756	1·0	72·3	11 32 42	9·00587

TABLE V.

Station.	m = magnetic moment of magnet.	x = horizontal force.	y = vertical force.	z = total force.
Blackheath.....	0·45626	3·9107	9·5361	10·3068
Cape Town.....	0·45595	4·3128	6·4039	7·7208
	0·45591	4·3111	6·4005	7·7170
	0·45456	4·3056	6·3930	7·7077
Bombay College.....	0·45406	8·0354	2·9425	8·5572
„ Observatory....	0·45456	8·0849	2·8210	8·5629
	0·45452	8·0841	2·8207	8·5621
	0·45465	8·0869	2·8217	8·5650
Aden	0·45423	7·6289	0·7304	7·6638
	0·45380	7·6216	0·7297	7·6565
Port Saïd.....	0·45421	6·4056	5·8762	8·6926
Malta	0·45331	5·6389	7·1175	9·0805
Palermo	0·45390	5·4005	7·5161	9·2551
Naples.....	0·45337	5·1451	7·9247	9·4484
Rome	0·45192	4·9144	8·1435	9·5114
	0·45307	4·9269	8·1642	9·5356
Florence	0·45430	4·7775	8·3476	9·6181
Moncalieri	0·45364	4·4755	8·7566	9·8340

The tables of Sir Edward Sabine (Phil. Trans., 1868) furnish very few details from which to deduce the secular variation of the intensity at the above stations.

The total force at the Cape Observatory in 1845 was 7·58, and this combined with 7·7152, the value for 1874, will give + 0·0047 as the annual change. In the Cape Magnetic Observations, vol. i, we find + 0·0066 as the corresponding value for 1848.

Taking the mean of the observations at Bombay in 1845 and 1847, and also the mean for 1856 and 1867, we find 8·285 and 8·505 as the values of the intensity for the epochs 1846 and 1861–5. Comparing this last value with 8·5633 of 1875, gives a yearly change of + 0·0043, whilst the apparent change in 1854 was + 0·0142. Turning now to the Colaba results from 1867 to 1873 we see that, for January 1st, 1871, the absolute value of the total force was 8·5391, with an annual change of + 0·0064. This would make the magnetic intensity 8·5663 for April, 1875, which agrees well with the observed value. The mean annual acceleration from 1861 to 1875 is considerably less than the value deduced from the observations of the last few years.

The intensity, 9·27, observed by Caligny at Malta in 1839, combined with that of 1875, makes the annual variation — 0·0052.

At Rome, 9·6575 in 1849 gives also an annual variation of — 0·0052; but at Naples, 9·55 in 1845, makes the yearly change only — 0·0033.

The Magnetic Declination.

The observations, on which the determination of this element depend,

are necessarily affected by many possible sources of error, when the work has to be done in the course of a long and difficult journey. The carriage of the instruments by land and sea; the frequent packing and unpacking; the great variations of temperature; the shortness of time at each station, scarcely allowing sufficient attention to be paid to choice of locality or examination of instruments; all tell more against the magnetic declination than against the other elements of terrestrial magnetism.

As we were detained a considerable time at the Cape of Good Hope, it was thought advisable to examine thoroughly all the instruments, and to take a very complete set of observations at the Cape Observatory, in order to make this well-known station the point of departure for all our work in Kerguelen and elsewhere. It was fortunate that this could be done leisurely, as we found on examination that the declination magnet was useless, the bar not being magnetic, nor capable of retaining a sufficient amount of magnetism to secure accurate results. On July 30th the bar was magnetised by aid of large permanent magnets, and then subsequent declination observations led to most unsatisfactory results. On August the 19th a coil was made use of to magnetise the same bar, but two series of readings then taken with the needle, and with the Observatory magnet, shewed the utter untrustworthiness of our needle. As the Rev. W. Sidgreaves, though most kindly assisted by the Government Astronomer, Mr. E. J. Stone, was unable to magnetise satisfactorily our reversible declination needle, we were forced to substitute in its place the vibration magnet. The latter not being arranged for rapid reversal, the zero of the scale was very carefully determined both at the Cape Observatory and afterwards at Kerguelen, and the scale value accurately tested.

The method of observation usually adopted, was to fix the position of a well defined distant mark, by aid of sun or star transits, with the Simms' theodolite, and then to note the bearing of the magnetic needle with respect to the same mark with the Jones' unifilar. At Florence the confined space, and still more the badness of the weather, made it advisable to trust wholly to the unifilar, and therefore no fixed mark was used, but the position of the magnet was referred directly to that of the sun, observed by aid of the mirror attached to the instrument. The same method was used at Moncalieri, and the corrections for the displacement of the mirror, deduced from the latter observations, served to correct the single reading at Florence. The necessary correction for any error in the mirror is explained in a former paper on the magnetic survey of the west of France (Phil. Trans. 1869, p. 43).

The chronometer made use of throughout the whole series of observations was a good pocket instrument from Greenwich Observatory, Maurice 6144. It was compared on every possible occasion with the

standard clocks of fixed observatories, and at other times with the chronometers kept on board H.M.S. "Volage." No direct observations were taken with the transit theodolite to check the error and rate of the chronometer, but these depend entirely on the comparisons. It will not be necessary to give in full the details of each comparison, but all the requisite information is contained in the following table.

TABLE VI.

Station.	1875.	Chronometer time.	Fast on G. M. T.	Daily rate.
		h. m. s.	h. m. s.	s.
Bombay Observatory	April 8	4 52 33 0 p.m.	6 58 7·23	
	„ 10	4 46 40·6	6 58 14·23	+ 3·50
	„ 12	10 37 0	6 58 22·43	+ 3·64
Aden, H.M.S. Volage	„ 27	3 0 0	6 59 16·90	+ 3·56
		9 0 0	6 59 17·88	+ 3·92
Port Saïd.....	May 15	5 1 0	7 0 33·20	+ 4·31
Malta	„ 24	5 38 0	7 1 11·00	+ 4·20
Palermo Observatory	„ 29	Noon.	0 50 17·74	
Naples.....	June 3	4 0 0	0 50 29·53	+ 2·30
Rome	„ 8	4 0 0	0 50 56·91	+ 5·48
Florence	„ 19	2 0 0	0 51 18·26	+ 1·94
Moncalieri	„ 20	7 6 0	0 50 0·67	
	„ 21	9 46 0	0 49 59·27	− 1·24

When the journey through Sicily was commenced, the chronometer was altered to approximate local times. The great change in the daily rate between Naples and Rome will not affect the results at Rome, as the declination was measured from the standard mark at the Roman College Observatory. Before reaching Moncalieri the chronometer appears to have met with an accident which entirely altered its rate, but the double comparison at this station renders this change of less moment.

TABLE VII.

Station.	Date.	Chronometer.	Circle reading for sun or star.	Circle reading for mark.	Circle reading for magnet.	Scale correction.	Declination.
		h. m. s.	° ′ ″	° ′ ″	° ′ ″	′ ″	° ′ ″
Cape Observatory	1874. Aug. 20	..	..	162 38 35	132 28 55	−22 45	* 30 0 34 W.
		1 15 0 p.m.	..	162 38 25	132 40 50	−32 36	29 58 20
		1 42 0	..	,, ,, ,,	132 8 40	0 0	* 29 57 54
		1 47 0	..	,, ,, ,,	132 42 30	−22 17	29 56 21
		1 55 0	..	,, ,, ,,	132 7 10	0 0	* 29 59 24
		2 2 0	..	,, ,, ,,	132 42 25	−32 17	29 56 26
		2 9 0	..	,, ,, ,,	132 7 0	0 0	* 29 59 34
	,, 22	3 55 0	..	233 55 50	203 56 20	−32 7	29 59 46
Bombay Observatory....	1875. April 8	3 30 0	..	221 51 40	222 54 40	−32 21	0 57 5 E.
		5 5 0	..	226 8 20	227 10 30	,,	0 56 15
		5 13 0	..	226 8 35	227 9 55	,,	0 55 25
		5 30 0	..	224 42 5	225 43 15	,,	0 55 15
,, College.....	,, 12	11 3 23·5	141 30 7·5	262 28 30	..	..	0 57 16·5
				203 36 45	159 29 0	,,	
		11 14 26·5	169 57 37·5	262 28 15	..	.	0 53 55·5
Aden	,, 27	9 6 22·2	111 45 0	117 45 15	..	..	2 19 39·4 W.
				230 14 25	126 21 20	,,	

Station.	Date.	Chronometer.	Circle reading for sun or star.	Circle reading for mark.	Circle reading for magnet.	Scale correction.	Declination.
		h. m. s.	° ′ ″	° ′ ″	° ′ ″	′ ″	° ′ ″
Port Saïd.....	May 15	9 51 40·3 p.m.	129 6 22·5	108 23 15	..	..	5 9 48·8 W.
				95 41 20	192 4 25	,,	
Malta	,, 24	10 37 10·1	197 50 15·0	89 59 45	..	..	12 8 40·2 ,,
				177 4 0	178 25 10	,,	
		10 49 22·7	199 28 30·0	89 59 45	..	..	12 8 8·9
Palermo	,, 29	10 13 12·5 a.m.	220 56 37·5	160 2 0	..	..	11 15 20·2 ,,
				257 13 30	187 15 0	,,	
		10 21 35·7	223 59 22·5	160 2 0	..	..	11 15 45·6
Naples.....	June 3	3 44 23·2 p.m.	182 56 30·0	201 20 7·5	..	..	11 31 57·8 ,,
				109 27 20	172 17 30	,,	
		3 51 58·4	184 12 0·0	201 20 7·5	..	..	11 31 5·9
		3 59 58·0	185 30 52·5	,, ,, ,,	..	..	11 31 9·8
				109 27 55	172 18 50	−32 1	
		4 13 17·3	187 39 22·5	201 20 7·5	..	..	11 31 15·9
Rome	8	11 40 0 a.m.		192 20 25	202 2 20	−32 21	12 17 20 ,,
				192 19 40	203 3 10	,,	12 15 45
Florence.....	,, 19	1 55 39·4 p.m.	212 15 25	..	143 48 0	,,	13 22 26·2 ,,
Moncalieri	,, 21	11 1 1·8 a.m.	159 46 25	..	191 1 10	,,	13 45 15·9 ,,
		11 7 25·3	162 32 40	..	..	,,	13 45 51·9
		11 13 25·5	165 14 50	..	..	,,	13 45 15·9

At the Observatories of the Cape, of Colaba at Bombay, and of Rome, it was only necessary to enter the approximate local mean time under the heading, "Chronometer," as the declination at these stations was measured from the standard marks. The bearing of these magnetic marks is respectively 0° 31′ 51″, 0° 26′ 26″, and 21° 26′ 54″ E. of the Astronomical North Point. At the remaining stations the sun's azimuth was computed for the hour angle of the sun given by the chronometer, except at the college of Bombay, where it was found more convenient to observe the stars Aldebaran and Capella, instead of the sun. The observations marked with an asterisk were taken with the needle of the Cape Observatory. The computed altitudes of the sun for the three observations at Moncalieri, were 63° 3′ 33″·60, 63° 48′ 57″·69, and 64° 29′ 17″·78. This difference of altitude being so slight makes the correction for the perpendicularity of the mirror much less reliable than it might otherwise have been.

The declination observations at the Cape of Good Hope date back as far as 1600, and the mean yearly amount of the secular variation for the first 240 years was found to be + 7′·66 W. It then diminished, and was + 0′·49 from 1841 to 1846, +1′·41 from 1843 to 1848, and 2′·16 from 1846 to 1850. The renewed acceleration in the annual variation seems to have lasted but for a short period, as a comparison of the mean value for 1874, with 29° 14′·62, the mean for 1848, gives only 1′·68 as the annual change.

From observations of this magnetic element at Bombay in 1845, 1856, 1867, and 1875, we notice a gradual easterly motion of the needle, it being at the above dates 0° 13′ W., 0° 19′ E., 0° 42′ E., and 0° 56′ E., shewing a decreasing yearly change of 2′·91, 2′·09, and 1′·75. The amount of the mean annual variation given by the series of monthly observations at the Colaba Observatory from 1867 to 1873 is 1′·77, which may be considered identical with the above.

At Aden the heat of the sun was so great that it was imprudent to remain long near the theodolite whilst taking the solar observation, the declination was therefore obtained under very disadvantageous circumstances, and consequently less weight can be attached to the result. This is apparent also from the abnormal increase in the secular acceleration. Thus the declination which was 5° 2′ W. in 1834, became 4° 15′ W. in 1857, the annual variation being, therefore, −2′·04; whilst in 1875 the angle observed was 2° 19′ 39″ W., which increases the yearly change to −6′·41.

The declination in 1875 at Malta, combined with the value 15° 20′ W., obtained in 1834, gives a yearly change of −4′·76; and the angle 13° 49′ measured at Rome in 1852–5, along with the value found in 1875, makes the annual variation −5′·95. For the remaining stations I am unacquainted with any published values of this element of the earth's magnetic force, but the careful series of

measures taken at Moncalieri, by P. Denza, will most probably appear shortly in the publications of that Observatory.

In conclusion I will subjoin in a single table the mean results for all the elements of terrestrial magnetism at the different stations.

Table VIII.

Station.	Dip.	Annual amount of Sec. var.	Horizontal force.	Total force.	Annual amount of Sec. var.	Declination.	Annual amount of Sec. var.
	° ′ ″	′				° ′ ″	′
Cape Town.	−56 2 22	−4·69	4·3098	7·7152	+0·0047	29 58 33 W.	+1·68
Colaba	+19 14 40	+1·56	8·0853	8·5633	+0·0043	0 56 0 E.	+1·75
Bombay....	+20 6 46	..	8·0354	8·5572	..	0 55 36 E.	
Aden.....	+ 5 28 9	+0·68	7·6253	7·6602	..	2 19 39 W.	
Port Saïd ..	+42 31 54	..	6·4056	8·6926	..	5 9 49 „	
Malta.....	+51 36 42	−3·49	5·6389	9·0805	−0·0052	12 8 25 „	−4·76
Palermo....	+54 18 8	..	5·4005	9·2551	..	11 15 33 „	
Naples	+57 0 23	−3·45	5·1451	9·4484	−0·0033	11 31 22 „	
Rome.....	+58 50 5[illegible]	−2·09	4·9207	9·5235	−0·0052	12 16 33 „	−5·95
Florence ..	+60 13 0	−3·28	4·7775	9·6181	..	13 22 26 „	
Moncalieri .	+62 55 42	..	4·4755	9·8340	..	13 45 28 „	

II. "On the Limits to the Order and Degree of the Fundamental Invariants of Binary Quantics." By J. J. Sylvester, M.A., LL.D., F.R.S., Professor in the Johns Hopkins University, Baltimore, U.S. Received December 26, 1877.

The developments which I have recently given to Professor Cayley's second method of dealing with invariants (the first method being that which has been exclusively used by Professor Gordan), has led me through the theory of the Canonical Generating Fraction to the following results, showing that the degree and order of the fundamental invariants and covariants to a quantic or system of quantics are subject to algebraical limits of a very simple kind, and I think it right that these results should not be withheld from the knowlege of those who are pursuing another and, as it seems to me, much more arduous and less promising direction of inquiry into the same subject.

By order I mean the dimensions of a derived form in the coefficients of its primitive (Clebsch and Gordan's *grad*), and by degree the dimensions in the variables (Clebsch and Gordan's *ordnung*).

First as to degree.

If there be a system of n, n' n'' . . odd degreed quantics and ν, ν', . . &c., even ones, then (with the exception of the case when the system reduces to a single linear function or a single quadratic) the degree of

any irreducible covariant to the system has for a superior limit $\Sigma\left(\frac{n^2+1}{2}\right)+\Sigma\left(\frac{\nu^2}{2}\right)-2$.

Thus, *ex. gr.*, where there is but one quantic, the limit is $\frac{n^2-3}{2}$ or $\frac{\nu^2-4}{2}$, according as the degree is n odd or ν even.

Secondly, as to order.

As the expressions become somewhat complicated when there are several quantics, I shall confine myself to a statement applicable to a single quantic, distinguishing between the three cases when n (its degree) is evenly even, oddly even, and odd.

A. When n contains 4, the superior limits for the order of the invariants and covariants respectively are for the former $\frac{(n+1)(n-4)}{2}$, and for the latter $\frac{(n+2)(n-3)}{2}$.

B. When n is even, but not divisible by 4, and is greater than 2, the limits for the two species are $\frac{3n^2-6n-12}{4}$ and $\frac{(n+2)(3n-8)}{4}$ respectively.

C. When n is any odd number greater than 3, the order of the invariants has for its limit $\frac{3}{2}(n+1)(n-3)$, and when it is any odd number greater than unity, the order of the covariants has for its limit $\frac{3n^2-4n-9}{2}$.

Further investigations will, I have good reason to believe, lead to considerably lower limits than those given for cases *B* and *C*.

Although morally certain the three formulæ *A*, *B*, *C* cannot be considered at present apodictically established, the formula respecting the limit to *degree* may, I believe, be regarded as admitting of a complete demonstration. There exists, however, a superior limit to the orders of the fundamental invariants or covariants, which may be regarded as subject to direct demonstration even in our present state of knowledge; this when n is even is n^2-2n-3 for invariants, and n^2-n-4 for covariants; and when n is odd, the corresponding limits are $2n^2-3n-5$ for invariants, and $2n^2-2n-5$ for covariants. But I have no moral doubt whatever of the validity of the formulæ *B* and *C* as they stand, and next to none of the validity of formula *A*.

III. "On the Structure and Development of the Skull in the Common Snake (Tropidonotus natrix). By W. K. Parker, F.R.S. Received October 15, 1877.

(Abstract.)

For several years past I have taken every opportunity to work at the skull of the common snake, but sufficient materials for completing a memoir upon it have not turned up until lately.

Last year, however, at the request of my friend Mr. P. H. Carpenter, Dr. Max Braun, of Würzburg, kindly sent me about fifty early embryos of reptiles; these were of four kinds, namely, of the common snake, the blind worm, the nimble lizard, and the gecko.

These invaluable embryos, added to what I have for many years been collecting, will enable me to bring before the Society a yearly tribute of a paper on the skull of these instructive types.

Lying at the very base of the *gill-less* Vertebrata, and possessing a skull at once the simplest, and yet the most curiously specialized, the snake is a type well worth careful study. I have found it so in my own division of work.

My guide in this piece of work has been Rathke, whose observations on the early stages of the skull appeared first, in translation, in this country, in Professor Huxley's "Croonian Lecture."

For many years past I have known that the key to the meaning of the skull in the whole series of the "Sauropsida"—reptiles and birds in one huge group—was to be found in that of the serpent. I have freely used it as such, and my nomenclature of the parts of the growing bird's skull is based on that of the snake's, although, until now, I have not been able to publish more than a mere abstract upon its characters.

For convenience' sake, I have divided my subject into *seven stages*, the first of these being illustrated by embryos with a very delicate vesicular head, bent down upon the neck, and whose entire length, supposing them to be uncoiled, was barely three-quarters of an inch in length. The last stage is the *adult*, whose skull, once interpreted, will greatly help in the interpretation of all the skulls that rise above and around it.

As to the finished skull, it is easy to judge beforehand that the cranial part must be a very solid and relatively small *box*, and that the facial part must be free and elastic to the utmost degree.

All the morphological specializations that take place in the head of the embryo steadily lead to this result; but the superstructure is marvellously unlike the foundations that were at first laid.

As to the general embryological study of the snake's embryo, it may be remarked that it is almost exactly like that of the bird's.

Comparing my own observations on this low type with the results given of the study of the chick in Foster and Balfour's excellent work, I find that few paragraphs in it would need any material alteration, and that the figures would mostly serve very accurately if in that work the word *chick* were to be exchanged for that of *snake* embryo.

The development of the vesicles of the brain, the organs of special sense, the rudiments of the cranium and face—those things that come across my path, to say nothing of the rest of the growing germ, all are developed similarly in the snake, below, and in the bird, above.

Like all the Vertebrata above the fishes and amphibia ("Ichthyopsida"), the embryo of the snake has the folds and clefts behind the mouth few and small; yet there is no change of pattern, and the rudiments of three pairs of gill-arches with their clefts can be made out.

But no embryo I have studied shows more clearly that all the folds that lie about the mouth, before and behind, as well as those that form the mouth itself, belong to one category.

Whilst the brain is merely a row of three vesicles, and the rudiments of the sense-capsules are three folds, or unclosed vesicles, the sides of the head grow downwards as a series of folds parted by deep notches or clefts.

These *clefts* are the "naso-lacrymal" in front of the rudimentary mouth; the mouth-clefts (right and left); the "tympano-eustachian;" and the three "hyo-branchial" clefts behind that.

The first *folds* lie under the fore-brain; these are the "fronto-nasal processes" or flaps; the second are the "maxillo-palatine;" the third, the mandibular; the fourth, the hyoid; and the fifth, sixth, and seventh are rudimentary branchials, that never give off branchial filaments or plicæ.

The *cavity* of the mouth is formed by the absorption of the mesoblast that lay in the angle of the head—bent double upon itself—just below the end of the notochord.

The walls of the mouth are formed by the maxillo-palatine rudiments in front, and the mandibular rudiments behind; below, the *floor* of the face is suppressed; at the sides, the right and left cleft form the right and left angle of the mouth.

The visceral folds develop but little cartilage; an outgrowth from the anterior ends of the trabeculæ grows downwards and backwards into the fronto-nasal process. It represents two visceral rudiments, the axis of the pre-maxillary arch; but this flap is single.

The maxillopalatine folds develop no cartilage, but become rich in "parosteal" bones.

Each mandible develops a large quadrate and a long articulo-meckelian cartilage.

All the cartilage seen on each side behind the lower jaw is a small

"epi-hyal" ("hyo-mandibular"), which becomes the stem of the "columella auris;" but the "branchial" folds develop no cartilage in them, and soon are lost in the sides of the neck.

There is but little cartilage developed in the cranium. The ear-capsules are large, long, and become solid cartilage before they ossify, and the simple nasal roofs chondrify and never ossify.

There is a large cartilaginous occipital ring or arch, but the sphenoidal regions, which have exceptionally autogenous alæ, are very feeble and small.

The trabeculæ, which are manifestly mere outgrowths of the para-choidal tracts, develop into long terete rods of cartilage, but do not ossify at all in front of the pituitary space.

They coalesce in the internasal region, and coalescing also with the descending laminæ of each nasal roof-cartilage, they form a low septum nasi.

There are two pairs of "extra-visceral," or *labial*, cartilages.

The "epi-hyal" rudiment coalesces during chondrification with the stapedial plate; it gives off a small scale from its postero-superior edge. This is a minute rudiment of the "stylo-hyal" element.

Both in the cranium and face there is a copious growth of very perfect bone developed in the sub-cutaneous stroma. In the cranium the frontals quite, and the parietals almost, meet below. Thus the trabeculæ are excluded from taking part in the formation of the front part of the cranial cavity.

These rods lie in the grooved sides of the "parasphenoid," which has no basi-temporal part, as in the bird, but becomes ankylosed to the double, *ectosteal* basi-sphenoid.

These are some of the most striking points in the growing snake's skull; for the rest I must refer to the main paper. I may, however, make a few remarks upon the "new things" to be seen in this low sauropsidan skull.

The occipital condyles are confluent at the mid-line, so as to form a rounded single surface, with a dimpled middle part.

The auditory capsule acquires three bony centres that do not unite with each other, but with the nearest bones of the cranium.

Thus the prootic coalesces with the alisphenoid, the epiotic with the superoccipital, and the opisthotic with the exoccipital.

The columella and stapes are one bone, not distinct as in the batrachia. The union takes place very early.

The mandible opens very far back, behind the occiput, and as the quadrate passes over the stylo-hyal, the two become ankylosed together.

For the palate a new bone appears, namely the "trans-palatine;" but the meso-pterygoid is not distinct from the pterygoid in the ophidia. It occurs in *Anguis fragilis*.

The little "pre-orbitals" that in the Amphibia form a floor to the

narial passage, and are termed by me the "septo-maxillaries," attain their highest development in serpents and lizards. I do not find them in the tortoises and crocodiles, but they re-appear—very small—in carinate birds, in connection, as in the snakes, with the vomers and labial cartilages.

In the mandible the upper part of Meckel's cartilage ossifies as the "articulare," and now we get the full number of the splints, namely, *five*—the dentary, sphenial, coronoid, angular, and surangular. The pier is a single bone, the quadrate. These six pairs of bones forming the free mandible occur also in the lizards, crocodiles, and in most birds In tortoises they are fewer, and in gallinaceous birds I have failed to find the coronoid piece at any stage.

The tongue of the snake shows no cartilage in its structure; neither cerato-hyal, basi-hyal, or thyro-hyal, have appeared in any of my dissections.

IV. "Observations on the Nervous System of Aurelia aurita." By Edward Albert Schäfer, Assistant Professor of Physiology in University College, London. Communicated by W. Sharpey, M.D., LL.D., F.R.S. Received October 31, 1877.

(Abstract.)

The author describes the nervous system of Aurelia as consisting, in addition to the lithocysts and certain tracts of specially modified epithelium in their neighbourhood, of an interlacement of nerve-fibres covering the whole of the under surface of the umbrella and lying between the ectodermal epithelium and the muscular sheet. Each nerve-fibre presents near the middle of its course a nucleated enlargement in the shape of a bipolar nerve-cell, which is thus interpolated in the course of the fibre. With regard to these nerve-fibres it is remarked—firstly, that they are of limited length, seldom exceeding four millimetres; secondly, that they never come into actual continuity with other fibres, although they frequently run closely parallel for a certain distance, and often form extremely intricate interlacements by the coming together of a number of fibres. The fibres occasionally branch. They are described as ending generally by finely-tapered extremities, which are in close contact with the substance of the muscular fibres, but sometimes the termination of the nerve is dilated into a flattened nucleated expansion, probably a primitive form of motorial end plate.

The structure and relations of the lithocysts are then treated of. The lithocyst is described as consisting of an ectodermic covering and an endodermic core, the two being nearly everywhere separated by a

thin layer of the jelly-like mesoderm. The ectodermic covering consists, except over the free end where the cells are simple and flattened, of long columnar, ciliated cells, the fixed ends branching into delicate fibres, which form a stratum underneath the epithelium. A similar condition of the ectoderm is described as met with in two depressions of the surface, one being situate above, and the other below, the lithocyst; and the resemblance which the elongated epithelium cells with the subjacent granular-looking, but in reality fibrous stratum, exhibits to the developing central nervous structures in the vertebrate embryo is pointed out. These parts, in fact, probably represent the first beginnings—phylogenetically—of a central nervous system. Some of the cells of the ectodermic covering of the lithocyst are pigmented, and these cells are provided each with an excessively long and fine (sensorial) filament instead of with vibratile cilia.

The endodermic core of the lithocyst consists of a prolongation from the nutritive canal of the margin, which at the terminal part of the lithocyst is continued as a solid projection, the cells of which contain calcareous crystals or otoliths.

A small lithocyst is further described, in which the otolithic prolongation of the nutritive canal penetrated the ectodermic covering, and projected freely into the surrounding medium.

Reference is made to the observations of Haeckel upon the nervous system of Geryonia and Cunina, and to a recently published preliminary notice by Oscar and Richard Hertwig, of the structure of the nerve-tracts and sense-organs in various other genera of Medusæ.

January 17, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

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

The following Papers were read:—

I. "On Cobra Poison." By ALEXANDER PEDLER, F.C.S., Professor of Chemistry in the Presidency College, Calcutta. Communicated by Dr. FRANKLAND, F.R.S., Professor of Chemistry in the Royal School of Mines. Received November 1, 1877.

On my arrival in India my attention was strongly directed to the enormous number of deaths, annually resulting from the bites of poisonous snakes. Statistics of the deaths from such causes are

collected by the various Provincial Governments of India, but as might be expected the difficulty of obtaining correct returns is very great, and it is certain that the number registered is far short of the real mortality resulting from this cause. Even with such incorrect returns, however, as many as 11,416 deaths in India from the bites of poisonous snakes were reported during a single year,* and it is probably within the mark to say that in the whole of India the annual number of deaths from snake bite is not less than 15,000.

Experiments without number have been made with a view to discover an antidote for snake virus; but although many so-called remedies have been reported, it has been found on closer examination, that none of them have been of the slightest efficacy either in modifying the action of the poison, or in any way tending to save life. The nearest approach to success among these unsuccessful remedies appears to have been the system of artificial respiration, suggested by Sir J. Fayrer, and extensively experimented with by the Snake Poison Commission at Calcutta, whose report was published in 1874,† and who found that, although life might be prolonged by the use of artificial respiration after poisoning by snake virus, in no case was there any ultimate hope of saving life by this means.

So far as I am acquainted with their results, the researches of other chemists have been hitherto unsuccessful either in elucidating the nature of the poison, or in ascribing any composition to the active principle which must be present. This want of success is the only justification of my bringing the result of my own researches before the Royal Society at all; while the great practical importance of a satisfactory solution of the problem will, I hope, be accepted as my apology for laying my work before them in its present incomplete form.

Of all the snakes in India the cobra (*Naja tripudians*) is the cause of the largest number of deaths, and the experiments here described were all made with cobra poison, extracted from the snakes in my own presence, by the usual method as described in published memoirs on the subject of snake poisoning. As may be imagined, the supply of cobra poison which is obtainable is very small, each snake yielding only from one to three grains of solid poison; so that, in all my experiments, I have been continually hampered with the extreme difficulty of procuring a sufficient amount of the poison to work with on a satisfactory scale. In submitting my results to the Society, I must at the outset state that it is possible that after further experiments I may have to modify some of the results here given.

The liquid poison, which, when extracted from the cobra, has a slightly alkaline reaction, was found to have a specific gravity of 1·095

* 1869. *See* Fayrer, Proc. Roy. Soc., xxi, p. 360.

† Report on Indian and Australian Snake Poisoning. Bengal Secretariat Press, Calcutta.

at 23° C., the experiment being made on the average poison collected from 16 snakes on November 26th, 1874. Whether evaporated in dry air, or in vacuo, or at 100° C., the fresh poison gave nearly the same amount of solid residue, the mean quantity being 28·28 per cent., the poison extracted from one batch of snakes in the rainy season (July) giving 27·74 per cent., and the poison from 16 snakes on November 24th, after the cessation of the rains, giving 28·82 per cent.

As these samples were taken the first during the hot and moist rainy season, when these snakes are most active, and yield the largest quantity of poison, and the second during the cold season, when they are very sluggish, and yield only small quantities of virus, it may be hypothesised that throughout the year there is very little change in the actual amount of solid present in the liquid poison shed by the cobra.

I have tested the fresh liquid poison with polarized light, but found that it was optically inactive.

With reference to the action of time on cobra virus, my own conclusions do not altogether agree with results previously obtained; thus, in the report of the Snake Commission above referred to, it is stated that the poison undergoes no change by being kept; but, from my expeperience, I have found, that although the poison will not alter if kept for two or three months only, yet if it be kept for a year or eighteen months, it alters in its properties very materially, becoming insoluble, and indeed losing to a considerable extent its poisonous action. In fact, I gradually collected a considerable quantity of the poison, with a view to experiment on a larger scale; but, on commencing work, I found the poison almost inert, and perfectly valueless for any scientific investigation on the subject, and in this way lost much time, and a very considerable amount of material extremely difficult to procure or replace.

The poison dried *in vacuo* over sulphuric acid was analysed, and the mean result was found to be:—

Carbon	49·32 per cent.
Hydrogen	7·01 ,,
Nitrogen.....	17·39 ,,
Ash.....	6·68 ,,
Oxygen and sulphur by difference	19·60 ,,
	100·00

If we regard the ash as being foreign to the organic poison, the proportion of the other elements will then become*—

* A part of these results was submitted by me to the Snake Poison Commission, and published by them at page 42 of their report.

Carbon	52·87
Hydrogen	7·51
Nitrogen	18·29
Oxygen and sulphur	21·33
	100·00

The composition of crude cobra poison then approaches rather closely to the known composition of the various kinds of albumin, but the amount of nitrogen in the virus is rather in excess of that contained in egg albumin.

On testing the cobra poison qualitatively, I found that it contained a considerable proportion of a substance which gave all the tests of ordinary animal albumin,* and the amount of this substance was estimated by precipitation with strong alcohol, 0·5403 gram of fresh cobra virus treated in this way gave ·0935 gram dry albumin, and ·0584 gram of a substance, which was gummy and amorphous in appearance. The albumin on testing was found to be slightly poisonous, and from its physiological action I concluded that it could not contain more than from one-fiftieth to one-hundredth part its weight of the poisonous principle. The portion soluble in alcohol was *excessively* poisonous. It appears, then, that in the poison obtained from the cobra there is about 17·3 per cent. of albuminous matter, and 10·9 per cent. of poisonous matter only, or that about 60 per cent. of the solid cobra poison is of an albuminous nature, and only 40 per cent. at the most is pure poison. I have endeavoured, by the use of solvents, both chemical and mechanical, to separate out some crystallizable principle from the poison; but although I have used a great variety of liquids, I have not yet succeeded in obtaining anything but indications of crystallizable matter, and I have now given up all hope of separating any distinct principle from the poison by this method. Dialysis through parchment paper was tried, in the hope of separating some principle, and about half a gram of the solid poison, dissolved in water, was allowed to dialyse for five days; at the end of that time the liquid from the interior of the dialyser, when evaporated, gave an amorphous gummy mass, which had all the physiological properties of cobra poison; the liquid, outside the dialyser, when evaporated, gave a residue the greatest part of which was like gum, but a few apparent crystals could be detected. This was tested physiologically, and was, if anything, rather more poisonous than the ordinary virus, and I was,

* Dr. Armstrong (Proc. Roy. Soc., xxi, 363) had already noticed that the liquid poison when kept contains albuminous matter, but the poison that he examined had already begun to undergo decomposition. The chemical composition of the poison, as determined by Dr. Armstrong, shows a much smaller proportion of carbon and nitrogen than that found by me in the fresh poison. As I have mentioned, the poison does not keep well either as a liquid or a solid, hence the disagreement of the results.

therefore, unable to obtain any satisfactory result by the aid of dialysis.

I have satisfied myself by numerous experiments that the reported remedy for snake bite, ammonia, has no action chemically upon the poison, but that, if anything, the addition of this substance to the poison before hypodermic injection rather increases the rapidity of the action of the poison, and in no instance does it induce any modification of the poisonous action.

Whilst experimenting with the poison, it struck me that it was possible that, as in the case of many organic poisons, the nitrogen might be in the triad condition, and that it might be possible to modify the poison by the action of ethylic iodide or other substances, as has been done in the case of some alkaloids. Ethylic iodide was digested with the poison, and the residue, which had increased in weight, when physiologically tested, was found to be very much less active than usual, although still poisonous; the sample taking about five times as long to kill as fresh cobra poison. Another sample of poison was treated with hydrochloric acid, and evaporated spontaneously, and the residue was very much less active than ordinary poison, and I have found that even the addition of a small quantity of this acid to a solution of the poison before injection materially retards the poisonous action; for a quantity (3 milligrams) of poison, which under ordinary circumstances would kill a chicken in about half-an-hour or forty minutes, after the addition of 0·2 c.c. of hydrochloric acid took 1 hour 6 minutes to produce the same effect. The poison treated with hydrochloric acid, when evaporated very slowly *in vacuo*, shows distinct traces of crystals; but there still remains a large quantity of amorphous soluble matter, from which, when working on a small scale, as from the nature of the subject I was obliged to do, the crystals cannot be separated.

As hydrochloric acid has some apparent chemical action on cobra poison, I tried the effect of a number of chemical compounds on this substance, and by a natural sequence of ideas, having found that hydrochloric acid had a tendency to act in the required direction, amongst them I employed platinum tetrachloride, and found that this yielded excellent results, and gave indications of what might be called a chemical antidote.

The following is a typical experiment. A quantity of fresh cobra poison was precipitated with alcohol, and the precipitate of albuminous matter well washed, then dissolved and tested, and found to have very slight poisonous action only, the alcoholic filtrate was then acidified with hydrochloric acid, and a solution of platinum tetrachloride added, when a small quantity of a yellow amorphous precipitate was formed; the solution on evaporation *in vacuo* yielded a semi-crystalline residue, from which the excess of the platinum salt was removed by washing with dilute alcohol; 0·1 gram of the solid platinum compound, which

appeared to be very insoluble in water, was administered internally to a chicken, and had no poisonous action; and the solution containing the excess of platinum salt when injected into a chicken had no action whatever.

Here, then, was a case in which, apparently, the poisonous action of the virus had been destroyed or neutralized, for the albuminous precipitate certainly did not contain more than from one-fiftieth to one-hundredth part of its weight of poisonous material.

I have endeavoured to obtain a sufficient quantity of the poison to give me enough of the platinum compound for the determination of its composition, but up to the present time I have only succeeded in making one complete analysis; the numbers which I have obtained are as follows:—

·2583 gram of platinum compound yielded ·3065 gram CO_2, ·0996 gram H_2O and ·0418 gram platinum. From this it follows that C = 33·42 per cent., H = 4·28 per cent., Pt. = 16·18 per cent. Also ·0946 gram platinum compound yielded ·1462 gram ammonia platinum chloride, therefore, N = 9·69 per cent. Unfortunately, I have been unable to determine the amount of chlorine, but if the compound has the ordinary composition of Cl_6 for Pt., the amount of chlorine present would be 17·42 per cent., and we should thus get:—

By Experiment.		Calculated for formula $PtCl_4(C_{17}H_{25}N_4O_7HCl)_2$	
Platinum	16·18 per cent.	16·36 per cent.	
Chlorine by calculation	17·42 „	17·65 „	
Carbon	33·42 „	33·81 „	
Hydrogen	4·28 „	4·31 „	
Nitrogen	9·69 „	9·29 „	
*Oxygen by difference	19·01 „	18·58 „	

Judging from such insufficient data, it would be impossible to assign with certainty any formula to this compound. It will be seen that I have appended the percentage composition calculated for $PtCl_4(C_{17}H_{25}N_4O_7HCl)_2$; but, of course, I do not imagine that I have ascertained the true formula of the poisonous principle; I merely throw it out as a suggestion that its composition may be something near $C_{17}H_{25}N_4O_7$, or the double of this.

From the experiment described above, it will be seen that the addition of the solution of platinum tetrachloride caused a marked change in the physiological action of the poison; and it should be borne in mind, that my main object in these experiments was the discovery of

* Only the faintest traces of sulphur are present in the compound.

an antidote (if possible) rather than the solution of the purely chemical question.

The experiments which I have made to investigate the physiological portion of the subject, naturally divide themselves into three distinct series: the first, in testing the action of graduated doses of the pure fresh poison hypodermically injected into animals; the second, in the injection of cobra virus mixed with dilute aqueous solution of platinum tetrachloride; and the third, in the action of the platinum salt on animals previously poisoned by fatal doses of the virus.

In my preliminary experiments, I assured myself that fresh cobra poison mixed directly with a small quantity of solution of platinum tetrachloride almost entirely loses its poisonous properties. In order to investigate this thoroughly, I selected chickens as good subjects to work on, and first ascertained the action of pure fresh cobra poison in various quantities, hypodermically injected, on these animals. The summary of the results which I obtained are shown in Table A, whilst the details of the experiment are described in an Appendix* which contains the description of most of the more important physiological experiments made during this investigation.

Table A.—*Action of Cobra Poison, hypodermically injected, on Chickens.*

No.	Amount of Dried Poison used.	Duration of Experiment.	Result.
		Hours. Minutes.	
1	6 milligrams	.. 17	Death
2	5 „	.. 31	„
3	1 milligram	.. 38	„
4	$\frac{1}{2}$ „	1 12	„
5	$\frac{1}{4}$ „	1 34	„
6	$\frac{1}{10}$ „	2 40	„
7	$\frac{1}{15}$ „	1 52	„
8	$\frac{1}{20}$ „	The animal was almost insensible for 12 hours.	Recovered

From this table, it appears that a quantity of $\frac{1}{15}$ milligram of pure dry cobra poison is sufficient to kill with certainty a healthy chicken, while $\frac{1}{20}$ of a milligram, when injected, produced such an effect, that for twelve hours the life of the chicken was despaired of; in larger doses the action of the poison is proportionately more rapid, until with a few milligrams death results within a few minutes.

A second series of experiments was then tried, by taking solutions of weighed quantities of the same sample of cobra poison as was used in the first series, mixing with known weights of platinum tetrachloride, and injecting. The experiments are subdivided into two classes, according to slight variations in the method of working; in some

* This Appendix is deposited in the Archives of the Royal Society.

cases the mixture of the poison and platinum salt was allowed to stand for a short time previous to the injection; and, in the remainder, the injection was performed a few seconds after the platinum solution had been added to the poison. The results are shown in Table B, which is divided into two sections: the first, showing experiments with the injection performed a short time after the mixture was made; and the second section showing injections made immediately after the two substances had been brought into contact with each other.

TABLE B.—*Action of Cobra Poison mixed with Platinum Tetrachloride on Chickens.*

I.—*Injection performed a short time after Mixing.*

No.	Weight of Dried Cobra Poison.	Weight of $PtCl_4$.	Duration or Symptoms.	Result.	No. of Chickens which the Poison if used alone was capable of killing.
1	3 milligrams	·01 gram	No symptoms of cobra poisoning	Survives	45 chickens
2	6 „	·02 „	„	„	90 „
3	6 „	·02 „	„	„	90 „
4	8 „	·025 „	„	„	120 „
			hours. min.		
5	10 „	·03 „	15 0	Death	150 „
6	15 „	·05 „	50 6	„	225 „

II.—*Injection performed at the instant of Mixing.*

			hours. mins.		
7	9 milligrams	·025 gram	4 0	Death	135 chickens
8	12 „	·04 „	1 25	„	180 „
9	12 „	·025 „	4 10	„	180 „

The first section of this table shows that almost any quantity, however large, of cobra poison mixed with the platinum salt, may be injected into chickens without producing any poisonous action; in the fourth experiment, for instance, enough poison to kill 120 chickens was injected into one chicken, and although the animal became drowsy and was evidently much upset by the injection of a large volume of liquid, the symptoms which were developed were not those of cobra poison, and the animal in the course of a day or two recovered its usual health and strength. In the case of No. 5, Table B, enough poison to kill 150 chickens was injected, and death resulted in fifteen hours; but the death apparently resulted from the effect of the shock to the system of the chicken, caused by the injection of such a large volume of liquid as was necessary. The same remark also applies to

Experiment 6, where enough poison to kill 225 chickens was injected, and death resulted from the setting in of diarrhœa, and from the subsequent exhaustion, in 50 hours 6 minutes after the performance of the hypodermic injection. In the experiments of injecting the mixture of the cobra poison and solution of platinum tetrachloride immediately after it was made, the results are not quite so satisfactory, but they still show that large quantities of poison (enough to kill 180 chickens being used in Experiment No. 1) may be injected, and that the fatal result does not take place until after the expiration of a period very much longer, as a rule, than would be the case without the use of the platinum salt. I have not thought it worth while to experiment with small quantities of poison in this manner, as the result of the first section conclusively shows that, if the platinum salt has time allowed to act upon the poison, the poisonous action of the virus is entirely destroyed. Before finally accepting this conclusion, I thought it advisable to try a similar set of experiments on other animals, such as dogs; and, as will be seen below, the experiments which have been performed have entirely borne out the conclusion above arrived at.

In the report of the Snake Poison Commission already referred to, a number of experiments have been made on the action upon dogs of graduated doses of cobra virus, and in the following table will be found a summary of the principal results:—

Table C.—*Action of Cobra Poison on Dogs.*

	Wt. of Poison injected.	Weight of Dog.	Time of Experiment.	Result.
			hrs. mins.	
*I.S.P.C. 3.	2½ grains	33 lbs.	1 43	Death
I.S.P.C. 4.	2 ,,	25 ,,	1 43	,,
I.S.P.C. 5.	1½ ,,	39 ,,	1 21	,,
I.S.P.C. 6.	1 grain	28 ,,	1 42	,,
I.S.P.C. 7.	1 ,,	24 ,,	0 57	,,
I.S.P.C. 8.	¾ ,,	35 ,,	1 32	,,
I.S.P.C. 9.	½ ,,	24 ,,	2 24	,,
I.S.P.C. 10.	½ ,,	19 ,,	1 58	,,
I.S.P.C. 11.	¼ ,,	20 ,,	4 20	,,
I.S.P.C. 12.	¼ ,,	41 ,,	8 0	,,
I.S.P.C. 16.	⅙ ,,	40 ,,	5 6	,,
I.S.P.C. 17.	⅛ ,,	14 ,,	4 28	,,
I.S.P.C. 14.	1/10 ,,	18 ,,	11 30	,,

From this table it appears that from a quarter of a grain (·0162 gram) to a sixth of a grain may be looked on as a fatal dose to an ordinary sized dog, while any quantities larger than this will produce a very rapidly fatal effect.

In order to test the action of the poison mixed with platinum salt,

* These refer to the number of each experiment in the report above quoted.

as before, I used weighed quantities of the dried fresh poison dissolved in water, and added known quantities of a standard solution of platinum tetrachloride; the results of these injections into dogs are shown in the table below, and in no instance did they show the slightest indication of any physiological action whatever. These animals were still alive when this paper was written.

TABLE D.—*Injection of Mixture of Cobra Poison and $PtCl_4$ into Dogs.*

No. of Expt.	Weight of Dog.	Weight of Cobra Poison used.	Weight of $PtCl_4$ used.	Duration of Expt.	Result.
1	38 lbs.	$\frac{1}{2}$ grain = ·0324 gram	·065 gram	..	Not affected
2	25 „	1 grain = ·0648 gram	·13 gram	..	„
3	35 „	3 grains = ·1944 gram	·30 gram	..	„

If we accept the conclusion that $\frac{1}{6}$ grain of cobra poison hypodermically injected will kill a dog under ordinary circumstances, in the three experiments under notice the quantities of cobra poison used were sufficient to kill three, six, and eighteen dogs respectively; and yet in no case did the dog show the slightest indication of cobra poisoning. I have not tried larger quantities than three grains of poison, as the volume of liquid which has then to be injected becomes inconveniently large; even in Experiment No. 3 in Table D no less than four or five separate hypodermic injections had to be made, to the great inconvenience of the dog experimented upon. From these results I think it may safely be said, that the compound which cobra poison produces when brought into contact with an aqueous and dilute (5 per cent.) solution of platinum tetrachloride is quite inert and physiologically inactive, though of course it does not follow that this substance can be used as an antidote in cases of snake poisoning. If I may be allowed the expression, platinum tetrachloride appears to be a chemical antidote to cobra virus, but whether it will be found to be a physiological antidote can only be settled by extensive researches.

Next, then, with a view to ascertain tentatively whether platinum tetrachloride can be used as an antidote, in the ordinary sense of the word, to snake poisoning, I have tried two sets of experiments described below. The results which I have obtained from this third series of experiments, although interesting and to a certain extent hopeful in their nature, have not, I am sorry to say, in any case shown that life can be saved by this means. As a preliminary step in the elucidation of this very important question, I have made some experiments on fowls and dogs by first hypodermically injecting known quantities of poison more than sufficient to kill, and after a definite interval injecting known quantities of the standard solution of platinum tetrachloride, at or near the same spot where the first injection was per-

formed. By the experiments of other investigators on the subject of snake poisoning, it has been found that the rapidity of the absorption of cobra poison is quite marvellous, and from the beginning I clearly saw that this would be the cause from which in all probability all experiments for the discovery of a true antidote would fail. So far as I could ascertain the non-success of the experiments about to be described appeared to be due to this cause; I first experimented upon chickens in the manner described above, and found that in two cases out of three life was prolonged to a very considerable extent, but I did not succeed in preventing death.

The results are given in Table E.

TABLE E.—*Experiments on Chickens.*

Injection of Platinum Salt after Poisoning by Cobra Virus.

No. of Experiment.	Amount of Cobra Poison.	Interval before Injection of $PtCl_4$.	Amount of $PtCl_4$.	Duration of Experiment.	Result.
				hrs. min.	
1	$\frac{1}{2}$ milligram	5 minutes	·025 gram	2 2	Death
2	$\frac{2}{10}$ „	1 minute	·05 „	1 5	„
3	$\frac{1}{4}$ „	1 „	·015 „	11 47	„

In Experiment No. 1 the amount of poison injected (enough to kill seven fowls) would, under ordinary circumstances, kill in about an hour or an hour and a quarter, but the animal here lived more than two hours. In Experiment No. 2 the poison would, under ordinary circumstances, kill in about an hour and a half, but in the experiment in question it killed in one hour and five minutes. This showed me a defect in my mode of working, in fact by injecting the platinum solution into the same orifice as the poison, and to about the same depth beneath the skin, I had merely driven the poison along before the second solution, and thus diffused it more rapidly through the system, naturally causing death to result in a shorter time than would have otherwise been the case. I should here mention that the same remark applies to some of the experiments I made on dogs described in the next table. In Experiment No. 3 in Table E, the amount of poison injected into the chicken was enough to kill three or four chickens, and would under ordinary circumstances have killed in from an hour and a half to one hour and forty minutes. In this case to avoid the defect in Experiment No. 2, the platinum solution was injected rather deeper than the poison, and the result was that the animal lived for eleven hours forty-seven minutes, or more than seven times as long as would have been the case had no platinum salt been injected. This was of course encouraging, and seemed to promise that the platinum salt would in certain circumstances be useful. The experiments made on dogs are

summarized in Table F, and the method used was the same as before described.

Table F.—*Injection of Platinum Tetrachloride into Poisoned Dogs.*

No. of Eexpt.	Wt. of Dog in lbs.	Amount of Cobra Poison used.	Interval before use of $PtCl_4$.	Amount of $PtCl_4$ injected.	Duration.	Result.
					hr. min.	
1	31	½ grain = ·0324 gram	1 to 2 min	·05 gram	2 45	Death
2	15	¼ grain = ·0162 gram	1 minute	·05 „	1 48	„
3	40	¼ grain	1 minute	·10 „	5 20	„
4	42	¼ grain	3 minutes	·15 „ internally	4 20	„

The platinum salt does not in these instances appear to have been as efficacious as in the experiments on chickens, and as previously mentioned I believe that the want of success was owing to the rapidity with which the poison is absorbed and diffused through the system, preventing any chance of the platinum salt coming in contact with the virus. This must prove a serious obstacle to the efficacy of any remedy. I confess that from my own experience of the rapidity of the absorption of this poison, I have considerable doubt as to the possibility of an antidote to snake poison ever being discovered, for in a few seconds after the actual bite of a snake the poison is absorbed by the blood, and carried by it to all parts of the system, and thus the task of overcoming or neutralizing the virus is rendered extremely difficult if not practically impossible.

In making the experiments described in the last two tables, I have been seriously hampered with the want of knowledge as to the physiological action of platinum salts in general, and of the platinum tetrachloride in particular; and although I have searched through every available authority, I have been unable to find any results bearing however slightly on this subject. To work out the problem before us, we must begin with selecting suitable living subjects for experiment. We must then ascertain what is the physiological action upon these animals of the platinum salt and of the cobra poison separately, both when administered internally and when injected hypodermically. Having settled these preliminary questions, we must proceed to test the applicability of the platinum salt as an antidote to the cobra virus by injecting hypodermically into similar subjects the smallest dose, which our preliminary experiments have shown to be fatal, and subsequently administering the platinum salt in various ways.

As my own physiological knowledge and surgical skill are very slight, I have asked Dr. E. Lawrie to join in the investigation above sketched out, and we have already commenced our experiments, the

results of which we hope to submit to the Society in a subsequent communication.

I do not, of course, lay any claim to have discovered an antidote (in the true sense of the word) to cobra poison, but I think that the experiments above described prove the inertness of the platinum compound of cobra virus, and that this may be deemed a sufficient ground for making more extensive researches on this subject. Nor do I at present venture to make any practical suggestions in connexion with the above facts as to the treatment of cases of snake bite, for until we are acquainted with the physiological action of platinum compounds this would be premature. The responsibility of recommending any change in the recognized methods of treatment would be very great, and it would be worse than rashness to deal with a subject of such vital importance without having a thorough knowledge of the effects likely to be produced by our actions.

Before concluding my paper I must tender my thanks to Mr. R. H. Wilson, C.S., officiating magistrate of the 24 Pergunnas, for kindly assisting me in procuring live cobras for the extraction of the poison, and to my assistant, Balm Poolin Behary Saor, for much aid rendered during the progress of a work not always pleasant to the feelings, and always more or less dangerous in its nature.

II. "On Repulsion resulting from Radiation. Part V." By William Crookes, F.R.S., V.P.C.S. Received December 3, 1877.

(Abstract.)

This paper commences with an experimental investigation of the amount of repulsion produced by radiation on disks of various kinds, and coated with different substances. The torsion apparatus for this purpose consists of a straw beam, suspended by a fine glass torsion fibre. At one end of the beam is a rod hanging downwards, and capable of taking six experimental disks in a vertical row. The other end of the beam is furnished with a pan and adjustible weights, to balance the varying weights of the disks submitted to radiation. A standard candle, at a fixed distance from the disks, supplies the radiation, and the amount of movement under its influence is measured by a ray of light reflected from a mirror on the beam to a graduated scale. An appropriate arrangement of screens enables any selected disk to be experimented on without the others being affected, and a standard lampblacked disk being always present, the results are capable of intercomparison. The beam, torsion fibre, disks &c., are sealed up in glass, and the whole apparatus is attached to a mercury pump, capable of carrying the exhaustion to any desired point.

The experimental powders are mostly chemical precipitates, laid on the surface of mica or pith disks as a water paint, no cement being used to promote adhesion. In other cases the substances are punched, cut, or filed into the shape of disks, 17·25 millims. in diameter.

The exhaustion, which has to be effected after each change of the experimental disks, is carefully brought to the same degree both by actual measurement on a McLeod gauge, and by getting the same repulsion on the standard black disk. In this way all the different results are fairly comparable one with the other. The presence of aqueous vapour must be specially guarded against by means of tubes containing phosphoric anhydride.

The effect of residual gas in tending to equalise the amount of repulsion on variously coloured surfaces is shown in an experiment with pith disks, one being lampblacked and the other retaining its natural white surface, the standard candle being at the same distance in each case. When the exhaustion is good enough to cause a fair repulsion, the ratio between the amplitude of swing (measured by the index ray) when the black is exposed and that when the white is exposed is as 100 : 55·5; at a little higher exhaustion the ratio is—

Black : White :: 100 : 42·5;

at a still better exhaustion (at which the experiments are usually tried) the ratio is—

Black : White :: 100 : 35.

The results of the quantitative examination of the repulsion resulting from radiation when falling on about 100 different substances are given in fourteen tables. Each table is in three columns, the first consisting of the names of the substances experimented on; the second the amount of repulsion observed, reduced to the standard of 100 for lampblack; the third column gives the repulsion observed when a water screen is interposed, reduced to the same standard.

Table I gives the results of the examination of black powders. Compared with lampblack = 100, these have an average value of 92·2, which becomes 99·1 by the interposition of water.

Table II contains white powders. These have an average value of 33·5, which is reduced to 8·3 behind water. The powerful absorption for the invisible heat rays which white powders exercise is somewhat remarkable. Assuming that the ultra-red rays from a candle are almost entirely cut off by a water screen, the comparatively strong action (33·5) produced by the naked flame must be mainly due to the absorption of the invisible heat rays; and when these are cut off by water the action is diminished nearly fifty times. With black powders the water only diminishes the action about eleven times.

Table III gives the red powders. Amongst these precipitated selenium is noteworthy. To the naked flame its value is 35·8, but

when a water screen is interposed the action becomes 69·5, in comparison with standard lampblack = 100. Omitting selenium, the mean action on red powders without a water screen is 32·2, and with a water screen, 24·9.

Table IV gives the brown powders. Amongst these, peroxide of thallium is remarkable as being repelled under the influence of radiation to a greater extent than any other body hitherto examined, its value being 121·7, in comparison to lampblack = 100. Brown powders behave most like black, the averages of the columns *without* and *with* a water screen being 92·7 and 94·5.

Table V gives the yellow powders. Among these anhydrous tungstic acid resembles scarlet selenium in its anomalous action, the figures being *without* water 50·8 and *with* water 72·2. The averages of the other yellow powders are 35·7 and (behind water) 13·8.

Table VI gives green powders. These show some discrepancies, which will be referred to farther on.

Table VII gives the blue powders. The action on these is of interest, as showing a much stronger proportionate action behind a water screen than with no screen, the averages being 55·8 and 65·2.

Table VIII gives the action on dyes and colouring matters of organic origin. Among these may be noticed saffranin, and a product of the decomposition of chlorophyll, which act differently to the others.

Table IX gives the action on metals prepared in different ways and coated with lampblack, mica, &c. Curious results are shown with iron and with gold, the former metal chiefly absorbing the invisible heat rays, whilst the latter metal is principally acted on by the luminous rays.

Tables X and X*a* give the results of an examination of various silver salts in their sensitive and non-sensitive state to light. The chloride, bromide, and iodide of silver, in their different states, are exposed to the standard candle after being submitted to the action of magnesium light, sunlight, and daylight. The results show how readily a change in the state of the surface is detected by an increased amount of repulsion under the influence of radiation.

Table XI gives the results of an examination of selenium disks. These are of two kinds, the vitreous and the crystalline. The latter is in the state most sensitive to *light* action. With the crystalline disk results have been obtained which seem to show that the impact of light on its surface produces a superficial disturbance there and in the adjacent gaseous molecules, which takes some time to subside. This is connected with the change in electric conducting power of crystalline selenium—a change which, when the element is transferred from light to darkness, takes some time to subside.

Table XII gives the results of the action of radiation on various substances which do not come under any of the foregoing headings,

such as pith, mica, charcoal, glass. The complicated nature of these actions is well shown in the results given by three pith disks, the first being plain white, the second lampblacked on the front, and the third lampblacked on the back. The first is repelled with a power of 17·7, the second (the standard) with a power of 100, whilst the third is not moved at all. The repulsion exerted on the white surface must be the same in each case, but the pressure of lampblack *behind* the pith causes a radiation of heat from the back surface, which produces molecular pressure just sufficient to neutralise the pressure in front.

Experiments are shown in this table with mica, both plain and roasted, and covered on one side or the other with lampblack. The results cannot well be described in abstract.

To show that physical condition has more effect in causing repulsion than chemical composition, results are given with various kinds of charcoal. It is shown that the repulsion suffered by cocoa-nut shell charcoal is much less than that by white pith, being only 11·6 against 17·7. At the same time, a radiometer made of cocoa-nut shell charcoal, lampblacked on one side, is only moderately sensitive, instead of being superior to one made of pith lampblacked on one side. The low figure shown by the charcoal is caused by its density enabling it to conduct heat from one surface to the other. Molecular pressure is, therefore, generated on both the back and front surfaces, and the figure given is simply the difference between the two opposing actions. Experiments show that this explanation is correct.

Besides water, other screens are used to filter the radiation of the candle before it falls on the disks. Water is, however, preferred for several reasons. It is almost perfectly opaque to the invisible heat rays, and, therefore, its employment allows easy discrimination between actions due to heat and to heat and light combined; secondly, it is colourless, and having no selective action on any visible ray of light, it can be used in conjunction with any coloured powder without complicating the results. Alum acts in a similar manner to water; coloured solutions act as water with a superadded action due to their colour. Very thick plates of glass have less action on the invisible heat rays than a thin layer of water. Sulphate of copper in so weak a solution as to appear only slightly green, has a very strong action when artificial light is used, as it cuts off the lowest visible red rays as well as the ultra red.

After having given the tabulated results of the examination and discussed the different actions, the author finds that the substances experimented on may be divided into two classes.

1. In which the repulsion behind water is *greater* in proportion to the standard than when no screen is present; and 2, in which the repulsion in proportion to the standard is *less* behind water than when no screen is present. Amongst class 1 may be mentioned copper

tungstate, saffranin, scarlet selenium, and copper oxalate; these are more affected by light than by invisible heat. Amongst class 2 may be mentioned pale green chromic oxide, persulphocyanogen, hydrated zinc oxide, barium sulphate, and calcium carbonate; these substances are more acted on by the ultra red rays than by the luminous rays.

From the results given in these tables, the author has been led to the construction of several radiometers which afford ocular proof of the general correctness of the indications given by the water screen. Thus, a radiometer made of pith disks coated on alternate sides with chromic oxide and precipitated selenium, moves in one direction to the naked flame of a candle and in the other direction when a water screen is interposed. A similar radiometer coated with saffranin and hydrated zinc oxide does not move at all when exposed to the naked flame, but revolves when a water screen is interposed. A radiometer coated with thallic oxide and Magnus's green platinum salt moves strongly when no screen is interposed, but is stopped by a water screen. Many other instruments having similar behaviour to the above are described, and their actions discussed and explained. The following experiment is given; a pith radiometer, coated with precipitated selenium and chromic oxide, is exposed to the radiation from a colourless gas flame from a Bunsen burner, coloured intensely green by thallium. To the eye, by this light, the chromic oxide looks nearly white and the scarlet selenium black. The rotation due to the superior repulsion of the chromic oxide is, however, apparently as strong as when the non-luminous flame is used. This is a proof that the train of reasoning the author has employed on former occasions is correct, viz., that certain substances have an absorptive action on rays of dark heat opposite to what they have on light, and that an optically white body may be thermically black, and *vice versâ*. Here, for instance, chromic oxide is optically green and thermically black, whilst scarlet selenium is thermically white.

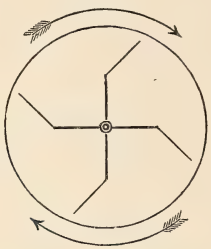
The author next examines the amount of repulsion produced when polarised light is allowed to fall on a plate of tourmaline suspended in vacuo in a torsion balance. It was considered possible that the repulsion might vary according to the plane of polarisation of the incident ray. Three separate sets of experiments are given in detail, and the results discussed; the final decision being that the special action originally thought possible does not exist in a degree appreciable with the present experimental means.

The next portion of the paper is devoted to an examination of the action of radiation on radiometers, the vanes of which consist of metallic plates, either bright on both sides or blacked on one side, and turned up at the corners in different degrees. When flat plates are taken, blacked on alternate sides, the rotation is normal or positive,

i.e. the black side is repelled. If one of the outer corners of each plate is turned up at an angle of 45°, so as to keep the blacked surface on the concave side, the positive rotation is either diminished, stopped, or converted into negative rotation, according to the amount of surface of the plate which has been turned up.

When the plates are kept flat, but the supporting arms are bent so as to present more of one side than of the other to the bulb, as shown in fig. 1, the fly rotates under the influence of radiation in the direction

Fig. 1.



of the arrows, even when there is no difference between one surface and the other. It is shown that the favourable presentation of the surface of the vanes to the inside of the bulb has more influence on the movement than has the colour of the surface. Experiments are described with radiometers of the form shown in fig. 1, and made of thick and thin mica, pith, aluminium bright on both sides, and aluminium blacked on one side. The action of light and of dark heat is given. The negative rotation set up in the fly when it is cooling from a high temperature, and the anomalous behaviour of the "favourably presented" radiometers when immersed in hot air or hot water, are examined. It is found that when a hot metal ring is applied to the equator of the bulb, the direction of rotation is always positive; and that when a hot ring of a smaller diameter is applied to the top or bottom of the bulb the direction of rotation is always negative. The direction of movement when the hot rings are removed and the fly is cooling is positive with the aluminium vanes, and negative with the thin mica and pith vanes.

The positive rotation, when the bulb is heated equatorially, is independent of the material of which the fly is made. It is caused by the hot ring of glass generating molecular pressure, which radiates towards the centre, and strikes the sloping vanes, driving them round as if a wind were blowing on them. The other movements of these "favourably presented" radiometers are explained in the paper, but it is difficult to reproduce the explanations in abstract without the aid of the diagrams which accompany the paper.

Having investigated the simplest form of favourably presented

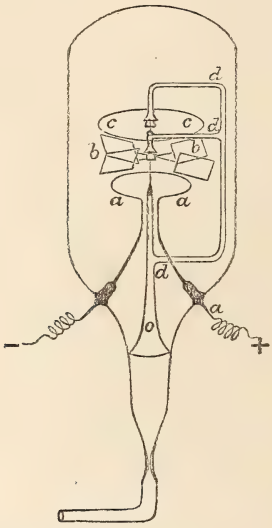
vanes, the author describes experiments with metal and other cones, cylinders, and cups. An abstract of some of these results was given in the Proceedings of the Royal Society for November 16, 1876, No. 175, page 313, but it was stated that the "molecular pressure" theory seemed incapable of accounting for some of the movements there observed. Further experiments have now cleared up this difficulty, and all the various movements are now seen to agree with this theory. The movement which appeared most anomalous was the attraction observed when a candle was allowed to shine on the hollow side of a cone or cup-vaned radiometer, the light being screened off the retreating side. When light falls on the hollow side of a cone-shaped vane, some is reflected and lost, but some is absorbed and is converted into heat of temperature. Aluminium being a good conductor of heat, and the thickness of metal being insignificant, it becomes equally warm throughout, and a layer of molecular pressure is formed on each surface of the metal. At a low exhaustion the thickness of this layer of pressure is not sufficient to reach from the metal cone to the side of the glass bulb. As the exhaustion increases, this layer extends further from the generating surface, until at a sufficiently high exhaustion the space between the side of the glass bulb and the adjacent portion of the metallic cone is bridged over, and pressure is exerted between the two surfaces. The more favourable presentation offered by the cone causes the pressure to be greatest between the glass bulb and the outside of the cone; the vanes are, therefore, pressed round by a force which is really acting from behind, although the movement looks as if the candle were attracting them.

A radiometer, the fly of which is furnished with four bright aluminium cups, rotates in the light as well as a flat vaned instrument blacked on one side. Experiments are described with one of these instruments attached to the mercury pump, and the observations of pressure and revolutions per minute under the influence of a standard candle are tabulated and drawn as a curve, taking the rarefaction of the air in millionths of an atmosphere as abscissæ and the number of revolutions a minute as ordinates. The curve, which is traced through the dots representing observations, shows a gradual increase in the sensitiveness of the instrument to light, up to about 50 millionths of an atmosphere; then the action keeps with little variation to about 30 millionths, thence it rapidly sinks, until at about 1 millionth it is less than $\frac{1}{38}$th of the maximum, and at 0·2 millionth the radiometer refuses to turn even when five candles are put near it.

The concluding portion of the paper is devoted to an examination of the movements produced in highly rarefied gases, when thin mica vanes suspended horizontally, and sloping like the vanes of a windmill, are exposed to the action of a ring of platinum, placed just below them, and rendered incandescent by a current of electricity. The

object has been to produce the molecular pressure by a very intense heat which would not have to pass through glass, and to allow it to act on vanes which are turned in the most favourable position for rotating under the influence of the molecular pressure. The direction is called *positive* when it is the same as would be produced by a wind blowing from the platinum wire, and *negative* when it takes the contrary direction. In the apparatus most experimented with a rotating disk of mica is supported on a separate pivot, immediately over the mica vanes. The normal or positive movement of this disk is in the opposite direction to that of the vanes; thus, if the positive movement of the vanes is in the direction of the hands of a watch, the positive movement of the disk is in the opposite direction.

Fig. 2.



With the apparatus full of air at the normal pressure the direction of rotation, both of the vanes and disk, is *positive* when the platinum wire is ignited. This is probably due to the ascending current of hot air.

At a pressure of 80 millims. the disk does not rotate. The vanes rotate *positively*, but slowly.

At 19 millims. no movement whatever takes place. The disk and vanes are as still when the wire is heated as when it is cold.

At 14 millims. the disk remains stationary. The vanes move slowly in the *negative* direction.

At 1 millim. the disk rotates in the *positive* direction slowly, whilst the vanes rotate *negatively* rather fast; both disk and vanes now rotate in the same direction.

At a pressure of 706 millionths of an atmosphere the direction keeps the same as at 1 millim. in each case, but the speed is greater.

At 294 millionths, the speed of the disk and vanes is exactly alike, both rotating together in the same direction. Up to this pressure and at some distance beyond, the vanes have been gradually diminishing whilst the disk has been increasing in speed. At a pressure of 141 millionths the disk rotates rapidly, *positively*, but the vanes do not rotate at all. At a little higher exhaustion than the last, viz., at 129 millionths, a great change is observed. The vanes, which were still, now rotate in the *positive* direction at a speed of 100 revolutions a minute, whilst the disk rotates positively as before, but with a little diminished velocity. It is probable that some of the speed of the disk has been quenched by the rapid movement of the vanes in the opposite direction, as the author has previously shown* that the viscosity of air at a rarefaction of 129 millionths of an atmosphere is only a little less than its viscosity at the normal density, and hence the vanes, at a speed of 100 revolutions a minute, must exert a considerable drag on the opposite rotation of the disk.

As the rarefaction increases above this point, the speed of both the disk and vanes increases, till they exceed 600 revolutions a minute. At the highest rarefaction attained (0·4 millionth of an atmosphere), there is no apparent diminution in speed.

These experiments have been repeated in a more elaborate series with an apparatus of great complexity. It is impossible without drawings to give an idea of the various arrangements by which data are secured, but it may be mentioned that at each pressure observations can be taken on the velocity of rotation of the disk and vanes, the viscosity of the residual gas, the repulsion exerted by a standard candle on a black mica plate, and the appearance of an inductive spark through a tube furnished with platinum wires. Different gases can be experimented with, and a series of observations are given with hydrogen gas as well as with air.

The author concludes his paper by showing that the ordinary ideas of a "vacuum" are very erroneous. Formerly an air-pump which would diminish the volume of air in the receiver 1,000 times was said to produce a vacuum. Later a "perfect vacuum" was said to be produced by chemical absorption and by the Sprengel pump, the test being that electricity would not pass; this point being reached when the air is rarefied 100,000 times. According to Mr. Johnstone Stoney, the number of molecules in a cubic centimetre of air at the ordinary pressure is probably something like 1,000,000000,000000,000000 (one thousand trillions). Now when this number is divided by 0·4 million, there are still left 250,000000,000000 molecules in every cubic centi-

* "Proceedings of the Royal Society," November 16, 1876, No. 175, p. 305.

metre of gas at the highest exhaustion to which these experiments have been carried. Two hundred and fifty billions of molecules in a cubic centimetre appear a sufficiently large number to justify the supposition that when set into vibration by a white-hot wire they may be capable of exerting an enormous mechanical effect.

January 24, 1878.

Sir JOSEPH HOOKER, K.C.S.I., in the Chair.

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

The following papers were read:—

I. "New Determination of the Mechanical Equivalent of Heat." By J. P. Joule, LL.D., F.R.S. Received November 15, 1877.

(Abstract.)

An account is given by the author, of the experiments he has recently made, with a view to increase the accuracy of the results given in his former paper, published in the "Philosophical Transactions" for 1850. The result he has now arrived at, from the thermal effects of the friction of water, is, that taking the unit of heat as that which can raise a pound of water, weighed in vacuo, from 60° to 61° of the mercurial thermometer; its mechanical equivalent, reduced to the sea-level at the latitude of Greenwich, is 772·55 foot-pounds.

II. "The Cortical Lamination of the Motor Area of the Brain." By Bevan Lewis, F.R.M.S., Pathologist and Assist. Med. Officer to the West Riding Asylum, and Henry Clarke, L.R.C.P. Lond., Med. Officer to the West Riding Prison. Communicated by D. Ferrier, M.A., M.D., F.R.S., Professor of Forensic Medicine, King's College, London. Received November 7, 1877.

[Plates 1–3.]

Whilst pursuing certain investigations upon the comparative histology of the brain, the authors of this article have been enabled to demonstrate certain facts with regard to cortical lamination, and to show the disposition and significance of certain elementary constituents, the importance of which, they believe, justifies their publication.

Types of Structural Lamination.

Meynert enumerates five distinct plans, according to which the various layers of the cortex are disposed, of which the more extensive formation is his *five-laminated type*, which he regards as that characteristic of the cortex of the vault and a portion of the gyrus fornicatus.* It is with regard to this type of structural lamination that the observations now to be recorded deal; and it is hoped that they will tend not only to point out how errors have crept into the descriptions of some writers, but also to reconcile conflicting opinions upon this subject. We regard the ascending frontal convolution as affording the most suitable representation of the typical lamination of the motor area, and our descriptions, therefore, apply more directly to this region than to the remaining gyri at the vertex.

It is premised that at least one authority of note, Baillarger, differs from Meynert in his description of this type of lamination, enumerating the layers as six in number, whilst Meynert notices five only.

Typical Lamination of the Motor Area.

The following description of the lamination of cellular elements in the ascending frontal convolution will suffice to convey the result of our observations.

First Layer.—A pale narrow band, extremely delicate and friable, is seen in sections to form the extreme limit of the cortical layers. The histological tissues composing this formation are connective, nervous, and vascular. The connective does not differ essentially from that universally present as the basis of the cortical substance. It consists of minute fibrils, interlacing, anastomosing, and forming, by an intricate plexiform arrangement, a dense felt-like bed, possessing great powers of elastic distension. In the areola betwixt these fibrils, a finely molecular base may be detected, which appears greatly altered by the employment of various reagents. In this matrix distinct connective cells are observed, including one or more nuclei within an investment of most delicate protoplasm, and from which, when seen under favourable circumstances, numerous minute branches radiate into the surrounding neuroglian network. In sections obtained from preparations hardened by chromic acid two forms of cellular elements are observed:

(*a.*) Cells that in diameter vary from 6μ to 9μ, the delicate protoplasm of which is almost invariably shrunken up or destroyed by the re-agents employed, appearing, therefore, like free nuclei, except when examined by fresh methods of preparation. These elements assume the direction taken by the vessels of the cortex, and frequently are seen in large numbers along the perivascular sheaths.

(*b.*) Larger cells, ranging in diameter up to 13μ, with a similarly

* "The Brain of Mammals." By Theodore Meynert (Stricker's Human and Comparative Histology, vol. ii, p. 381).

delicate protoplasmic investment. These cells include one, two, or three nuclei, and assume a spheroidal or flask-like configuration *in situ;* when, however, seen near the frayed-out margin of a teased preparation, they are found to give off numerous radiating processes. Aniline black (for which nervous elements have a great affinity) stains their nuclei, investing protoplasm and its extensions so uniformly and faintly, that it is with some difficulty they are recognised, complete differentiation from the faintly-tinted matrix being scarcely attainable. In form, reaction to staining agents, and still more as regards their modifications by disease, they are undoubtedly proved to be connective elements. We find occasionally that these elements proliferate freely, assume the most quaint forms, and produce a dense matting of fibres. Such a condition, truly a sclerosis, is figured in a paper recently contributed to the Royal Society by one of the authors.* No other cellular elements appear in the first layer of the cortex; the angular and fusiform nerve-cells alluded to by some writers have had no existence in specimens we have examined by the fresh and hardened methods. They are probably elements of the connective group, possibly identical with the larger nucleated bodies just described, the appearance of which is greatly altered by the methods of preparation employed. The elements found in the first layer, therefore, are neuroglia corpuscles and perivascular cells. Both varieties are shown in the drawings accompanying this paper. (*Vide* Plate 1, First Layer.)

Second Layer.—This consists of a series of closely aggregated pyramidal and oval cells, of small size, whose apical processes are arranged radially to the surface of the cortex. Numerous other processes arise from the basal angles, and radiate outwards and downwards from the cell, including an extensive area in their distribution. Their measurements vary from 11μ to 23μ in length, and from 6μ to 9μ in breadth. Their nucleus is comparatively of large size, often measuring 4μ to 6μ in diameter, sometimes assuming the pyramidal form of the enclosing cell, when it may attain a length of 9μ. With the exception of their pigmy size and extremely crowded aggregation, no differences are apparent between them and the larger elements of the third layer. (*Vide* Plate 1, Second Layer.)

Third Layer.—The reddish-grey band coinciding with the second layer, as seen by naked-eye examination, appears abruptly marked off from the pale layer subjacent to it. On microscopic examination, however, it is found that the line of demarcation can be only approximately obtained, as the elements of both overlap each other very irregularly. The distinction between these two layers near their common boundary must depend upon a two-fold condition, viz., the gradual increase in size of the elements of the third layer and their

* "The Relationships of the Nerve-cells of the Cortex to the Lymphatic System of the Brain." By Bevan Lewis, Proc. Roy. Soc., Sept., 1877.

more distant grouping. It will be seen in traversing the full depth of this layer (Plate 1, Third Layer) that the cells almost uniformly increase in size from above downwards, yet small pyramidal bodies, no larger than those at the commencement of the series, are found occasionally at all depths, side by side with the larger nerve-cells. The configuration of these cells is by no means uniform. They often assume the pyriform rather than the truly pyramidal form, having their larger basal extremity marked by numerous dentations, from whose projecting points delicate branches are thrown off on all sides. Their apical process is directed radially to the surface of the cortex, and can be traced up into its second layer. Each cell contains a nucleus and a distinct nucleolus. The basal or axis-cylinder process is rarely seen, as it is necessarily divided, in most cases, by the oblique direction which it takes to reach the radiating fibres of the medulla. These cells vary greatly in dimensions: those in the uppermost regions of this stratum measuring 12μ by 8μ, their nucleus being 7μ by 5μ; whilst those in parts bordering upon the subjacent layer average 22μ in length, and the largest cells even attain a size of 41μ by 23μ.

Fourth Layer.—We have now entered upon highly debatable ground, including the chief topic we have proposed for consideration. Much confusion appears to have prevailed with regard to the characters and relationships of this and the subjacent layers. Thus, Meynert* describes the fourth layer as constituted of irregular angular cells, forming a stratum *immediately* superincumbent on his fifth layer of *spindle-cells*. Baillarger, on the other hand, enumerates six cortical layers, of which Meynert's angular and spindle-cells form his fourth and sixth respectively. After considering fully the views of various writers on this subject, we find ourselves unable to agree with most of them regarding this type of lamination. The layer immediately beneath the third undoubtedly consists of angular cells which in size, form, and branchings are accurately described by Meynert. They resemble, *on a small scale*, transverse sections of large pyramidal cells, numerous branches being given off from all sides of the cell, and rarely are they seen extended radially towards the surface of the cortex. Now we find this stratum of angular cells *intersected* by a layer of immense ganglionic cells, which run about midway through the stratum, and it is distinctly affirmed that these angular cells are found freely *above and below* this important formation (Plate 1, Fourth Layer). On turning our attention more especially to these ganglionic cells, we find that they differ very materially from any of the elements already described. The enormous dimensions to which they attain, averaging 71μ in length by 35μ in breadth, stamp them as the most notable objects in the whole depth of the cortex, whilst in special cells the not unusual length of 126μ, shows that they occasionally rival even

* Op. cit., p. 389.

the large multipolar cells in the anterior column of the cord. It will naturally be asked how Meynert could have overlooked this important layer of cells. Our explanation is, that whilst failing to regard them as a layer *distinct from the third*, yet the existence of these individual elements was recognised by him, as he states that the cells of the third layer progressively augment till they attain the length of 40μ.* As the largest cells of the third layer, however, but rarely attain such dimensions, we presume that he had in view the elements to which we now allude, probably regarding the depth at which they were placed as an exceptional condition. Baillarger, on the other hand, whilst recognising the layer, has also, we think, fallen into the error of misplacing it in the order of cortical lamination. The error consists essentially in placing these cells on too low a level, in fact, between the angular and spindle cells, whilst the many hundred specimens examined by us enable us to localise this series midway down the band of angular elements, a position maintained with singular uniformity. Dr. Major† follows Baillarger in regarding the cortex of the vault and that of the central lobe as consisting of six layers. Mierzejewski‡ and Betz‖ follow Meynert, and refer to a five-laminated type, but the large cells of the cortex more particularly have been made the subject of special study by both writers. The position of these great cells being such as we have described, one of two schemes must be accepted as representing the normal lamination of this region. Either the layer of angular cells above these great elements must be regarded as a series separate from those below the same, thus making the type common to this region of the cortex consist of seven laminæ, of which the great cells referred to constitute the fifth; or, on the other hand, these latter cells with the belt of angular elements above and below may be regarded as a single layer, and termed the mixed or ganglionic series. We incline to the latter as the preferable arrangement, for, apart from the consideration that the former plan introduces an unnecessary multiplication of the series of layers, there remains the important fact that the series of large cells alluded to *is not a constant layer*. Indeed, there are extensive portions even of the ascending frontal gyrus when *these elements are wholly absent*.

At these sites the angular elements present the appearance of a single undivided belt. We are thus compelled to regard this remarkable layer of cells as an intercalated series, and regard it as preferable still to adhere to Meynert's five-laminated type, assuming the fourth layer (which we term the ganglionic layer) to be subject to the modification above alluded to. Far more important considerations, however, than

* Op. cit., p. 386.

† "Histology of the Island of Reil." West Riding Asylum Med. Reports, vol. vi.

‡ "Etudes sur les Lesions Cérébrales dans la Paralysée Générale." Archives de Physiol., 1875.

‖ "Gehirncentra." Betz, Centralblatt f. d. Med. Wissensch., Aug., 1874.

those dwelt upon attach themselves to these cells, and these are of such a character as to warrant us considering them apart, after having completed our description of the separate layers of the cortex by a brief examination of the deepest stratum.

Fifth Layer.—This series consists of fusiform or spindle-shaped cells, arranged in columns by the interposition of bands of medullary fibres. (Plate 1, Fifth Layer). They average in size 25μ by 9μ, the largest attaining the dimensions of 32μ by 13μ. Their nuclei are almost invariably oval, and measure from 11μ to 13μ in length by 6μ to 9μ in breadth. As remarked by Meynert,* these cells are not strictly bi-polar, as a large proportion throw off several branches, whilst a very frequent form gives off a lateral branch from the projecting side of the cell midway between either extremity (Plate 1, Fifth Layer). These cells are arranged in columns as above stated, their long axis lying in the plane of the intervening medullary fibres, but along the base of a sulcus their long axis lies horizontally to the surface of the cortex, suggesting the belief that they belong to the connecting or arcuate system. An extensive series of observations was made with the object of calculating the varying number of cells in the third and fifth layers at different points along the ascending frontal convolution, and it was found that the average number of pyramidal cells in the quarter-inch field† was fifty-seven, the average number of spindle cells was forty. The extremes were respectively thirty-three to ninety-nine, and twenty-one to seventy-three. Although at some sites, therefore, the proportion may increase to three times what it is at other sites, no relationship could be traced between these variations and the alternating thickness and dense groupings of cells in the remaining layers. For convenience of reference we subjoin a table of measurements of the cells of the different layers of the cortex in the ascending frontal convolution.

	Length.	Breadth.
First Layer.		
Small cells	6μ—9μ	6μ—9μ
Large „	13μ	
Second Layer	11μ—23μ	6μ—9μ
Third Layer.		
Smaller cells	12μ	8μ
Larger „	22μ—41μ	4μ—23μ
Fourth Layer.		
Ganglionic cells	71μ	35μ
Largest cell observed	126μ	55μ
Fifth Layer	25μ—32μ	9μ—13μ

* Op. cit., p. 389.

† The quarter-inch field alluded to was equivalent to a circle half a millimetre in diameter.

Ganglionic Cells of the Fourth Layer.

Three years ago Professor Betz* published the results of a series of observations which had for their object the more accurate description quantitatively and qualitatively of the histological elements of the cortex. His results were of much interest and importance. Professor Betz terms these great cells "giant pyramids," and gives a short account of their anatomical distribution in man and the higher apes. The important conclusion arrived at by Betz was that the "giant pyramids," in their form and distribution, have the significance of motor cells. We have thought it highly desirable to make a critical examination of the relationship which these ganglionic cells bear to the extensive area defined as the motor area by Ferrier. We shall consider these cells under the four heads of Form, Size, Processes, and Distribution.

1st. *Form.* This is very variable, as may be seen by reference to Plate 2. The pyramidal form is frequent, whilst a plump body elongated towards either pole approaching in contour somewhat the fusiform character is very typical of these cells. The truly fusiform, or bi-polar cell, is more rarely met with except in the region of the first frontal convolution. The cell itself is devoid of a cell-membrane, and its protoplasmic contents extend for some distance into the very numerous branches arising from it. To this may be attributed the great irregularity in marginal conformation, for the more numerous the branches arising from a cell the more irregular will its outline be. No other nervous elements in the cerebral cortex assume such fantastic outlines, or give rise to so many branching processes. Considering their dimensions and their numerous connections, we may be justified in regarding them as among the most highly integrated anatomical units in the cerebrum. Each cell contains a large round or oval nucleus enclosing a nucleolus. They are peculiarly prone to pigmentation—patches of pigment being found in the protoplasm of most of these cells as a constant and normal constituent, just as it is found universally present in the multipolar cells of the cord. Their immediate environments have been already considered in a paper alluded to above ("Relationships of the Nerve-cells to the Lymphatic System").

2nd. *Size.* The measurement of a large number of these cells from the same site in the ascending frontal convolution gave us an average length of 60μ—an average diameter of 25μ, the extremes being 30μ and 96μ for length, 12μ and 45μ for breadth. Taking, however, a very large selection of cells from *various points* of this convolution the average length was 71μ, the average width was 35μ, the extremes varying from 36μ to 126μ for length, and from 24μ to 55μ for breadth.

* "Anatomischer Nachweis Zweier Gehirncentra." Prof. Betz, Centralblatt f. d. Med. Wissensch., Aug., 1874.

The breadth of these cells was again tested by their measurement in several sections cut *at right angles* to their long diameter. In this case 156 cells averaged 35μ. The nucleus averaged 13μ long, by 9μ broad, the extreme length 20μ, extreme breadth 12μ.

3rd. *Processes.* The average number of secondary processes* arising from ninety of these cells was seven, the largest number observed was fifteen. Sections across the long axis of the cell, however, occasionally brought into view as many as eighteen, and exhibited the cell as occupying the centre of an extensive area over which its branches spread, radiating outwards and downwards from all points of its margin. From this mode of branching it will, of course, be apparent that the absolute number of processes given off by a single cell cannot be determined with any degree of certainty, and that statements with regard to the comparative number of such in man and the lower animals must be received with extreme caution, as the number exhibited will vary with the direction of the section and the methods employed for examination. It will be readily conceived how the fresh method of preparation described by one of the authors of this paper,† brings into view a far larger number of processes than are observed by other methods of preparation.

4th. *Distribution.* The facilities afforded by the ether-freezing microtome,‡ have enabled the writers to examine in rapid succession and under most favourable conditions extensive portions of the same brain in a perfectly fresh state, and with but little trouble a series of convolutions may thus be sliced from end to end and the sections examined *seriatim.* The results of such an examination have been to demonstrate the very important fact that these "giant cells" are distributed over certain *definite areas* which are remarkably constant, and it appears to the authors that these *cell-groupings* are especially and exclusively a characteristic feature of the motor area, as defined by Ferrier. That these giant cells may be found in isolated positions at parts not included in the motor area the writers do not question; but they have not succeeded in finding any *large groups* or *distinct areas* of such elements in portions of the cortex at a distance from this region of the motor centres. The present investigation has been limited to a portion of the motor area, viz., the ascending frontal and two upper frontal convolutions; but it is hoped that this attempt to sketch the topographical distribution of these cells may prove but the introduction to a more complete and accurate investigation of the histology of individual convolutions. Such an investigation must eventually be made if scientific precision is aimed at by the histologist in the field of cerebral

* By secondary processes we mean all branches, exclusive of the two main branches—the basal and apical.

† Monthly Microscopical Journal, Sept., 1876.

‡ *Vide* Journal of Anat. and Phys., April, 1877, and Mo. Micros. Journal.

pathology. The scheme (Plate 3) appended to this paper represents the distribution of these cells as determined from a thorough microscopic examination of the left ascending frontal, and two upper frontal convolutions of eight human brains, in all of which the arrangement of these cells was remarkably uniform.

The broad upper extremity of the ascending frontal convolution, limited above by the longitudinal fissure and below by the lower line of origin of the first frontal, was found to present two large groupings of these cells. The first group (fig. 1, A—D) includes some of the largest cells in this formation, and is distributed over the *posterior* or *parietal* aspect of the gyrus adjacent to the ascending parietal convolution. This group disappears towards the summit of this lobule, so that the *anterior* or *frontal* half of a section usually shows none of these cells present.

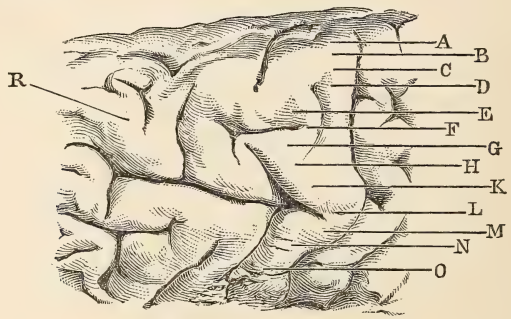


Fig. 1.—Left ascending frontal and two upper frontal convolutions, seen from above. The letters refer to the parts from which sections figured in the scheme were taken.

The second group (fig. 1, E—G) appears near the lower point of origin of the first frontal, and is found along the frontal aspect at this point—none being found posterior or along the parietal side. The first group referred to occupies the upper two-thirds of this portion of the ascending frontal, and limited almost exclusively to the aspect adjacent to the ascending parietal, corresponds to the areas 2 and 4 in Ferrier's drawings.* In the scheme several sections through this area (Plate 3, A—D) represent the exact number and distribution of these cells at different levels from above downwards. The lower group (Plate 3, E), which takes a position forwards near the first frontal, appears to correspond closely to another motor area (No. 5 of Ferrier). The cell-groupings here are entirely on the frontal half of the convolution, but at slightly lower levels, as at F and G they are observed to spread over the vertex of the gyrus and appear at its posterior aspect (Plate 3, F—G).

The portion of this convolution intervening between the two upper

* "The Functions of the Brain." By Dr. Ferrier. Figs. 63 and 65.

frontals presents almost invariably a marked sinuous flexure, the central portion of which often forms a tolerably plump lobule. A large area is represented by the parietal aspect of the upper two-thirds of this region in which the cells are large and numerous, extending beyond the vertex of the convolution (Plate 3, K), but, sloping backwards at a lower level until they finally disappear (Plate 3, M). Thus from the points M to N no groupings of these cells were discoverable. Again, the remaining third of this region, and the portion immediately connected with the second frontal convolution, show a separate grouping of these great cells (Plate 3, N—O). The latter group commences posteriorly and spreads beyond the vertex of the convolution. The two separate areas last described (H—M and N—O) occupy approximately the circles 6 and 7 in Ferrier's figures.* The posterior extremities of the superior and middle frontal

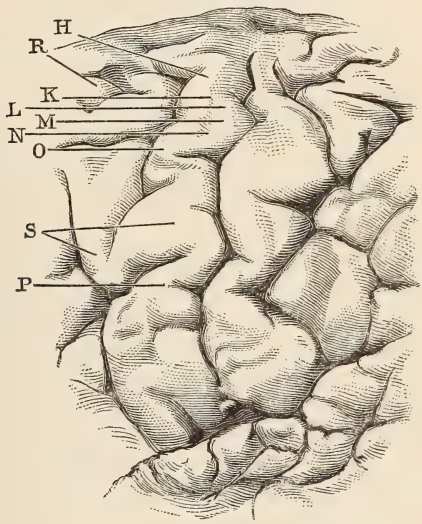


Fig. 2. Left ascending frontal convolution, seen from the side, including that portion of the two upper frontals (R—S) admitted into the scheme for examination. The letters, as in fig. 1, correspond with those of the scheme (Plate 3).

convolutions exhibit a very extensive area of these cells, which, however, rapidly disappear as our sections extend forwards. It is to be observed that this extensive group of cells can be followed back into the ascending frontal convolution, communicating therefore with the series which at the origin of the frontals have been seen to occupy the anterior aspect (Plate 3, R and S).

To recapitulate, we have presented to us a series of distinct groupings of these great cells arranged chiefly along the *parietal aspect* of

* Op. cit. Figs. 63 and 65.

the ascending frontal convolution, interrupted by two groups which occupy the *frontal aspect* at the origin of the superior and middle frontals, and which at these points appear to run into an extensive area disposed over the posterior third of the two latter convolutions. That portion of the ascending frontal marked P (on the chart of the convolutions) has a group of these cells situated along its parietal aspect. The portion intervening between the two lower frontals we have excluded from our scheme, relegating it to a later period, when it will be described in common with the rest of the motor area. Meanwhile we would draw attention to the fact that these cells diminish in *size* towards the lower extremity of the ascending frontal, as they also do in the areas included in the frontal gyri, as shown by the following table :—

	Average Size of Ganglion Cells.	Largest Cell.
Left ascending frontal (*upper extremity*) ...	$60\mu \times 25\mu$	$90\mu \times 45\mu$
First frontal (*area at posterior end*)	$45\mu \times 20\mu$	$69\mu \times 27\mu$
Left ascending frontal (*lower extremity*) ...	$35\mu \times 17\mu$	$41\mu \times 18\mu$

In the area at the posterior end of the first frontal (fig. 1, R), the cells assume an elongated contour, a very large majority approaching the spindle form. It is necessary to add that the limits of these areas is not always sharply defined, as occasionally these cells graduate off into smaller and yet smaller elements of the same character, until they eventually disappear. In many instances this *fading off* at the margin is very noticeable. In other cases, when these ganglionic cells are absent their position is occupied by a series of cells of uniform size, but, whilst exhibiting the general characters of the greater elements, they do not exceed in size the smaller cells of the third layer. These elements, however, invariably occupy the same level as the large ganglionic cell, and we regard them as undeveloped representatives of this series. Within the areas specified these large cells are grouped closely together in twos or threes, and often assume the arrangement of a belt one cell deep. To recapitulate, then, we find these cells form an *interrupted series* differing thus from the continuous layers above them, and that groupings of cells are thus distributed over certain areas of the cortex closely corresponding to several of the motor centres of Ferrier. Again, we find each group is in itself constituted of numerous secondary groups of cells, "the nests" of Betz, which differ much in the number of cells forming each group. It must also be noted, that although the giant size of these cells in the upper half of the ascending frontal constitutes a most characteristic feature—the all-important fact to bear in mind is their *interrupted distribution*, as

Human Brain

Section of Ascending Frontal Convolution Exhibiting the Typical Lamination of the Motor Area, x 87 diams.

W. West & Co. sc.

Ganglionic Cells of Cortex.
From Sections across their long Axis.

W. West & Co. sc.

Ganglionic Cells of Cortex.
From Vertical Sections.

Schem. illustrating Distribution of Ganglionic Cells of Cortex
Left Superior Middle Frontal Convolution of Man. A–S

this feature still predominates in other portions of the motor area where the *size* of the cells no longer warrants us in calling them giant cells, their position in the order of lamination, their general form and distribution in clusters sufficing to identify them as the same elements.

This disparity in size between these elements in the fourth layer of the motor area induces us to reject the term giant cell as applied *generally* to this formation, and to call them in preference the ganglionic cells of the cortex. Their close resemblance to the ganglionic cells of the spinal cord has been fully recognised by Betz and other observers.

EXPLANATION OF DRAWINGS.

Plate 1. A section taken through the ascending frontal at its upper extremity, exhibiting the five layers of the cortex as seen magnified 87 diameters.

Plate 2. The ganglionic cells of the fourth layer of the cortex from an area opposite the first frontal on the ascending frontal convolution. In the lower group the various forms assumed by these giant cells are well shown, whilst the upper group represents the appearance presented by sections carried across their long axis. The latter method illustrates the wide area commanded by their outspreading branches.

Plate 3. A scheme illustrative of the exact number and arrangement of the ganglionic cells at different points of the ascending frontal and two upper frontal gyri.

Each curved line is supposed to represent the boundary line of a section carried vertically through the ascending frontal from before backwards. The right hand portion of the curve, therefore, represents the side adjacent to the ascending parietal; the left hand segment of the curves represents the frontal aspect, from which the frontal gyri arise. Figs. 1 and 2 serve to localise the site of these sections. The exact number of these cells, and their relative positions, were first sketched, as accurately as possible, under a quarter-inch power, and the sketch reduced by photography. That portion of the scheme relating to the two frontals shows, at R, a section through the area of the first frontal carried *across* its length, and at S in like manner, a section through the ascending frontal opposite the second frontal, and, therefore, carried *along* the length of that convolution.

N.B.—Plates 1 and 2 represent sections obtained from frozen brain, by means of the ether microtome.

III. "Researches in Spectrum Analysis in connection with the Spectrum of the Sun." By J. Norman Lockyer, F.R.S. Received November 17, 1877.

(Abstract.)

The author refers to the work already done in the new map of the Solar Spectrum as enabling the chemical constitution of the Sun's atmosphere to be studied under more favourable conditions.

He shows that the work already done enables him to confirm the presence of Sr, Pb, Cd, K, Ce and U, and also that it indicates the probability of the presence of Va, Pd, Mo, In, Li, Rb, Cs, Bi, Sn, La, Gl, Yt or Er.

IV. Note on the Bright Lines in the Spectra of Stars and Nebulæ." By J. Norman Lockyer, F.R.S. Received December 31, 1877.

Owing to absence from England in April last, I have only just become acquainted with Dr. Huggins' paper, in reply to that by Mr. Stone, on the above subject. As Mr. Stone has again directed attention to the matter, I am anxious to say that I agree with him so entirely* that two years ago I searched for indications of a large chromosphere in the case of *a* Lyræ and some other stars. I believe I have had glimpses of bright lines at *F* and *b*, but if this discussion had not arisen I should

* I append an extract from a lecture on the Structure of Nebulæ and Stars, which I gave at Manchester in the autumn of 1876 ("Manchester Science Lectures"), to show the perfect accord there is between us. "There are nebulæ and stars with spectra so similar that if one had the evidence of the spectroscope alone, it might be impossible to decide which was nebula and which was star. Now this may be a little startling to some of you, and therefore it is only fair I should explain it. The stars, you know, are so remote from us that in the most powerful telescopes to which spectroscopes are applied, they appear only as the finest points of light. Now these points of light, it is not absurd to imagine, may in some instances be two millions, or perhaps even three millions, of miles in real diameter. We know that our own sun, which is certainly not the largest star in the heavens, is nearly one million miles in diameter; that is to say, the true sun, the true stellar nucleus, is one million miles in diameter. Now when I dealt in my second lecture with the physical constitution of the sun, I pointed out that the sun which we see, the sun which sends us the majority of the light we receive, is but a small kernel in a gigantic nut, so that the diameter of the real sun may be, say, two million miles. Suppose then that some stars have very large coronal atmospheres; if the area of the coronal atmosphere is small compared with the area of the section of the true disc of the sun, of course we shall get an ordinary spectrum of the star; that is to say, we shall get the indications of absorption which make us class the stars apart; we shall get a continuous spectrum barred by dark lines. But suppose that the area of the coronal atmosphere is something very considerable indeed, let us assume that it has an area, say fifty times greater than the section of the kernel of the star itself; now, although each unit of surface of that coronal atmosphere may be much less luminous than an equal unit of surface of the true star at the centre, yet if the area be very large, the spectroscopic writing of that large area will become visible side by side with the dark lines due to the brilliant region in the centre where we can study absorption; other lines (bright ones) proceeding from the exterior portion of that star will be visible in the spectrum of the apparent *point* we call a star. Now it is difficult to say whether such a body as that is a star or a nebula. We may look upon it as a nebula in a certain stage of condensation; we may look upon it as a star at a certain stage of growth."

have still hesitated to mention this, as I have been hoping for an increase of optical power which would have enabled me to be quite certain on the point. My aperture (6 inches Cooke) is not adequate to put the result beyond all doubt.

As, however, the question has been raised, it is better at once to state the attempt and its result, and to ask others with greater optical power to search for the lines; taking the precaution to use the cylindrical lens close to the eye, and not to apply it to the instrument until the rays to be examined are absolutely in focus on the slit, if a slit is used. It is possible scintillation may help matters.

V. "On the relative 'Facility of Production' of Chemical Combinations." By Sir B. C. Brodie, Bart., D.C.L., F.R.S. Received November 21, 1877.

(Plate 4.)

A circumstance which cannot fail to impress the student of chemistry is the extreme paucity of chemical substances. The combinations of which he can conceive are innumerable, but those which he can realize are few—a mere sprinkling from the sea-shore sand—and, making the fullest allowance for human incapacity, it yet appears that there are natural facilities and natural obstacles in the way of making certain compounds which are independent of our skill and power.

It is my object to show that among these is to be reckoned that relative "facility of production" of chemical substances which depends on the mathematical laws of combination which are inherent in the very nature of chemical combination and unalterable. By this I mean the relative facility with which the units of matter may be constructed and taken to pieces, owing to the relation of these units to the number and kinds of the simple weights of which they are made up; and I propose to consider in what way, if any, this unequal "facility of production" is connected with these mathematical laws. Is it true that, as a matter of theory, there arises from these laws any greater facility for the production of one compound than of another, or do all stand on the same level? and, further, does experience tally with the conclusions of theory?

In my second Memoir,* on the Calculus of Chemical Operations, I have shown that the unit of every chemical substance is to be regarded as constituted by the performance of the several operations $\alpha, \chi, \xi \ldots$ upon the unit of space, and have also shown† that the ultimate analysis of every chemical event leads to the con-

* "Phil. Trans.," vol. clxvii, part i, p. 35, 1877.

† *Loc. cit.*, p. 115.

clusion that chemical events occur by the transference to and fro among the units of matter and space of certain portions of matter $w(a) \cdot w(\chi) , w(\xi) \ . \ . \ .$, there termed simple weights, which are the results of the performance of the operations $a , \chi , \xi \ . \ . \ .$; the same unit therefore may be constructed in various ways corresponding to the differences in order in which these simple weights are transferred so as to build up the same unit of matter. Some explanation is necessary on this point.

There is nothing in the laws of combination (and these alone we are now considering) from which we can infer the performance of any one of these operations taken singly to be essentially easier or more difficult than the performance of any other; that is to say, the facility of constructing the unit a is the same as that of constructing the unit ξ; but when we consider combinations of more than one operation the case is different. The combinations of the operations a and ξ taken two and two together are a^2, $a\xi$, and ξ^2; the combination a^2 is unknown, $a\xi$ is the unit of water, and ξ^2 the unit of oxygen. Now, there is only one way in which the unit ξ^2 can be produced, namely by the successive performance of two operations ξ. Similiarly, there is only one way in which a^2 may be produced. But there are two ways in which the unit $a\xi$ may be produced, namely by first performing the operation ξ on the unit of space and then the operation a, in which case the unit $a\xi$ is the result; and also by first performing the operation a and then the operation ξ, which results in the combination ξa. These two results (so far as our present purpose is concerned) are identical, as follows from the commutative law $a\xi = \xi a$ demonstrated in Part I.* We hence say that the "facility of production" of the unit $a\xi$ is twice that of the respective units a^2 and ξ^2. The ternary combinations of a and ξ are a^3, $a^2\xi$, $a\xi^2$, and ξ^3; a^3 and $a^2\xi$ are unknown, $a\xi^2$ is the unit of binoxide of hydrogen, and ξ^3 is the unit of ozone. On these principles the respective "facility of production" of these units is—

$$\begin{aligned} a^3 \ &. \ . \ . \ 1 \\ a^2\xi \ &. \ . \ . \ 3 \\ a\xi^2 \ &. \ . \ . \ 3 \\ \xi^3 \ &. \ . \ . \ 1. \end{aligned}$$

Again, the ternary combinations of the operations a and ν are a^3, unknown; $a^2\nu$, the unit of ammonia; $a\nu^2$, the unit of nitrogen; ν^3, unknown. The respective "facilities of production" of these units are—

$$\begin{aligned} a^3 \ &. \ . \ . \ 1 \\ a^2\nu \ &. \ . \ . \ 3 \\ a\,\nu^2 \ &. \ . \ . \ 3 \\ \nu^3 \ &. \ . \ . \ 1. \end{aligned}$$

* "Phil. Trans.," part ii, 1876, p. 796.

Similarly, the "facility of production" of the unit of the hydrocarbon valerylene $a^4\kappa^5$ is to the "facility of production" of the hydrocarbon benzene $a^3\kappa^6$ as 126 to 84. The measure of the "facility of production" of a unit of matter being in all cases that number which expresses the different ways in which the operations may be permuted by which that unit is made. It is important to observe that the comparison which is here effected is that between the "facility of production" of the units of homogeneous substances, namely units severally composed of the same number of simple weights. Thus we can compare the "facility of production" of valerylene $a^4\kappa^5$ and benzene $a^3\kappa^6$, but we have no means of effecting this comparison between the unit of valerylene $a^4\kappa^5$ and the unit of propylene $a^3\kappa^3$.

That this "facility of production" is among the causes which determine the existence of certain units of matter to the exclusion of others can hardly be denied, as a speculative truth, by anyone who admits that these units are built up from simple weights in the manner I have described, but the action of this cause might be so veiled from our view, by the action of other causes, that we might never be able to detect it by the isolation of its effects. The verification of a scientific theory is effected by comparing the results indicated by that theory with the actual results of experience, and no physical theory can be regarded as demonstrated unless it can stand the test of this comparison. The system of combinations which in the present instance lends itself most readily to this verification is the system of hydrocarbons. This system contains some eighty-eight combinations, and, although limited as regards the full attainment of the object in view, is yet far more extensive than any other similar binary system. The composition, too, of the several units of the system is well determined. The gaseous density of the hydrocarbon being, in numerous cases, ascertained by experiment, and, where this is not the case, being determined by reasoning, generally of a very conclusive character.*

Before proceeding further, I will give a list of the hydrocarbons to be considered, and their symbols. For facility of reference, they are arranged according to the powers of a.

1	unit of space.		$\alpha^2\kappa^3$	unit of allylene.	
κ	,,	carbon.	$\alpha^2\kappa^6$	,,	phenylene.
α	,,	hydrogen.	$\alpha^3\kappa^2$	,,	ethane.
$\alpha\kappa^2$	,,	acetylene.	$\alpha^3\kappa^3$	,,	propylene.
$\alpha^2\kappa$	,,	methane.	$\alpha^3\kappa^4$	,,	crotonylene.
$\alpha^2\kappa^2$	,,	ethylene.	$\alpha^3\kappa^5$	,,	valylene.

* In compiling this list, pains have been taken not to introduce non-existing hydrocarbons. It has not appeared necessary, in all cases, to verify the list by reference to the original authorities, but it may be taken as representing the existing view of the subject as given in the best text-books. In making these references, I have been much indebted to my friend Mr. W. F. Donkin, who has gone into the question with the greatest care.

$\alpha^{3}\varkappa^{6}$	unit of	benzene.
$\alpha^{3}\varkappa^{8}$	,,	phenylacetylene.
$\alpha^{4}\varkappa^{3}$	,,	propane.
$\alpha^{4}\varkappa^{4}$	,,	butylene.
$\alpha^{4}\varkappa^{5}$	,,	valerylene.
$\alpha^{4}\varkappa^{7}$	,,	toluene.
$\alpha^{4}\varkappa^{8}$	,,	cinnamene.
$\alpha^{4}\varkappa^{10}$	,,	naphthalene.
$\alpha^{4}\varkappa^{12}$	,,	ethine naphthalene.
$\alpha^{5}\varkappa^{4}$	,,	tetrane.
$\alpha^{5}\varkappa^{5}$	,,	amylene.
$\alpha^{5}\varkappa^{6}$	,,	diallyl.
$\alpha^{5}\varkappa^{7}$	,,	
$\alpha^{5}\varkappa^{8}$	,,	xylene.
$\alpha^{5}\varkappa^{9}$	,,	allylbenzene.
$\alpha^{5}\varkappa^{10}$	,,	dihydronaphthalene.
$\alpha^{5}\varkappa^{11}$	,,	methylnaphthalene.
$\alpha^{5}\varkappa^{12}$	,,	diphenyl.
$\alpha^{5}\varkappa^{13}$	,,	fluorene.
$\alpha^{5}\varkappa^{14}$	,,	anthracene.
$\alpha^{5}\varkappa^{16}$	,,	pyrene.
$\alpha^{6}\varkappa^{5}$	,,	pentane.
$\alpha^{6}\varkappa^{6}$	,,	hexylene.
$\alpha^{6}\varkappa^{7}$	,,	œnanthylidene.
$\alpha^{6}\varkappa^{9}$	,,	cumene.
$\alpha^{6}\varkappa^{10}$	,,	phenylbutene.
$\alpha^{6}\varkappa^{12}$	,,	ethylnaphthalene.
$\alpha^{6}\varkappa^{13}$	,,	diphenylmethane.
$\alpha^{6}\varkappa^{14}$	,,	stilbene.
$\alpha^{6}\varkappa^{15}$	,,	methylanthracene.
$\alpha^{6}\varkappa^{16}$	,,	
$\alpha^{6}\varkappa^{18}$	,,	chrysene.
$\alpha^{7}\varkappa^{6}$	,,	hexane.
$\alpha^{7}\varkappa^{7}$	,,	heptylene.
$\alpha^{7}\varkappa^{8}$	,,	caprylidene.
$\alpha^{7}\varkappa^{10}$	,,	cymene.
$\alpha^{7}\varkappa^{14}$	,,	benzyltoluene.
$\alpha^{7}\varkappa^{16}$	,,	dimethylanthracene.
$\alpha^{7}\varkappa^{17}$	,,	naphthylphenylmethane.
$\alpha^{7}\varkappa^{18}$	unit of	diphenylbenzene.
$\alpha^{7}\varkappa^{20}$	,,	dinaphthyl.
$\alpha^{7}\varkappa^{22}$	,,	idrialin.
$\alpha^{8}\varkappa^{7}$	,,	heptane.
$\alpha^{8}\varkappa^{8}$	,,	octylene.
$\alpha^{8}\varkappa^{9}$	,,	
$\alpha^{8}\varkappa^{10}$	,,	terpene.
$\alpha^{8}\varkappa^{11}$	,,	amylbenzene.
$\alpha^{8}\varkappa^{14}$	,,	anthracene hexhydride.
$\alpha^{8}\varkappa^{15}$	,,	benzylxylene.
$\alpha^{8}\varkappa^{19}$	,,	triphenylmethane.
$\alpha^{9}\varkappa^{8}$	,,	octane.
$\alpha^{9}\varkappa^{9}$	,,	nonylene.
$\alpha^{9}\varkappa^{10}$	,,	rutylene.
$\alpha^{9}\varkappa^{12}$	,,	amyltoluene.
$\alpha^{9}\varkappa^{16}$	,,	benzylcumene.
$\alpha^{9}\varkappa^{18}$	,,	tetramethylanthracene.
$\alpha^{9}\varkappa^{20}$	,,	tolyl diphenylmethane.
$\alpha^{9}\varkappa^{24}$	,,	triphenylbenzene.
$\alpha^{10}\varkappa^{9}$	,,	nonane.
$\alpha^{10}\varkappa^{10}$	,,	diamylene.
$\alpha^{10}\varkappa^{13}$	,,	amylxylene.
$\alpha^{10}\varkappa^{26}$	,,	tetraphenylethylene.
$\alpha^{11}\varkappa^{10}$	,,	decane.
$\alpha^{11}\varkappa^{26}$	,,	tetraphenylethane.
$\alpha^{12}\varkappa^{11}$	,,	endecane.
$\alpha^{12}\varkappa^{19}$	,,	dimesityl methane.
$\alpha^{13}\varkappa^{12}$	,,	dodecane.
$\alpha^{14}\varkappa^{13}$	,,	tridecane.
$\alpha^{14}\varkappa^{15}$	,,	benylene.
$\alpha^{15}\varkappa^{14}$	,,	tetradecane.
$\alpha^{15}\varkappa^{15}$	,,	triamylene.
$\alpha^{15}\varkappa^{16}$	,,	cetenylene.
$\alpha^{16}\varkappa^{15}$	,,	pentadecane.
$\alpha^{16}\varkappa^{16}$	,,	cetene.
$\alpha^{17}\varkappa^{16}$	,,	hexdecane.
$\alpha^{20}\varkappa^{20}$	,,	tetramylene.
$\alpha^{27}\varkappa^{27}$	,,	cerotene.
$\alpha^{30}\varkappa^{30}$	,,	melene.

With the view of effecting a comparison between the hydrocarbons actually produced and their relative "facility of production," as indicated by theory, I have constructed the following map. The nature of this map will be readily understood from inspection. So far as the numbers are concerned, it is the arithmetical triangle of Pascal. Each square contains a number, and the symbol of a hydrocarbon. "A line drawn so as to cut off an equal number of units from the top horizontal row and the extreme left-hand vertical column, is called a base. The bases are numbered, beginning from the top left-hand corner. Thus, the tenth base is a line drawn through the numbers 1, 9, 36,

84, 126, 126, 84 36, 9, 1."* These numbers, it will be observed, are the numerical coefficients of the several terms in the expansion of $(a + \kappa)^9$ and the symbols in the corresponding squares the symbols corresponding to those coefficients. Thus, for example, in the case of the fourth base, which passes through the numbers 1, 3, 3, 1, we have $(a + \kappa)^3 = a^3 + 3a^2\kappa + 3a\kappa^2 + \kappa^3$. The numerical coefficients, as is well known, express the number of ways in which the letters in the corresponding symbols may be permuted. Now, the "facility of production" of the hydrocarbon varies with the number of these permutations, so that in each square the symbol is associated with the number expressing its "facility of production." If experience be concordant with theory, the actually existing hydrocarbons which appear on the several base-lines (between which it is our object to effect the comparison in question) will be found in those squares in which the highest numbers appear. Now, if a perpendicular be let fall upon any base from the upper left-hand corner of the square in the top horizontal row, this line will either pass through the square containing the greatest number or will pass between two such squares, the numbers equally diminishing on each side. The perpendicular drawn through all the bases will be termed the axis or main diagonal of the system.

The squares in which the actually existing hydrocarbons appear are in the map shaded; there are no known hydrocarbons corresponding to the plain squares. If we follow the course of the axial line, it will be perceived that, up to the twentieth base, the squares through which the axis passes and the squares which lie immediately to the right and to the left of the axis, with one exception, are coloured grey, so that, up to this point, all the hydrocarbons which have theoretically the greatest "facility of production" have actually been produced. Further, a close inspection will show that there is a decided tendency in the system to approximate to this central line.

It does not follow, from these considerations, that "facility of production" should be the only cause determining the actual existence of the hydrocarbons. The case is not so simple as that of drawing white and red balls from an urn or cards from a pack. At one time the efforts of chemists may be mainly, although even unconsciously, directed to filling up one part of this system; at another time to filling up another part; efforts which may have their origin in testing the truth of speculations or possibly in the discovery of some one method or one hydrocarbon;† but the remarkable fact remains that, notwithstanding the operation of these multifarious causes acting in various

* *Vide* Todhunter, "History of the Theory of Probability."

† Thus the great attention which has of late been paid to the derivatives of phenyl has caused what may be considered as an exaggerated growth of the system to the right of the main diagonal.

directions, the paramount influence of this "facility of production" should still be apparent as the one cause which determines the general aspect of the system.

The causes I have mentioned would obviously determine an unequal growth of the system and preclude a perfectly symmetrical arrangement, at any given time of observation, of the cluster of hydrocarbons around the axial line. How far this inequality is due to accidental causes, and how far to the operation of permanent causes acting in one direction, is impossible, from the slender data we possess, to say, but the chemist should ever be alive to the detection of permanent deviations, in the form of the actual system, from the form indicated by theory, for in such observations lie our best means of detecting the existence of other causes affecting its growth, besides that predominant cause which has been here discussed.

January 31, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President in the Chair.

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

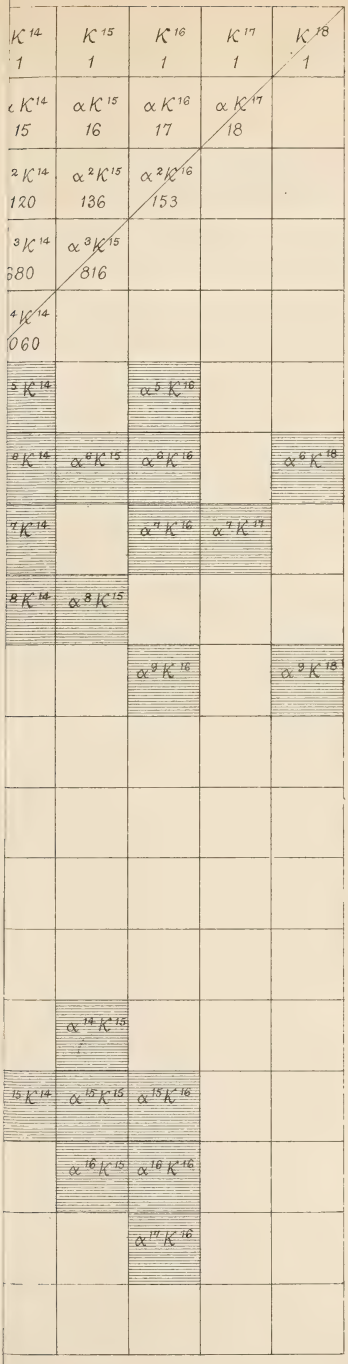
The following papers were read:—

I. "Further Researches on the Minute Structure of the Thyroid Gland." Preliminary Communication. By E. CRESSWELL BABER, M.B. Lond. Communicated by Dr. KLEIN, F.R.S. Received November 21, 1877.

In a previous communication to the Society* I have described some observations made on the minute anatomy of the thyroid gland of the dog. Since then I have extended these observations, under the direction of Dr. Klein, to the glands of other vertebrate animals. The chief results as yet arrived at will be very shortly described in the present communication, a full account of them being reserved to a future paper.

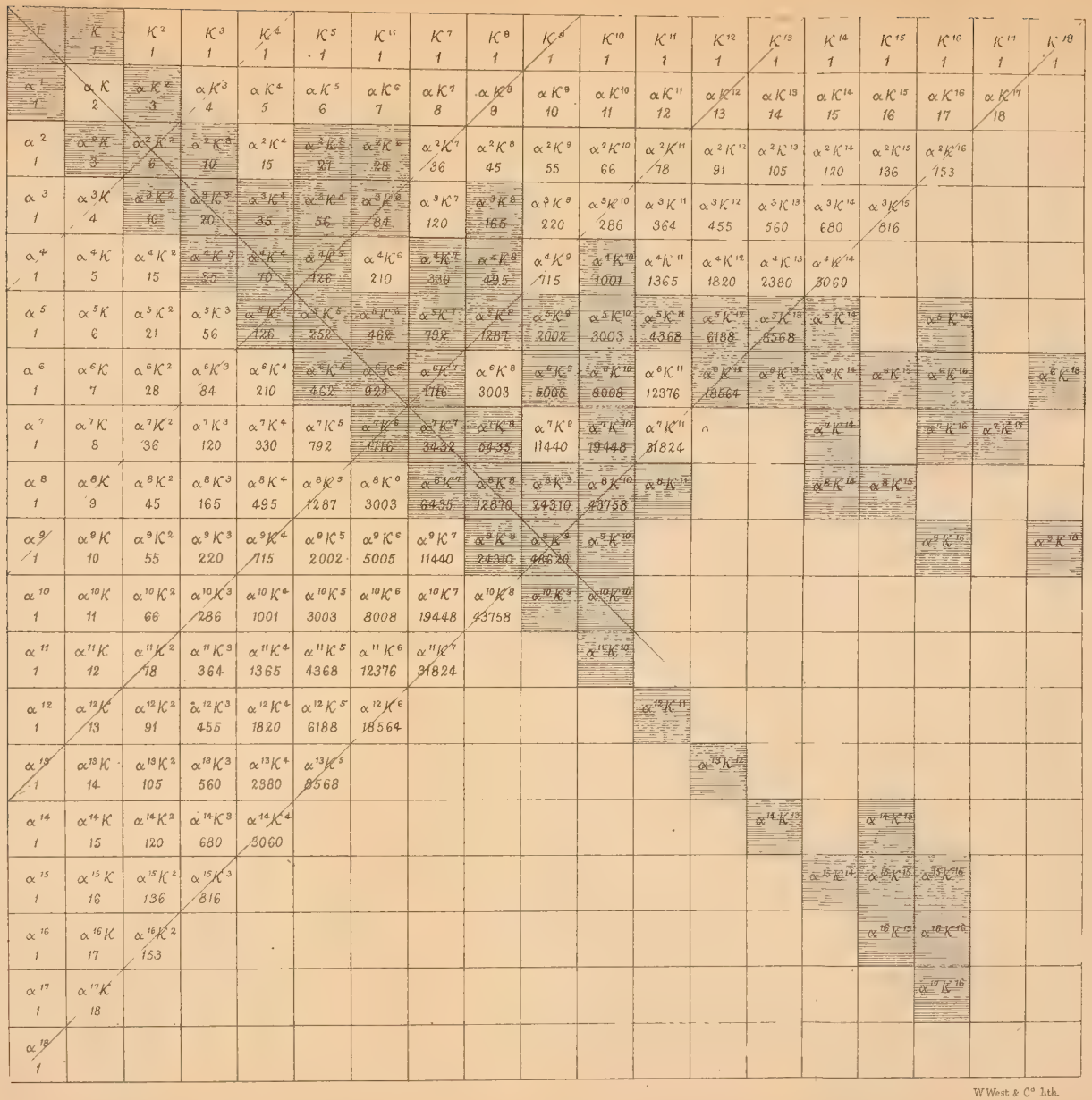
Lymphatics.—In the thyroid gland of the dog I have described a dense rounded network of lymphatics traversing the gland in all directions, and consisting of lymphatic vessels, tubes, and spaces. A similar system of lymphatics has been observed in the glands of other mammalia, as kitten, rabbit, man, and horse; the extent of distribution, however, as shown by the injection, appears to vary in different

* "Philosophical Transactions," 1876, vol. clxvi, pt. ii, p. 557.



W. West & Co. lith.

MAP OF THE SYSTEM OF HYDROCARBON.



W West & C° lith.

animals. In the *tortoise* a network of lymphatics can be injected, of which the smaller ramifications run between almost all individual vesicles. The homogeneous or granular material which I have described in the lymphatics of the dog's thyroid, has been observed in the same vessels in other thyroid glands, *e.g.*, in those of horse, man, sheep. It occurs in both injected and uninjected glands. The quantity varies much in different glands of the same animal, it being sometimes present in large quantities, and at other times appearing to be entirely absent. These observations so far merely confirm the results arrived at in the dog. In the thyroid gland of *birds*, however, a different arrangement of parts takes place. On injecting the thyroid gland of a pigeon by the method of puncture with Berlin blue, it becomes swollen, and the injection is seen running *in the jugular vein*, with which the gland is in close apposition.* Examination of sections of the gland shows that the injection has entered vessels containing blood-corpuscles. The same vessels become filled on injecting in like manner with nitrate-of-silver solution. To ascertain whether by the puncture method the blood-vessels really become injected, another gland was injected with Berlin blue from the lower part of the carotid artery, the artery at the same time being secured above the gland. In this case, in which the injection had entered the capillaries and veins, it was evident that these were the same system of vessels which were injected by the method of puncture. I may, therefore, state that on repeated injections of the thyroid gland of the pigeon, both with Berlin blue solution and with nitrate of silver by the method of puncture, *I have been unable to inject any system of lymphatic vessels, but have always found the injection in the blood-vessels of the gland.* In the thyroid gland of this bird the blood-vessels present the following characters:—The capillaries, as usual, form a network running between the individual gland-vesicles, but in proportion to the size of the vesicles (which in this gland are as a rule small), they do not appear so minute, nor to have such complicated ramifications as in the case of mammalia, but resemble more the distribution of the lymphatics in some of those animals. The veins frequently surround, either partially or entirely, the arteries which they accompany. Immediately under the capsule of the gland numerous large veins are seen, and in the fibrous capsule itself, I have noticed layers of blood-corpuscles, which appear to be contained in blood-vessels communicating with the veins of the interior. Once or twice I have noticed in the large veins on the surface of the gland (under the capsule), in addition to blood-corpuscles, and perhaps coloured injection, a greater or less quantity of a material of homogeneous aspect, presenting an appearance similar to the material seen

* It will be remembered that on injecting the dog's thyroid gland in a similar manner, the injection was seen emerging from the gland *in lymphatic vessels* which ran to neighbouring lymphatic glands.

in the vesicles, and also to that described above in the lymphatics of the thyroid gland of the dog and other mammalia. In the thyroid gland of the *rook* no system of lymphatics is injected by the method of puncture, but the blood-vessels, presenting an appearance very similar to those in the pigeon, become filled. Since the publication of my researches on the thyroid gland of the dog, I have become acquainted with P. A. Boéchat's thesis on the structure of the thyroid gland (published in 1873), in which he describes the lymphatics of this organ. I regret that I was not previously aware of his researches, but it is satisfactory to find that the results of our independent observations on this subject are very similar.

Epithelial Cells.—In the thyroid gland of several animals I have observed in the epithelial cells *numerous very fine parallel striæ*, running in the direction of the long axis of the cell, *i.e.*, from the summit (or free extremity) of the cell down towards its base. I can sometimes trace this striation running apparently the whole length of the cell from apex to base; at other times it is only visible for a greater or less distance near the summit of the cell. I have observed it most clearly in the thyroid glands of the skate, tortoise, pigeon, and kitten. Between the epithelial cells of the thyroid gland a *reticulum* may often be observed. I have noticed it more especially in the tortoise, skate, kitten, &c. Recently Otto Zeiss * has described a reticulum between the epithelial cells of the thyroid gland.

Parenchyma.—The large round cells described in my previous paper in the thyroid gland of the adult dog, and there named "parenchymatous cells," I have since observed in dogs aged five and nine weeks respectively. In the thyroid gland of the *cat* parenchymatous cells are present in considerable numbers, although not nearly so numerous as in the dog. Parenchymatous cells are also seen in the thyroid of the *rabbit*. In the thyroid gland of the *pigeon* large groups are frequently seen consisting of cells, which are larger than the adjacent epithelial cells, round or oval in shape, and provided each with a single spherical or oval-shaped nucleus. They resemble very much the parenchymatous cells seen in the dog.

Undeveloped Portions.—In the thyroid gland of the *adult dog* bodies of considerable size are frequently seen, which differ entirely in structure from the rest of the gland. They are rounded or flattened in shape, usually situate on the surface of the organ, and possess the following structure. They consist of a solid mass of more or less *cylindrical rows of cells*, which are much convoluted and interlace in all directions. Between them run capillary blood-vessels, and also probably some lymphatics. These "cylinders" are composed of cells resembling epithelial cells, columnar or cubical in shape, those on the surface of the

* "Mikroskopische Untersuchungen über den Bau der Schilddrüse." Strassburg, 1877.

cylinder, next to the capillaries, being arranged at right angles to the long axis of those vessels. Each cell is provided with a nucleus, usually oval in shape. In very few, if in any, of these cylinders have I been able to detect any central canal. In the dog I have always observed these "undeveloped portions" as distinct bodies, not continuous with the normal gland tissue, but separated from it by layers of connective tissue, and frequently lying in depressions on the surface of the gland. They appear to be portions of gland, whose development has become arrested at an early stage, and there is no evidence to show that they undergo any further development subsequently.

In the *kitten* similar undeveloped portions are seen, but in this animal they may sometimes be observed to be continuous with the ordinary gland tissue. In this case a formation of vesicles from the cylinders of cells appears to be taking place by the growth into them laterally of processes of connective tissue with blood vessels, and by their excavation into vesicles. In the kitten the cylinders are less convoluted than in the dog, and throughout the gland the fully formed vesicles frequently appear grouped in rows, which have a more or less parallel arrangement. Somewhat similar, but shorter, cylinders of cells are seen in the thyroid of the *pigeon*, scattered throughout the gland. I may mention that in the thyroid glands of fœtal pigs (measuring about $2\frac{1}{4}$ inches in length) before the formation of vesicles has taken place, I have seen cylinders presenting an appearance similar to those above described.

In sections of the glands of young dogs (aged about five weeks and three months respectively) I have observed that the vesicles are very much branched, and present numerous hollow ramifications. In the thyroid glands of numerous other dogs of different ages, I have with equal certainty ascertained that the vesicles presented very few, if any, of these hollow branches. These much-branched vesicles are doubtless the hollow branched cavities (tubes) of Zeiss, which he obtains by floating them out from portions of the fresh gland of young animals. In glands in which the vesicles present this appearance, the walls of the vesicles are frequently inflexed so as to form numerous projections into the interior, as already described by Verson and myself. Zeiss has repeatedly endeavoured to inject these hollow-branched cavities by the method of puncture, but without success. Neither have I, in all my injections, ever succeeded in filling any such structures, which must surely have been the case if the hollow-branched cavities were in communication with one another to any extent, for it seems almost impossible to suppose, as Zeiss does, that the contents of these cavities can prevent the injection from running into them, whilst we know that the viscid contents of the lymphatics have no such effect. I am of opinion that these hollow branched cavities do not communicate with one another to any extent, and that, in the dog at least, they merely

form a stage in the growth of the gland. In the mature state of the organ in this animal I consider that the vesicles, as usually supposed, consist of cavities more or less spherical in shape, which are not in communication with one another.

The points mentioned in this note, together with others connected with the subject, will be fully discussed in a future paper.

II. "On Stratified Discharges. V. Discharge from a Condenser of Large Capacity." By William Spottiswoode, M.A., LL.D., Treas. and V.P.R.S. Received November 22, 1877.

The principal object of the following communication is to describe an instrumental arrangement which has proved very convenient for the production of steady striæ. The first attempt which was made nearly two years ago (February, 1876), consisted in charging a Leyden battery of nine large jars by means of an induction coil, and discharging it gradually through a vacuum tube. This was effected by connecting one terminal of the tube with the outside of the battery, and presenting the other terminal, made pointed, to a knob connected with the inside, at suitable distances. The following effects were then observed:

(1.) When the interval between the terminal and the knob was considerably greater than striking distance, the appearance in the tube was cloudy and apparently unstratified, or showed only faint indications of stratification. It was, in fact, very similar to that produced by attaching one terminal of the tube to one of an induction coil, and carrying the other to the earth.

(2.) When the interval was within striking distance, the usual jar-discharge without stratification or dark space took place.

(3.) When the interval was slightly greater than striking distance, but not so great as in the first case, a bright stratified discharge was observed. The proper motion due to a decline in tension was shown by a revolving mirror, and by a careful but rapid alteration in the distance during discharge, the motion could be arrested or even reversed. The duration of the whole, although long compared with a single flash from an ordinary coil, did not exceed half a second.

This experiment gave reason to hope that if a condenser of sufficient capacity were constructed, the discharge might be prolonged, and even varied, so as to allow an actual study of its various phases to be made.

The next attempt was made during last summer with some condensing plates, constructed for cable purposes, and kindly lent to me by Messrs. Latimer Clark, Muirhead, and Co. The results were in every way calculated to encourage further steps.

At the suggestion of Mr. De la Rue, and with the assistance of his

battery for the purpose of testing the instrument, the same firm constructed for me condensers of which the following are the particulars. Each condenser is contained in a box, and has a capacity of 13·8 microfarads, subdivided into ten sections, each section containing forty sheets of tinfoil, 18 in. × 13 in., insulated from each other by eight sheets of parafined tissue paper. The superficial area of foil in each box is 1,300 square feet, and that of parafined paper 14,166 square feet. It was found that these condensers could easily be charged with a 4-inch induction coil, worked by two Grove's, or even bi-chromate, cells. A much smaller coil would certainly suffice if the coil were made with a thick secondary, since $\frac{1}{4}$- to $\frac{1}{2}$-inch sparks are all that are required. In order to charge the condenser, one terminal of the coil was carried to outside of the condenser, and the other to the other with an intervening air spark. The object of the air spark was twofold: first to ensure that the tension of the electricity was sufficient to give the required charge to the condenser, and, secondly, to prevent the latter from discharging itself back through the secondary of the coil. After some trials, it was found that the air spark might, with great advantage in steadiness of action, be replaced by a vacuum-tube which offered sufficient resistance: such, for instance, may generally be found among those prepared for spectrum analysis, although these differ very widely in resistance. Lastly, the condenser was furnished with a safety discharger, consisting of a brass sphere and a point adjustable in distance from one another, so that the condenser might discharge itself at a suitable tension; *i.e.*, before the tension rose high enough to break down the insulation of the plates.

The discharge through the vacuum tubes on which experiments were being made was effected either by leading the two sides of the condenser directly to the terminals of the tube; or more often by leading one direct, and the other through the intervention of a resistance coil, such as was described in the Proceedings of the Royal Society for 1875, pp. 461—2. By altering the length of the resisting column as the tension in the condenser declined, the charge could be delivered through the tube at any required rate.

By this arrangement a steady stratified discharge can be maintained for one, two, or more minutes, according to the nature and pressure of the gas contained in the tube. In one case, with a nitrogen tube of 30 inches in length and 2 in diameter, a special fixed phase was maintained for upwards of five minutes with one of the boxes above mentioned.

Speaking in general terms, the same connexion between resistance in the circuit and the flow of the striæ as had previously been noticed with the induction coil and rapid contact-breaker (Proceedings of the Royal Society for 1875, pp. 458—9) was observed with this method; but the phenomena were exhibited with greater distinctness, and could be examined more at leisure.

In particular, with the nitrogen tube above mentioned, and other similar tubes, the direction of the flow reversed itself as the charge in the condenser became more exhausted. This was apparently due rather to a diminution in strength of current, or quantity of electricity passing through the tube, than to fall in tension, inasmuch as any particular phase could be maintained by gradually diminishing the resistance in the circuit as the tension declined. The penultimate phase was a forward flow from the positive terminal, the ultimate a fixed condition of striæ. When sufficient resistance was interposed in the circuit, these striæ showed a faint indication of fissure into pairs of laminæ, and even actually broke into pairs by forward jerks. Very shortly after this the column became blurred, and the discharge then finally ceased. It should be further mentioned that by a suitable increase or diminution of resistance in the circuit the flow could be reversed again and again at pleasure.

It has frequently been noticed that in some tubes the column of striæ shows a tendency to mobility, while in others it is comparatively fixed; in one case it appears to be in a position of unstable, in the other in a condition of stable, equilibrium. The former may generally be exemplified in hydrogen and nitrogen vacua, the latter in carbonic acid, hydrochloric acid, and other vacua. Experiments which I have recently made with another, in some respects yet more powerful, method, tend to bring out the connexion between these two classes, but I reserve an account of them for a future occasion.

Pursuing this subject further, I repeated the same experiments with an 18-inch, instead of a 4-inch coil, using as a battery either six large bichromate of potash cells, or, with still better effect, a large Gramme's machine, worked by steam. The results were in every way satisfactory. Tubes in which with the 4-inch coil the striæ were at best only imperfectly developed, or in which it was impossible to maintain the discharge for any appreciable time, were illuminated successfully in both respects; and in many cases the supply of electricity from the coil to the condenser could be so regulated as to maintain special phases for an indefinite time. The change of tint from pale salmon colour to violet-gray in (impure?) carbonic acid vacua, due to increased tension, as observed by Mr. De la Rue with his great battery, were here displayed with great brilliancy.

The advantage of the 18-inch over the 4-inch coil consisted not so much in the tension as in the quantity of electricity given off to the condenser at each secondary discharge; and it seems probable that a coil specially constructed with very thick primary and secondary, and capable of giving sparks from $\frac{1}{4}$ to $\frac{1}{2}$ inch in length, would be the most suitable instrument for the purpose. It would, of course, be necessary that the condenser should have sufficient capacity to act as a fly-wheel during the intermittence of the supply from the coil.

III. "On the Expression of the Product of any two Legendre's Coefficients by means of a Series of Legendre's Coefficients." By Professor J. C. ADAMS, M.A., F.R.S. Received November 22, 1877.

The expression for the product of two Legendre's coefficients which is the subject of the present paper, was found by induction on the 13th of February, 1873, and on the following day I succeeded in proving that the observed law of formation of this product held good generally. Having considerably simplified this proof, I now venture to offer it to the Royal Society; and, for the sake of completeness, I have prefixed to it the whole of the inductive process by which the theorem was originally arrived at, although for the proof itself only the first two steps of this process are required. The theorem seems to deserve attention, both on account of its elegance, and because it appears to be capable of useful applications.

As usual let Legendre's nth coefficient be denoted by P_n, then P_n may be defined by the equation

$$P_n=\frac{1}{2^n\lfloor n}\cdot\frac{d^n}{d\mu^n}(\mu^2-1)^n$$

It is well known that the following relation holds good between three consecutive values of the functions P, viz.:

$$(n+1)P_{n+1}=(2n+1)\mu P_n-nP_{n-1}$$

Now $P_1=\mu$

$$\therefore\ P_1P_n=\frac{n+1}{2n+1}P_{n+1}+\frac{n}{2n+1}P_{n-1}$$

Again, we have $$P_2=\frac{3}{2}\mu P_1-\frac{1}{2}$$

$$\therefore\ P_2P_n=\frac{3}{2}\mu P_1P_n-\frac{1}{2}P_n$$

$$=\frac{3}{2}\frac{n+1}{2n+1}\mu P_{n+1}+\frac{3}{2}\frac{n}{2n+1}\mu P_{n-1}-\frac{1}{2}P_n$$

Substitute for μP_{n+1} and μP_{n-1} their equivalents obtained by writing $n+1$ and $n-1$ successively for n in the above formula

$$\therefore\ P_2P_n=\frac{3}{2}\frac{(n+1)\,(n+2)}{(2n+1)\,(2n+3)}P_{n+2}$$

$$+\left\{\frac{3}{2}\frac{(n+1)^2}{(2n+1)\,(2n+3)}-\frac{1}{2}+\frac{3}{2}\frac{n^2}{(2n-1)\,(2n+1)}\right\}P_n$$

$$+\frac{3}{2}\frac{(n-1)n}{(2n-1)\,(2n+1)}P_{n-2}$$

By a slight reduction the coefficient of P_n becomes

$$\frac{n(n+1)}{(2n-1)\,(2n+3)}$$

Hence

$$P_2P_n=\frac{3}{2}\frac{(n+1)\,(n+2)}{(2n+1)\,(2n+3)}P_{n+2}+\frac{n(n+1)}{(2n-1)\,(2n+3)}P_n$$

$$+\frac{3}{2}\frac{(n-1)n}{(2n-1)\,(2n+1)}P_{n-2}$$

Again, putting $n=2$ in our original formula, we have

$$P_3=\frac{5}{3}\mu P_2-\frac{2}{3}P_1$$

$$\therefore\ P_3P_n=\frac{5}{3}\mu P_2P_n-\frac{2}{3}P_1P_n$$

$$=\frac{5}{2}\frac{(n+1)\,(n+2)}{(2n+1)\,(2n+3)}\mu P_{n+2}+\frac{5}{3}\frac{n(n+1)}{(2n-1)\,(2n+3)}\mu P_n$$

$$+\frac{5}{2}\frac{(n-1)n}{(2n-1)\,(2n+1)}\mu P_{n-2}-\frac{2}{3}\frac{n+1}{2n+1}P_{n+1}-\frac{2}{3}\frac{n}{2n+1}P_{n-1}$$

Substitute for μP_{n+2}, μP_n and μP_{n-2} their equivalents as before

$$\therefore\ P_3P_n=\frac{5}{2}\frac{(n+1)\,(n+2)\,(n+3)}{(2n+1)\,(2n+3)\,(2n+5)}P_{n+3}$$

$$+\left\{\frac{5}{2}\frac{(n+1)(n+2)}{(2n+1)(2n+3)}\frac{n+2}{2n+5}+\frac{5}{3}\frac{n(n+1)}{(2n-1)(2n+3)}\frac{n+1}{2n+1}-\frac{2}{3}\frac{n+1}{2n+1}\right\}P_{n+1}$$

$$+\left\{\frac{5}{3}\frac{n(n+1)}{(2n-1)(2n+3)}\frac{n}{2n+1}+\frac{5}{2}\frac{(n-1)n}{(2n-1)\,(2n+1)}\frac{n-1}{2n-3}-\frac{2}{3}\frac{n}{2n+1}\right\}P_{n-1}$$

$$+\frac{5}{2}\frac{(n-2)\,(n-1)n}{(2n-3)\,(2n-1)\,(2n+1)}P_{n-3}$$

By reduction the coefficient of P_{n+1} in this expression becomes

$$\frac{3}{2}\frac{n(n+1)\,(n+2)}{(2n-1)\,(2n+1)\,(2n+5)}$$

and similarly the coefficient of P_{n-1} becomes

$$\frac{3}{2}\frac{(n-1)\,n(n+1)}{(2n-3)\,(2n+1)\,(2n+3)}$$

Hence we have

$$P_3P_n=\frac{5}{2}\frac{(n+1)\,(n+2)\,(n+3)}{(2n+1)\,(2n+3)\,(2n+5)}P_{n+3}$$

$$+\frac{3}{2}\frac{n(n+1)\,(n+2)}{(2n-1)\,(2n+1)\,(2n+5)}P_{n+}$$

$$+\frac{3}{2}\frac{(n-1)\,n(n+1)}{(2n-3)\,(2n+1)\,(2n+3)}P_{n-1}$$

$$+\frac{5}{2}\frac{(n-2)\,(n-1)n}{(2n-3)\,(2n-1)\,(2n+1)}P_{n-3}$$

Again since $$P_4=\frac{7}{4}\mu P_3-\frac{3}{4}P_2$$

we have $$P_4P_n=\frac{7}{4}\mu(P_3P_n)-\frac{3}{4}(P_2P_n)$$

Whence by substituting the values found above for $P_3\,P_n$ and $P_2\,P_n$ and again for μP_{n+3}, $\mu\,P_{n+1}$, &c., we obtain

$$P_4P_n=\frac{5\,.\,7}{2\,.\,4}\frac{(n+1)\,(n+2)\,(n+3)}{(2n+1)\,(2n+3)\,(2n+5)}\left\{\frac{n+4}{2n+7}P_{n+4}+\frac{n+3}{2n+7}P_{n+2}\right\}$$

$$+\frac{3\,.\,7}{2\,.\,4}\frac{n(n+1)\,(n+2)}{(2n-1)\,(2n+1)\,(2n+5)}\left\{\frac{n+2}{2n+3}P_{n+2}+\frac{n+1}{2n+3}P_n\right\}$$

$$+\frac{3\,.\,7}{2\,.\,4}\frac{(n-1)\,n(n+1)}{(2n-3)\,(2n+1)\,(2n+3)}\left\{\frac{n}{2n-1}P_n+\frac{n-1}{2n-1}P_{n-2}\right\}$$

$$+\frac{5\,.\,7}{2\,.\,4}\frac{(n-2)\,(n-1)\,n}{(2n-3)\,(2n-1)\,(2n+1)}\left\{\frac{n-2}{2n-5}P_{n-2}+\frac{n-3}{2n-5}P_{n-4}\right\}$$

$$-\frac{3\,.\,3}{2\,.\,4}\frac{(n+1)\,(n+2)}{(2n+1)\,(2n+3)}P_{n+2}-\frac{3}{4}\frac{n(n+1)}{(2n-1)\,(2n+3)}P_n$$

$$-\frac{3\,.\,3}{2\,.\,4}\frac{(n-1)n}{(2n-1)\,(2n+1)}P_{n-2}$$

By reduction, the coefficient of P_{n+2} in this expression becomes

$$\frac{5}{2}\frac{n(n+1)\,(n+2)\,(n+3)}{(2n-1)\,(2n+1)\,(2n+3)\,(2n+7)}$$

Similarly, the coefficient of P_{n-2} becomes

$$\frac{5}{2}\frac{(n-2)\,(n-1)\,n(n+1)}{(2n-5)\,(2n-1)\,(2n+1)\,(2n+3)}$$

and finally, the coefficient of P_n becomes

$$\left(\frac{3}{2}\right)^2\frac{(n-1)\,n(n+1)\,(n+2)}{(2n-3)\,(2n-1)\,(2n+3)\,(2n+5)}$$

Hence, collecting the terms, we have

$$P_4P_n=\frac{1.3.5.7}{1.2.3.4}\frac{(n+1)(n+2)(n+3)(n+4)}{(2n+1)(2n+3)(2n+5)(2n+7)}P_{n+4}$$

$$+\frac{1.3.5}{1.2.3}\cdot\frac{1}{1}\frac{n(n+1)(n+2)(n+3)}{(2n-1)(2n+1)(2n+3)(2n+7)}P_{n+2}$$

$$+\frac{1.3}{1.2}\cdot\frac{1.3}{1.2}\frac{(n-1)n(n+1)(n+2)}{(2n-3)(2n-1)(2n+3)(2n+5)}P_n$$

$$+\frac{1}{1}\cdot\frac{1.3.5}{1.2.3}\frac{(n-2)(n-1)n(n+1)}{(2n-5)(2n-1)(2n+1)(2n+3)}P_{n-2}$$

$$+\frac{1.3.5.7}{1.2.3.4}\frac{(n-3)(n-2)(n-1)n}{(2n-5)(2n-3)(2n-1)(2n+1)}P_{n-4}$$

where the law of the terms is obvious, except perhaps as regards the succession of the factors in the several denominators.

With respect to this it may be observed that the factors in the denominator of any term P_p are obtained by omitting the factor $2p+1$ from the regular succession of five factors $(n+p-3)(n+p-1)(n+p+1)(n+p+3)(n+p+5)$.

For instance, where $p=n+4$, $2p+1=2n+9$, so that the factor $2n+9$ is to be omitted, and we have $2n+1$, $2n+3$, $2n+5$ and $2n+7$, as the remaining factors, and so of the rest.

Hence by induction we may write, supposing to fix the ideas that m is not greater than n

$$P_mP_n=\frac{1.3.5..(2m-1)}{1.2.3...\quad m}\cdot\frac{(n+1)(n+2)....(n+m)}{(2n+1)(2n+3)...(2n+2m+1)}$$
$$\times[(2n+2m+1)P_{n+m}]$$

$$+\frac{1.3.5..(2m-3)}{1.2.3..(m-1)}\cdot\frac{1}{1}\cdot\frac{n(n+1)...(n+m-1)}{(2n-1)(2n+1)...(2n+2m-1)}$$
$$\times[(2n+2m-3)P_{n+m-2}]$$
$$+\ \&c.\qquad\&c.$$

$$+\frac{1.3.5...(2m-2r-1)}{1.2.3...(m-r)}\cdot\frac{1.3.5...(2r-1)}{1.2.3....\quad r}$$
$$\times\frac{(n-r+1)(n-r+2)...(n-r+m)}{(2n-2r+1)(2n-2r+3)...(2n-2r+2m+1)}$$
$$\times[(2n+2m-4r+1)P_{n+m-2r}]$$
$$+\ \&c.\qquad\&c.$$

$$+\frac{1}{1}\cdot\frac{1.3.5..(2m-3)}{1.2.3..(m-1)}\cdot\frac{(n-m+2)(n-m+3)...(n+1)}{(2n-2m+3)(2n-2m+5)...(2n+3)}$$
$$\times[(2n-2m+5)P_{n-m+2}]$$

$$+\frac{1\,.\,3\,.\,5\,\ldots\,(2m-1)}{1\,.\,2\,.\,3\,\ldots\,\;m}\cdot\frac{(n-m+1)\,(n-m+2)\,\ldots\,n}{(2n-2m+1)\,(2n-2m+3)\,\ldots\,(2n+1)}$$
$$\times[(2n-2m+1)\,\mathrm{P}_{n-m}]$$

And it remains to verify this observed law by proving that if it holds good for two consecutive values of m, it likewise hold good for the next higher value.

If the function $\frac{1\,.\,3\,.\,5\,\ldots\,(2m-1)}{1\,.\,2\,.\,3\,\ldots\,m}$ be denoted by $\mathrm{A}(m)$, the general term of the above expression for P P_n may be very conveniently represented by

$$\frac{\mathrm{A}(m-r)\,\mathrm{A}(r)\,\mathrm{A}(n-r)}{\mathrm{A}(n+m-r)}\left(\frac{2n+2m-4r+1}{2n+2m-2r+1}\right)\mathrm{P}_{n+m-2r}$$

r being an integer which varies from 0 to m.

The fundamental property of the function A is that

$$\mathrm{A}(m+1)=\frac{2m+1}{m+1}\mathrm{A}(m)$$

$$\text{or}\quad \mathrm{A}(m)=\frac{m+1}{2m+1}\,\mathrm{A}(m+1)$$

We may interpret $\mathrm{A}(m)$ when m is zero or a negative integer, by supposing this relation to hold good generally, so that putting $m=0$, we have

$$\mathrm{A}(0)=\mathrm{A}(1)=1$$

Similarly $$\mathrm{A}(-1)=\frac{0}{-1}\mathrm{A}(0)=0$$

and hence the value of $\mathrm{A}(m)$ when m is a negative integer will be always zero.

We will now proceed to the general proof of the theorem stated above.

Let $\mathrm{Q}_{m,n}$ or Q_m simply, denote the quantity of which the general term is

$$\frac{\mathrm{A}(m-r)\mathrm{A}(r)\mathrm{A}(n-r)}{\mathrm{A}(n+m-r)}\left(\frac{2n+2m-4r+1}{2n+2m-2r+1}\right)\mathrm{P}_{n+m-2r}$$

In this expression r is supposed to vary from 0 to m, but it may be remarked that if r be taken beyond those limits, for instance if $r=-1$, or $r=m+1$, then in consequence of the property of the function A above stated, the coefficient of the corresponding term will vanish. Hence practically we may consider r to be unrestricted in value.

Similarly, let Q_{m-1} denote the quantity of which the general term is

$$\frac{\mathrm{A}(m-r)\,\mathrm{A}(r-1)\,\mathrm{A}(n-r+1)}{\mathrm{A}(n+m-r)}\left(\frac{2n+2m-4r+3}{2n+2m-2r+1}\right)\mathrm{P}_{n+m-2r+1}$$

writing $m-1$ for m and $r-1$ for r in the general term given above. Also let Q_{m+1} denote the quantity of which the general term is

$$\frac{A(m-r+1)\,A(r)\,A(n-r)}{A(n+m-r+1)}\left(\frac{2n+2m-4r+3}{2n+2m-2r+3}\right)P_{n+m-2r+1}$$

writing $m+1$ for m in the general term first given. In consequence of the evanescence of $A(m)$ when m is negative, we may in all these general terms suppose r to vary from 0 to $m+1$.

Let us assume that $Q_{m-1}=P_{m-1}P_n$, and also that $Q_m=P_mP_n$, then we have to prove that $Q_{m+1}=P_{m+1}P_n$.

As before, $(m+1)P_{m+1}+mP_{m-1}-(2m+1)\mu P_m=0$

$$\therefore (m+1)P_{m+1}P_n+mP_{m-1}P_n-(2m+1)\mu P_mP_n=0$$

Hence our theorem will be established if we prove that

$$(m+1)Q_{m+1}+mQ_{m-1}-(2m+1)\mu Q_m=0$$

Now $$Q_m=\ldots\ldots$$

$$+\frac{A(m-r+1)A(r-1)A(n-r+1)}{A(n+m-r+1)}\left(\frac{2n+2m-4r+5}{2n+2m-2r+3}\right)P_{n+m-2r+2}$$

$$+\frac{A(m-r)\,A(r)\,A(n-r)}{A(n+m-r)}\left(\frac{2n+2m-4r+1}{2n+2m-2r+1}\right)P_{n+m-2r}$$

$$+\ldots.$$

Multiplying by μ and substituting for $\mu P_{n+m-2r+2}$ and μP_{n+m-2r} &c., in terms of $P_{n+m-2r+1}$ &c., we find the coefficient of $P_{n+m-2r+1}$ in μQ_m to be

$$\frac{A(m-r+1)\,A(r-1)\,A(n-r+1)}{A(n+m-r+1)}\left(\frac{n+2m-2r+2}{2n+m-2r+3}\right)$$

$$+\frac{A(m-r)\,A(r)\,A(n-r)}{A(n+m-r)}\left(\frac{n+m-2r+1}{2n+2m-2r+1}\right)$$

Hence the coefficient of $P_{n+m-2r+1}$ in $(m+1)Q_{m+1}+mQ_{m-1}-(2m+1)\mu Q_m$ will be

$$\frac{A(m-r+1)A(r)A(n-r)}{A(n+m-r+1)}(m+1)\left(\frac{2n+2m-4r+3}{2n+2m-2r+3}\right)$$

$$-\frac{A(m-r+1)A(r-1)A(n-r+1)}{A(n+m-r+1)}(2m+1)\left(\frac{n+m-2r+2}{2n+2m-2r+3}\right)$$

$$-\frac{A(m-r)A(r)A(n-r)}{A(n+m-r)}(2m+1)\left(\frac{n+m-2r+1}{2n+2m-2r+1}\right)$$

$$+\frac{A(m-r)A(r-1)A(n-r+1)}{A(n+m-r)}\,m\left(\frac{2n+2m-4r+3}{2n+2m-2r+1}\right)$$

The sum of the first two lines of this expression is

$$\frac{A(m-r+1)\,A(r-1)\,A(n-r)}{A(n+m-r+1)\,(2n+2m-2r+3)}$$

$$\times\left\{\frac{2r-1}{r}(m+1)(2n+2m-4r+3)-\frac{2n-2r+1}{n-r+1}(2m+1)(n+m-2r+2)\right\}$$

Suppose for a moment that $n-r+1=q$, then the quantity within the brackets becomes

$$\frac{2r-1}{r}(m+1)\,(2m+1+2q-2r)-\frac{2q-1}{q}\,(2m+1)\,(m+1+q-r)$$

Now this quantity evidently vanishes when $q=r$, and therefore it is divisible by $q-r$. It also vanishes when $m+1=r$, and therefore it is likewise divisible by $m-r+1$.

Hence it is readily found that this quantity

$$=-\frac{q-r}{qr}(m-r+1)\,(2m+2q+1)$$

or
$$=-\frac{n-2r+1}{r(n-r+1)}\,(m-r+1)\,(2n+2m-2r+3)$$

So that the sum of the first two lines of the expression for the coefficient of $P_{n+m-2r+1}$ is

$$-\frac{A(m-r+1)\,A(r-1)\,A(n-r)}{A(n+m-r+1)}\left\{\frac{(m-r+1)\,(n-2r+1)}{r(n-r+1)}\right\}$$

Again, the sum of the other two lines of the expression for the coefficient of $P_{n+m-2r+1}$, is

$$\frac{A(m-r)\,A(r-1)\,A(n-r)}{A(n+m-r)\,(2n+2m-2r+1)}$$

$$\times\left\{-\frac{2r-1}{r}(2m+1)\,(n+m-2r+1)+\frac{2n-2r+1}{n-r+1}\,m(2n+2m-4r+3)\right\}$$

As before suppose $n-r+1=q$, and the quantity within the brackets becomes

$$-\frac{2r-1}{r}\,(2m+1)\,(m+q-r)+\frac{2q-1}{q}\,m(2m+1+2q-2r)$$

Now this quantity evidently vanishes when $q=r$, so that it is divisible by $q-r$. It also vanishes when $m=-q$, and therefore it is likewise divisible by $m+q$.

Hence it is readily found that this quantity

$$=\frac{q-r}{qr}\,(q+m)\,(2m-2r+1)$$

or
$$=\frac{n-2r+1}{r(n-r+1)}\,(n+m-r+1)\,(2m-2r+1)$$

and therefore the sum of the last two lines of the expression for the coefficient of $P_{n+m-2r+1}$ is

$$\frac{A(m-r)\,A(r-1)\,A(n-r)}{A(n+m-r)}$$

$$\times\left\{\frac{(n-2r+1)}{r(n-r+1)}\cdot\frac{(n+m-r+1)\;(2m-2r+1)}{2n+2m-2r+1}\right\}.$$

Hence the whole coefficient of $P_{n+m-2r+1}$ is

$$\frac{A(m-r)\,A(r-1)\,A(n-r)}{A(n+m-r+1)}\cdot\frac{n-2r+1)}{r(n-r+1}$$

$$\times\{2m-2r+1)-(2m-2r+1)\}\;=\;0.$$

And the same holds good for the coefficient of every term. Hence we finally obtain

$$(m+1)Q_{n+1}+mQ_{m-1}-(2m+1)\mu Q_m=0,$$

which establishes the theorem above enunciated.

The principle of the process employed in the above proof may be thus stated:

Every term in the value of Q_m gives rise to two terms in the value of μQ_m or in that of $(2m+1)\mu Q_m$; one of these terms is to be subtracted from the corresponding term in $(m+1)Q_{m+1}$, and the other from the corresponding term in mQ_{m-1}, and it will be found that the two series of terms thus formed identically destroy each other.

Hence we can find at once the value of the definite integral

$$\int_{-1}^{1}P_mP_nP_p\,d\mu$$

for if $p=n+m-2r$ we have

$$P_mP_n=.....+\frac{A\left(\frac{m+p-n}{2}\right)A\left(\frac{n+m-p}{2}\right)A\left(\frac{n+p-m}{2}\right)}{A\left(\frac{n+m+p}{2}\right)}\cdot\frac{2p+1}{n+m+p+1}P_p$$

$$+\;\&c.$$

Hence

$$\int_{-1}^{1}P_mP_nP_p\,d\mu$$

$$A=\frac{\left(\frac{m+p-n}{2}\right)A\left(\frac{n+m-p}{2}\right)A\left(\frac{n+p-m}{2}\right)}{A\left(\frac{n+m+p}{2}\right)}\;\frac{2p+1}{n+m+p+1}\int_{-1}^{1}(P_p)^2d\mu$$

$$=\frac{2}{n+m+p+1}A\frac{\left(\frac{m+p-n}{2}\right)A\left(\frac{n+m-p}{2}\right)A\left(\frac{n+p-m}{2}\right)}{A\left(\frac{n+m+p}{2}\right)}$$

or if $\frac{n+m+p}{2}=s$

$$\int_{-1}^{1} P_m P_n P_p d\mu = \frac{2}{2s+1} \frac{A(s-m)A(s-n)A(s-p)}{A(s)}$$

where as above

$$A(m)=\frac{1 \cdot 3 \cdot 5 \ldots (2m-1)}{1 \cdot 2 \cdot 3 \ldots m}=2^m \cdot \frac{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{5}{2} \cdot \ldots \left(m-\frac{1}{2}\right)}{1 \cdot 2 \cdot 3 \ldots m}$$

It is clear that, in order that this integral may be finite, no one of the quantities m, n, and p must be greater than the sum of the other two, and that $m+n+p$ must be an even integer.

I learn from Mr. Ferrers that, in the course of the year 1874, he likewise obtained the expression for the product of two Legendre's coefficients, by a method very similar to mine. In his work on "Spherical Harmonics," recently published, he gives, without proof, the above result for the value of the definite integral $\int_{-1}^{1} P_m P_n P_p d\mu$.

IV. "Experiments on the Colours shown by thin liquid Films under the Action of Sonorous Vibrations." By Sedley Taylor, M.A., late Fellow of Trinity College, Cambridge. Communicated by J. W. L. Glaisher, M.A., F.R.S. Received December 12, 1877.

(Plates 5 and 6.)

Professor Helmholtz remarks, at page 603 of the fourth edition of his "Tonempfindungen," that a film of soapsuds and glycerine forms, when caused to occupy the orifice of one of his "resonators," an extremely sensitive means by which to make visible the vibrations of the air within its cavity.

While I was engaged in verifying this observation, my notice was attracted to the parallel bands of colour which traversed the film, and it occurred to me to examine whether the forms of these bands were affected by the sonorous vibrations which agitated the film. A few rough trials having convinced me that they were so affected, I at once proceeded to submit the phenomena which presented themselves to a closer examination.

Having caused a film to adhere to the circular aperture of a Helmholtz resonator, and allowed the fluid to drain off until the interference-colours became visible, I set the resonator, nipple downwards, in a stand, so that the film was exactly horizontal, and then stroked with a resined bow a tuning-fork of the same pitch mounted on its resonance

box and placed near the resonator. With a tenacious film and uniform bowing, this resulted in the formation of a fixed figure, consisting of coloured bands, straight or curved, symmetrically arranged, and generally accompanied by one or more colour-vortices rotating in opposite directions. A single sweep of the bow usually sufficed to call out such a figure from a previous state of colour-chaos, and, when it was once established, careful bowing would keep it fairly steady for some little time. While, however, its form remained thus constant, its colours underwent a progressive series of changes as the thickness of the film gradually diminished. Presently there would come a complete break-up of the figure, often accompanied by a violent rotation of the whole film, after which a different figure, generally of a simpler form, would establish itself, to be in its turn, provided the film only held out long enough, replaced by another and still more simple figure. Sometimes, but much less frequently, the changes of form occurred without a separating interval of disorder, the incoming figure supplanting its predecessor with kaleidoscopic abruptness. The most interesting feature of these figures was their stationary colour-vortices, arranged pairwise, and churning round and round with a velocity which seemed limited only by the loudness of the sound at command, and by the capacity of the film for bearing the strain put upon it. The fixed bands, viewed at any one instant, mostly presented a surprising degree of complexity, with entire symmetry in the arrangement both of form and of vividly contrasting colour. The general effect may, perhaps, be compared to that of an elaborate "set-piece" of fireworks, in which turbines of coloured flame play a conspicuous part. But the great charm of the experiment lay in watching the successive changes of form and hue which, beautiful from the very outset, became, as the film neared its moment of dissolution, surpassingly gorgeous. It would, I think, be difficult to point to a more splendid series of phenomena in the whole range of physical optics.

To give anything like a complete representation of the colour-appearances observed would severely tax the utmost resources of pictorial art. The illustrations* appended to this paper are meant only to convey a general notion of the phenomena which presented themselves. The memoranda for them were carefully taken down during actual observation of the experiments, so that, in their broader features, the figures given may be safely depended on. Minute accuracy of detail could not, in dealing with such wayward phenomena, be secured. except by a greater expenditure of time than was feasible. I propose to make a few remarks on some of the colour-figures produced by the mode of experimenting described above.

Gentle bowing, and a film not too thin, usually called out fig. 1, in

* Drawn by Mr. Daniel Wood, Master of the Cambridge School of Art, who most kindly placed his skill and scanty leisure at my disposal.

which bright concentric rings contrasted with a differently tinted ground, while the whole was surrounded by a margin of mixed colours in an unorganised condition. Stronger bowing, or the progressive thinning of the film, would then establish fig. 2, the most typical and persistent of the whole series. Here concentric circular bands were enclosed by others approximately square, and these again, together with four sets of irregularly flattened ovals symmetrically disposed, by other square bands, inclined at 45° to the former ones. Outside the latter were four pairs of colour-vortices, with stationary tadpole-shaped nuclei, rotating in the alternate directions shown by the arrows. Nearly the whole space included between the outer square and the edge of the film was occupied by colours whirling in circular or slightly elliptic orbits. The contrast between the fixed and moving portions of this figure was always extremely striking and beautiful.

In fig. 3, a set of fixed concentric rings separated adjacent pairs of vortices.

Figs. 4 and 5 presented a pentagonal arrangement with five pairs of vortices. The dotted space in the latter figure appeared speckled over with minute air-bubbles.

Fig. 6 belonged to a less regular class of forms.

Fig. 7 was only once observed. Its central rings were inclosed in a series of triangular bands, outside which were three sets of flattened ovals similar to those in fig. 2, but each containing a single vortex rotating about a nucleus.

Fig. 8 usually showed itself only when the thinning of the film was far advanced. There was a flow of colour along the dotted lines in the direction of the arrow, which subsequently divided into two streams, and, after passing outside the two sets of flattened ovals and through the channels separating them from a third irregular crescent-shaped series of bands, united again opposite the protuberance in the concavity of the latter, and went on performing the same circuit. When the film had become excessively thin, this figure frequently showed nothing but the two tadpole nuclei, with an oval vortex about each, whose longer diameter was not much less than that of the film itself.

In all the preceding figures the axis of symmetry was evidently determined by the direction in which the sonorous vibrations reached the film.

The results which have been as yet described were obtained from films clinging to the circular orifice of a resonator. A simpler mode of proceeding is to form the film on an aperture cut in a piece of cardboard or thin sheet of metal, and place this upon the open end of a resonance-box, into which its appropriate tuning-fork has been previously screwed. The box is, of course, to be held steady, with its opening horizontal, while the fork is thrown into vibration. By

operating in this manner with apertures of various shapes, I obtained a large number of distinct figures: a few of the least complex have been selected for illustration.

In figs. 9, 10, 11, the aperture used was an equilateral triangle; in figs. 12, 13, 14, a square. The general character of these results will, after the explanations already given, be readily understood on inspection of the diagrams. Their axis of symmetry depended on the position of the film with reference to that of the tuning-fork.

When larger apertures, or a fork and box of higher pitch, were used, the resulting figures became rapidly more complex, and forms were readily obtained in which the whole film was covered with an uniform pattern consisting of some single figure analogous to one of those already described repeated over and over again. With very acute sounds the separate figures became too small for recognition and too numerous for counting.

The resonance of an air-cavity is, as is well known, not limited to supporting a note of one single degree of pitch, but can also reinforce other notes, provided they are not too far distant from its proper or fundamental tone. It was therefore to be expected that colour-figures could be obtained from a film in the mouth of a resonator by employing sounds other than that to which its cavity was tuned. In putting this to the test of experiment I found it convenient to replace the sounds of mounted tuning-forks by those of my own voice. The film, formed as at first in the orifice of a resonator, showed itself sensitive to very slight differences of pitch. When the note sung was somewhere in the neighbourhood of the resonator's proper tone, the smallest sharpening or flattening which my voice could produce led to an instantaneous and unmistakable change of figure. Moreover, the limits of pitch on either side of the resonator's own note, within which permanent figures could be obtained, were considerably wider than those between which audible reinforcement by the resonator itself occurred.

In order to test the capacity of two figures corresponding to tones of different pitch for coalescing into a regular resultant-form, I placed a film symmetrically with respect to the open ends of two resonance-boxes belonging to forks forming consonant intervals with each other. By exciting either fork separately a steady figure was obtained, but the coexistence of their sounds caused an immediate tumultuous movement from which no permanent joint form emerged. There seemed to be an absolute incompatibility between the two figures which made all compromise impossible. Desirous of ascertaining whether this repugnance extended to the case of sounds all but identical in pitch, I used a pair of unison forks, one of which had been slightly flattened so as to beat about twice per second with its fellow. Some very interesting phenomena were the result. The fixed

portions of the approximately identical figures due to either fork by itself, took up, while both were sounding, a swaying movement about their mean position; one complete oscillation of figure synchronizing exactly with each beat* heard. The resonance-boxes had, in this experiment, been placed with their openings exactly opposite each other, and the film, which was rectangular in shape, midway between, but in a horizontal plane slightly above them. The swaying motion was, under these circumstances, on the whole rectilinear, as though each fork alternately gave the entire figure a pull in its own direction. The behaviour of the vortices was still more remarkable. With vigorous and equal bowing they rotated several times in one direction during the first half of each beat, and the same number of times in the opposite direction during the second half of it. If, instead of occupying the relative positions above described, in which the forks when sounding singly gave rise to antagonistic vortices, they were both placed on one side of the film, the result was rotation during one half-beat and inaction during the next, followed again by similar alternations, but the direction of rotation remaining constant throughout. In this case the vortices moved most rapidly during the maximum and rested during the minimum of intensity. But in others it was not so, and I even observed instances where in one part of the figure the rotation coincided with the maximum and the quiescence with the minimum, while at another part of it the exactly contrary state of things prevailed.

In every experiment hitherto described the film was either acted on by the resonance of a spherical air-cavity, which practically reinforces only the fundamental tone of a compound sound in unison with it; or else the vibrations employed were exclusively those of a mounted tuning-fork which follow the pendulum law. Hence no other kind of movement was transmitted save that which gives rise to what Helmholtz calls a "simple tone." In order to examine the effects produced by composite sounds, it was desirable to let their vibrations act on a film unconnected with any resonant cavity. For this purpose one end of a caoutchouc tube of large bore was fitted into a metal ring fixed in a horizontal plane on which the film-bearing discs could be placed. Notes of the human voice, of tuning forks, organ pipes, &c., being sounded into the tube, either directly or through a funnel in the shape of an ordinary ear-trumpet, their effects on films of various forms and sizes could be conveniently observed. A very wide field for research was thus opened up, which I do not propose to enter upon here beyond simply mentioning one result obtained in this manner which possesses an independent interest. When two notes, identical in pitch and

* The absence of an English equivalent for the German *Schwebung*, which denotes the whole phenomena from one maximum of intensity (*Schlag*) to the next, is most inconvenient, and makes itself very perceptible here.

loudness, but differing markedly in what is called quality or *timbre*, were alternately sounded, two perfectly distinct figures were obtained, each presenting itself again and again for many alternations the instant its own note was sounded, and remaining constant until that note ceased. By this apparatus, therefore, permanent pictures of the relative quality of musical sounds may be secured.

I content myself here with a description of the phenomena I have observed, and make no attempt at determining the mechanical conditions under which they occur. It may, however, be worth while to remark that the most striking feature of the figures above described, the vortices, can be exactly reproduced with the caoutchouc tube apparatus by gently sucking a little air through it, taking care that the whole opening of the tube is not in simultaneous contact with the lips, and that its other end is not completely closed by the disc and film.

Before concluding I wish to draw attention to some allied phenomena described by Mr. E. B. Tylor in "Nature," for May, 1877, p. 12. Distinct patterns were obtained by him, but it would appear from the directions he gives for producing "a film more free from interference-colours, so as to display the vibration-figures on an almost clear ground" that no permanent colour-patterns are in question in his letter. He speaks indeed of "the gorgeous scenic effect of the masses of prismatic colour whirled hither and thither by the musical vibrations," but of nothing more fixed and regular. The mode of experimenting adopted by him on that occasion suggested the third form of apparatus described in the present paper.

February 7, 1878.

Sir JOSEPH HOOKER, K.C.S.I., 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 Comparison of the Standard Barometers of the Royal Observatory, Greenwich, and the Kew Observatory." By G. M. WHIPPLE, B.Sc., Superintendent of the Kew Observatory. Communicated by order of the Kew Committee, WARREN DE LA RUE, F.R.S., Vice-Chairman. Received November 26, 1877.

The Standard Barometers of these two important establishments, up to within a recent date, had never been compared directly, although

Fig. 1.

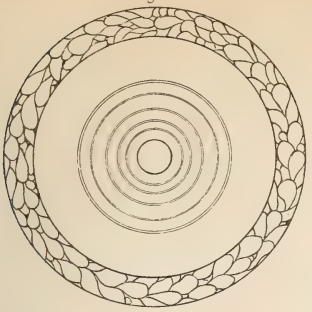


Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.

W. West & C° lith.

Fig. 8.

Fig. 9.

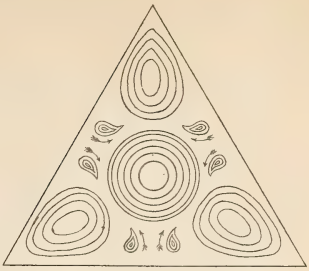


Fig. 10.

Fig. 11.

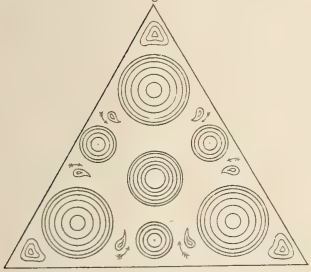


Fig. 12

Fig. 13.

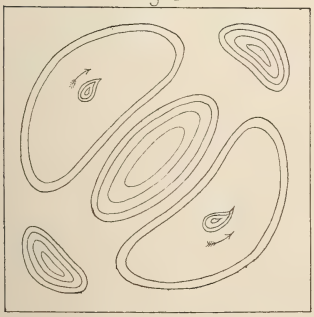
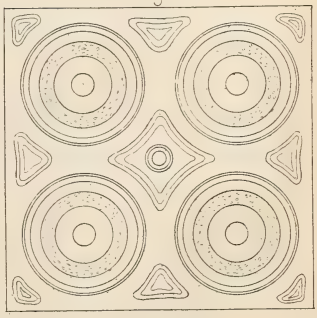


Fig. 14.



W. West & Co lith.

from time to time indirect comparisons have been made by persons not connected officially with either.

Attention has recently been drawn to the subject by the publication of a memoir by Dr. H. Wild, Director of the Central Physical Observatory, St. Petersburgh, entitled "Ueber normal-Barometer und ihrer Vergleichung," in which the author states (p. 100) that the standard barometer of the Royal Observatory reads 0·465 m.m., or 0·018 inch lower than that of the Kew Observatory, arriving at that conclusion by means of somewhat circuitous comparisons. Professor Mohn, of Christiania, in 1876, writes also that he finds the difference Kew — Greenwich to be + 0·49 m.m., or + 0·019 inch.

In 1869 the late Colonel Strange made a comparison between the two standards by means of a barometer which he had conveyed from the one observatory to the other, the results of which are stated by him to be as follows:—

By comparison No.	1	Greenwich lower	than Kew	0·009 inch.
„ „	2	„	„	0·006 „
„ „	3	„	„	0·004 „

In consequence of these statements the Kew Committee made application to the Astronomer Royal for permission to make a direct comparison between the two instruments, and received from him a courteous consent to their proposition.

Accordingly, having selected four barometers—Adie 655, Adie 657, Negretti and Zambra $\frac{C}{429}$, and Negretti and Zambra $\frac{C}{431}$, all standards on the Fortin principle—as suitable instruments for the purpose, I made arrangements to convey them to Greenwich and back safely in a carriage.

On April 23rd, 1877, Mr. Baker and I made a set of observations of the two large Kew standards, constructed by the late Mr. Welsh; Newman 34 [the working standard of the Observatory], Adie 655 and Adie 657, and subsequently of Newman 34, and the two Negretti and Zambra standards.

The latter we also compared again on the morning of the 24th, before starting for Greenwich.

Having carefully packed the instruments, accompanied by an assistant, I went to the Royal Observatory, and there suspended the barometers in the large room of the Magnetic Observatory, facing and about eight feet distant from the standard barometer (Newman 64).

After leaving the instruments about an hour, in order that they might assume the correct temperature of the room, which did not differ greatly from that of the external air at the time, we made a set of twelve readings of all the four instruments and of the Greenwich standard, myself and the assistant reading them alternately, the tem-

perature of each barometer being also noted by the observer at the time of observation.

On the 25th and 26th of April Messrs. Ellis, Nash, and other Greenwich observers made a set of nine comparisons, and on the latter day Messrs. Baker and Foster, two Kew observers, went to Greenwich and made twelve comparisons of the five instruments, and then dismounted, packed, and brought them back to the Kew Observatory, again suspending them in their places.

On April 27th the four hack barometers (to use a term happily suggested by Mr. Galton) were again read twelve times with the Kew standard of reference barometer, Newman 34, by the same three Kew observers. The result of this set of comparisons shows the mean difference between Kew and Greenwich to have been (Greenwich—Kew) = + ·0016 inch.

2nd Comparison.

The four hack barometers were again on the 1st of May compared at Kew with Newman 34, and then conveyed to Greenwich, where they were put up in the same place as before, and read again in a similar manner to that employed on the occasion of the previous visit.

On the 1st the Kew assistants made twelve sets of readings; on the 2nd and 3rd the Greenwich observers made twelve sets of comparisons. On the 4th we made a like number of sets, returning the instruments subsequently to Kew, where they were read on the 5th with the Kew standard of reference.

The result of the second series of comparisons gives a mean difference (Greenwich — Kew) + 0·0007 inch.

3rd Comparison.

Before making this comparison the tube of Adie 655 was accidentally broken, and having been repaired and refilled, its index error was somewhat changed; also barometers Negretti and Zambra $\frac{C}{429}$ and $\frac{C}{431}$ having been removed from the Observatory by the makers, two other instruments, also by Negretti and Zambra, Nos. ⌈ 903 and ⌈ 1105, were selected with which to make the comparisons.

On the 14th May these four barometers were all compared ten times with the Kew barometer (Newman 34), the observers being the same as before.

The next day they were taken to Greenwich, and hung up in the usual place. Due time having been allowed for them to become settled, and the temperature equalised, ten sets of readings were made precisely in a similar manner to the previous ones.

On the 16th the Greenwich observers made eight sets of readings, on the 17th seven sets, and on the morning of the 18th two sets.

Later on the same day Messrs. Baker and Foster made twelve sets, carrying the barometers again back to Kew, where, on the 19th, twelve sets of observations were finally taken.

On the 23rd the Kew barometer (Newman 34) was again compared with the two Normal barometers of the Observatory, and its correction found to be — 0·007 inch, the same as determined on April 23rd.

The readings at Greenwich on the 17th May were taken at a time when the pressure was rapidly falling, and a storm passing over the Observatory; and on the 18th its effect had not completely passed off, the wind being high and the temperature changing somewhat quickly.

The observations made on these two days were discordant from these causes, and have therefore been left out of the comparison, and the result of the third series of observations gives the difference (Greenwich — Kew) + 0·0014 inch.

We then have the following determinations :—

Mean difference from 1st series of	128	comparisons	+ 0·0016	inch.
,, ,, ,, 2nd ,,	144	,,	+ 0·0007	,,
,, ,, ,, 3rd ,,	72	,,	+ 0·0014	,,
Final mean of	344	,,	+ 0·0012	,,

This value is unchanged if we weight the determinations proportionally.

A correction of — 0·006 inch has been applied throughout to the readings of the Greenwich standard; this correction having been made by the Astronomer Royal to all the readings of that instrument since August 30th, 1866 (when the long sliding rod, after removal, was replaced), to make them accord with the old Royal Society standard's indications.

Throughout the observations the temperatures of all the portable barometers were observed by means of thermometers fixed to the middle of their brass tubes, having the bulbs in immediate contact with the mercurial column.

The errors of these thermometers were all carefully determined at Kew, and every reading has been corrected to the Kew standard.

The Greenwich standard barometer is provided with two thermometers—a large one with a bulb approximately of the same diameter as the barometrical column, and placed about half way up, but at a distance of 1·25 inches in front of it—and a small thermometer not divided on its stem, whose bulb is immersed in the cistern of the barometer itself.

The readings of the latter instrument do not admit of great exacti-

tude, and therefore have not been considered in the discussion, although they were made.

As the position of the Greenwich standard barometer precludes its being read by daylight, two gas burners are used to illuminate it at the time of observation, one being fixed at the level of the pointer in the cistern, the other at the barometric height. The flame of these burners is raised only at the instant of observation.

The gas lights being immediately behind the barometer, and distant about six inches, somewhat heat it, and it was thought that probably the thermometer in front of the instrument might not, perhaps, indicate the true temperature of the mercurial column and scale.

With a view to determine the amount of this heating effect, on October 8th I took six standard thermometers to Greenwich, and, attaching them to the barometer in close contact with the tube and scale, three in front and three behind, made a series of observations in order to investigate approximately the distribution of temperature throughout the instrument.

This was found to be as follows when the gas burners were turned low, as in their ordinary condition.

Mean of	Lower thermometers.	= 59°·70
,,	Middle ,, ...	= 61°·07
,,	Upper ,, ...	= 62°·46
,,	Whole ,, ...	= 61°·06
,,	Greenwich attached thermometer 61°·42	
	Correction for Index error — 0°·90 ..	= 60°·52
Difference	.. ..	= 0°·54
Mean of	Greenwich cistern thermometer	= 58°·26

When, however, the gas burners were turned on full, the temperature of the upper part of the barometer became much increased, a rise of 7·1° taking place in five minutes, whilst the thermometer attached to the barometer was only affected to the extent of 0°·8.

Great care was exercised in turning down the lights immediately after reading during the periods of comparison, so that it is assumed that no instance of such a great irregularity in the distribution of temperature ever occurred.

As from the table it appears that the temperature indicated by the barometer's attached thermometer was probably 0°·5 lower than the average temperature of the air surrounding the instrument, a correction of — 0°·4 has been applied to its readings instead of — 0°·9, the true correction for index error as determined by Mr. Ellis by direct

comparison with the Kew standard thermometer No. 515, the property of the Royal Observatory.

If the readings of the thermometer be corrected for index errors only, as is the case under ordinary circumstances when observations are made, the difference between the Greenwich and Kew standard barometers is reduced by the amount of ($0°{\cdot}5 \times 0{\cdot}0027 =$) ·0013 inch, and the two barometers differ only by the amount — 0·0001 inch, that is to say they virtually agree.

In conclusion I would beg to express my thanks to the Astronomer Royal and Messrs. Ellis and Nash for the courteous manner in which they afforded us every facility and assistance in carrying out these comparisons, and to Messrs. Baker, Foster, and Constable for the assistance they have given in the reduction of the observations, as well as in the actual comparisons.

The tables containing the detailed observations are preserved at the Kew Observatory for reference.

Note.—Since the effect of the gas burners upon the Greenwich barometer has had attention called to it, Mr. Ellis has fitted opal glass screens between the gas-jets and the barometer. By this means the temperature is rendered much more equable round the instrument.

II. "On the Diurnal Range of the Magnetic Declination as recorded at the Trevandrum Observatory." By Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at Owens College, Manchester. Received November 28, 1877.

1. The Observatory at Trevandrum was supported by His Highness the Rajah of Travancore, and its Director was Mr. J. A. Broun, F.R.S., who has recently published the first volume of the results of his labours, giving the individual observations of magnetic declination, and deducing from them conclusions of great scientific value.

Among the other results published by Mr. Broun, are the diurnal ranges of the magnetic declination at Trevandrum, for each civil day in the eleven years, 1854 to 1864. (Table LVIII, page 163.)

In one respect the treatment of the declination observations at Trevandrum differs from that pursued at the Kew Observatory, inasmuch as in the former place, where disturbances are little felt, the diurnal ranges are from all the observations.

The geographical position of the Trevandrum Observatory was as follows:—

Latitude, 8° 30′ 32″ N.
Longitude, 5h. 7m. 59s. E. of Greenwich.

A. *Annual Variation of Declination-Range.*

2. The following table exhibits mean monthly results of the decli-

nation-range, corresponding to 48 points in the year, the whole series of eleven years being taken.

Table I.—Containing monthly means (48 to the year) of the diurnal declination-range, thus:—January (0) denotes the monthly mean of which the middle date is the very commencement of the year; January (1) that of which the middle date is one quarter-month after the commencement, and so on:—

Date.	Series 1854–64.	Date.	Series 1854–64.
	′		′
January (0)	2·97	July (0)	3·46
„ (1)	2·98	„ (1)	3·44
„ (2)	3·06	„ (2)	3·37
„ (3)	3·00	„ (3)	3·41
February (0)	2·86	August (0)	3·60
„ (1)	2·70	„ (1)	3·80
„ (2)	2·50	„ (2)	4·00
„ (3)	2·41	„ (3)	4·17
March (0)	2·29	September (0)	4·15
„ (1)	2·15	„ (1)	4·03
„ (2)	2·04	„ (2)	3·66
„ (3)	1·96	„ (3)	3·15
April (0)	2·06	October (0)	2·73
„ (1)	2·24	„ (1)	2·40
„ (2)	2·42	„ (2)	2·27
„ (3)	2·65	„ (3)	2·26
May (0)	2·87	November (0)	2·35
„ (1)	3·04	„ (1)	2·55
„ (2)	3·25	„ (2)	2·72
„ (3)	3·25	„ (3)	2·90
June (0)	3·29	December (0)	3·07
„ (1)	3·30	„ (1)	3·08
„ (2)	3·32	„ (2)	3·12
„ (3)	3·42	„ (3)	3·02

3. It will be noticed from this table that the annual variation of the declination-range at Trevandrum is very different in character from its annual variation at the Kew Observatory.

The chief feature of the annual variation at Kew is the small value of the range at the time of the winter solstice.

On the other hand, the chief features of the annual variation of declination-range at Trevandrum are two minima, nearly corresponding in time to the two equinoxes, and two maxima, one large, some time in August (after the summer solstice) and a smaller one about the time of the winter solstice. These features have been already remarked by Mr. Broun, who has likewise pointed out that the character of the diurnal variation of the declination at the summer solstice is at Trevandrum the opposite of its character at the winter solstice—the change from the one type to the other taking place about the equinoxes. He has likewise pointed out, as a result of this

change of type, that about the times of the equinoxes there is not only a small average value of the range, but likewise an uncertainty about its character, as it sometimes appears to be of the one type and at other times changes to the other type.

B. *Variations of Long Period.*

4. In order to investigate the long-period variation of the Trevandrum declination-range, I have treated these observations precisely in the way in which the Kew declination-ranges were treated (Proc. Roy. Soc., March 22, 1877). By this method proportional values of the declination-range at Trevandrum have been obtained corresponding to weekly points for each year, and it is believed that these values are freed from any recognised inequality depending either on the month of the year or on the relative position of the sun and moon. If this method should be found to furnish nearly the same results in the case of two observatories so widely apart as Kew and Trevandrum, and with such marked differences in the annual variation of the declination-range, we may conclude that this separation of inequalities has been successfully accomplished. The proportional values of the Trevandrum declination-range are given in Table II.

TABLE II.—Exhibiting monthly means of declination-range for weekly points, the mean value of the range for the whole series for each point being reckoned = 1,000.

(The date of the first point for each year is given.)

1854. Jan. 4	1855. Jan. 3.	1856. Jan. 2.	1857. Jan. 7.	1858. Jan. 6.	1859. Jan. 5.	1860. Jan. 4.	1861. Jan. 2.	1862. Jan. 1.	1863. Jan. 7.	1864. Jan. 6.
1078	876	805	873	1163	1126	1166	945	1093	1132	844
1038	888	832	891	1118	1132	1099	889	1075	1122	890
943	915	917	918	1113	1128	1094	908	1014	1189	910
932	909	976	896	1062	992	1120	931	959	1260	981
934	942	1000	928	988	1012	1113	846	998	1204	1054
944	955	946	978	1050	1005	1157	854	947	1120	1076
976	894	853	989	999	1110	1203	823	1011	1026	1089
950	922	765	928	1007	1183	1292	951	1056	1010	958
895	907	707	890	1094	1157	1411	1030	1050	990	889
925	895	694	786	1095	1221	1367	1060	1080	1074	796
939	931	733	764	1098	1116	1416	1066	1037	1107	806
962	929	767	789	1083	1064	1454	986	1031	939	945
1011	978	802	761	1160	1105	1290	981	971	962	967
970	955	850	779	1111	1094	1406	996	977	919	948
1030	934	829	804	1124	1206	1288	925	952	990	922
999	983	764	877	1033	1291	1206	903	974	1030	915
1023	935	707	1027	960	1283	1219	945	981	1023	920
1106	940	669	1053	1013	1239	1155	895	916	1074	928
1040	949	683	1053	992	1177	1185	932	951	1040	982
1042	873	731	992	1069	1117	1189	985	942	1079	1025
996	895	748	877	1015	1099	1205	965	1021	1068	1098
927	878	755	856	970	1147	1181	1031	1091	1036	1137
939	882	755	805	953	1130	1175	1096	1098	1021	1143
950	918	780	827	962	1096	1183	1111	1099	975	1088

1854. Jan. 4.	1855. Jan. 3.	1856. Jan. 2.	1857. Jan. 7.	1858. Jan. 6.	1859. Jan. 5.	1860. Jan. 4.	1861. Jan. 2.	1862. Jan. 1.	1863. Jan. 7.	1864. Jan. 6.
951	924	837	790	1012	1106	1220	1076	1103	985	1004
993	884	842	816	1030	1079	1197	1071	1069	1017	1009
970	826	828	857	1036	1106	1176	1123	1034	1103	954
932	840	826	845	1096	1112	1175	1086	1010	1114	967
923	852	855	921	1041	1123	1117	1135	981	1085	954
890	872	892	846	1060	1039	1212	1219	948	1040	969
888	883	900	810	1108	946	1410	1152	917	989	1001
890	848	919	866	1053	965	1485	1169	886	914	1018
883	842	866	813	1041	1046	1469	1251	881	885	1032
832	817	859	882	1017	1204	1473	1173	910	818	1040
877	900	864	968	919	1290	1322	1129	953	771	1010
894	940	847	1051	881	1328	1195	1178	921	776	1010
927	926	833	1160	900	1221	1177	1145	902	789	1045
1016	974	823	1189	890	1117	1144	1139	918	796	978
950	914	809	1118	919	1147	1134	1201	966	858	958
943	870	756	977	910	1251	1163	1165	1094	950	928
916	877	806	852	923	1339	1148	1081	1144	990	879
885	856	796	852	1054	1311	1032	1141	1149	992	911
949	813	812	901	1117	1260	1023	1091	1072	972	948
946	757	837	927	1235	1178	1018	1058	1048	984	996
987	715	857	966	1308	1119	1034	1102	1043	930	947
986	740	880	905	1168	1171	1098	1002	1107	1013	914
988	749	983	846	1147	1104	1159	992	1163	986	908
990	713	991	858	1044	1269	1118	1033	1129	1016	874
919	712	986	989	955	1336	1053	1019	1086	994	946
896	716	950	1031	983	1353	1007	1094	1057	929	997
854	702	872	1159	971	1377	934	1141	1076	884	1054
869	768	851	1211	1142	1160	867	1151	1054	853	1046
		865						1088		

5. The numbers of Table II have next been dealt with precisely in the way in which the corresponding numbers were dealt with in the case of the Kew Observatory, that is to say, a set of nine-monthly values of declination-range have been obtained corresponding to similar nine-monthly values of spotted solar area. These are exhibited in the following tables.

Table III.—Declination-Range, Nine-Monthly Values.

	1854.	1855.	1856.	1857.	1858.	1859.	1860.	1861.	1862.	1863.	1864.
Jan. (2)	..	930	798	882	1033	1095	1239	1000	1044	1066	938
Feb. (2)	..	924	783	889	1025	1117	1235	988	1034	1070	963
Mar. (2)	..	916	789	888	1039	1118	1232	993	1027	1062	969
April (2)	..	903	806	881	1052	1112	1245	1009	1011	1048	971
May (2)	956	904	814	889	1038	1128	1252	1032	995	1018	983
June (2)	952	900	805	903	1023	1146	1247	1062	998	986	980
July (2)	953	883	813	905	1029	1158	1222	1074	1006	974	981
Aug. (2)	948	858	830	924	1029	1172	1182	1085	1017	970	984
Sept. (2)	934	845	849	946	1032	1169	1146	1100	1037	955	
Oct. (2)	925	841	870	964	1041	1173	1109	1098	1049	949	
Nov. (2)	920	829	875	993	1056	1203	1087	1091	1046	936	
Dec. (2)	925	819	866	1022	1066	1236	1048	1068	1052	925	

I. Solar spotted Area.
II. Kew Declination-Range.
III. Trevandrum Declination-Range.
IV. Mean of (II) and (III).

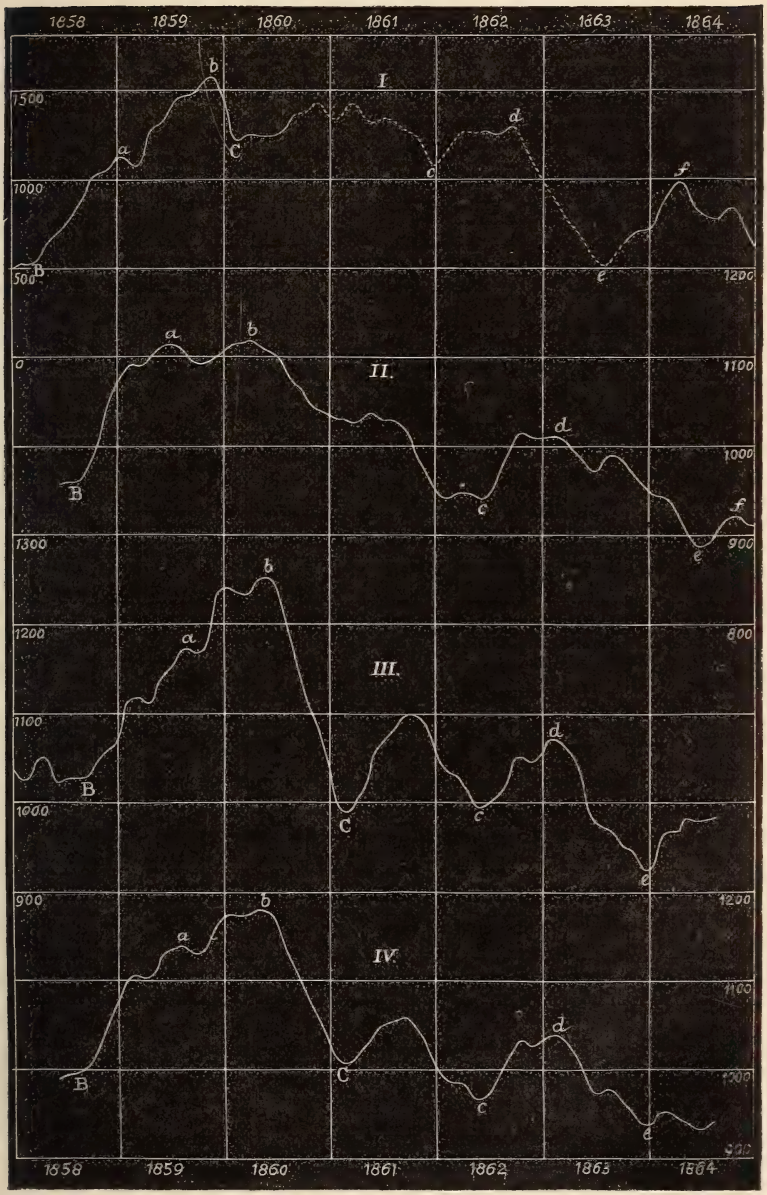


TABLE IV.—Spotted Solar Areas, Nine-Monthly Values.

	1853.	1854.	1855.	1856.	1857.
January (2).....	475	214	170	17	87*
February (2).....	490	173	155	17	101
March (2).....	475	141	138	16	117
April (2).....	450	126	112	21	144
May (2).....	438	127	83	27	182
June (2).....	434	131	48	26	214
July (2).....	425	140	20	26	235
August (2).....	390	147	15	32	287
September (2).....	353	165	15	40	350
October (2).....	318	191	15	44	400
November (2).....	283	193	16	51	450
December (2).....	252	182	17	70	479

6. The results of Tables III and IV are exhibited in the diagram which accompanies this paper.

In fig. 1 we have a curve representing the nine-monthly values of spotted area.

In fig. 2 we have the Kew and in fig. 3 the Trevandrum declination curve represented by nine-monthly values of the proportional numbers.

In fig. 4 we have a curve representing the mean between the proportional numbers of Kew and those of Trevandrum.

From these figures it will be seen that a lagging behind the sun is a feature both of the Kew and the Trevandrum curves, while generally the prominent points in the Kew and Trevandrum curves agree well together in point of time.

On the whole it would appear that by taking the mean of the proportional numbers for the two stations, we get a curve that represents the solar curve better than one derived from a single station.

The whole period compared together represents both for the solar curve (fig. 1) and the mean curve (fig. 4), a series of three smaller periods, one extending from B to C and embracing the maximum; another extending from C to *c*, and a third from *c* to *e*; and this is as far as the observations common to both stations allow us to go in point of time.

7. It may be of interest to compare, by means of the tables, the period between the solar minimum of 1855 and that of 1867, with the period between the corresponding declination-range minima. The first of these declination minima occurred at Trevandrum (the Kew observations not having then begun) on February 15, 1856, and the second of

* The numbers for the years 1858–64 are given in Proceedings of Royal Society, March 22, 1877, page 109.

them occurred at Kew (the Trevandrum observations having been discontinued) on August 15, 1867. The period is thus one of eleven years and six months.

On the other hand, the sun-spot period is that between September 15, 1855, and March 15, 1867, being likewise eleven years and six months.

C. *Variations which seem to depend on Planetary Configurations.*

8. In a paper on the Kew declination-range already alluded to, it was shown that the planetary periods of most frequent occurrence appear to be well indicated by the results of sixteen years' observations. Indeed, for the two periods of shortest length—that of Mercury about the sun, and that of Mercury and Jupiter, it was found that half of the observations gave a result of the same character as the whole sixteen years.

From this we might conclude that these periods will probably (if they have a real existence) be indicated by the Trevandrum observations.

It will be seen from the following tables that the Trevandrum declination-ranges give results for these two planetary periods very similar to those given by the Kew observations.

TABLE V.—Period of Mercury about the Sun.

(0° denoting Perihelion—65 sets for Kew—47 for Trevandrum.)

	°		°	Kew.	Trevandrum.
Between	0	and	30	+429	+263
,,	30	,,	60	+433	+223
,,	60	,,	90	+256	+237
,,	90	,,	120	+ 5	+300
,,	120	,,	150	−280	+150
,,	150	,,	180	−439	−433
,,	180	,,	210	−413	−879
,,	210	,,	240	−279	−740
,,	240	,,	270	−140	−263
,,	270	,,	300	+ 13	+333
,,	300	,,	330	+158	+680
,,	330	,,	360	+278	+506

TABLE VI.—Period of Conjunction of Mercury and Jupiter.

(0° denoting Conjunction—63 sets for Kew—43 sets for Trevandrum.)

				Kew.	Trevandrum.
Between	0°	and	30°	+633	+453
,,	30	,,	60	+759	+270
,,	60	,,	90	+652	+129
,,	90	,,	120	+328	−118
,,	120	,,	150	−119	−384
,,	150	,,	180	−504	−467
,,	180	,,	210	−678	−487
,,	210	,,	240	−677	−407
,,	240	,,	270	−548	−122
,,	270	,,	300	−322	+223
,,	300	,,	330	− 10	+415
,,	330	,,	360	+343	+503

I desire, in conclusion, to thank Mr. William Dodgson, who has given me much assistance in the calculations and diagrams of this paper.

III. "Note on the Value of Euler's Constant; likewise on the Values of the Napierian Logarithms of 2, 3, 5, 7, and 10, and of the Modulus of common Logarithms, all carried to 260 places of Decimals." By Professor J. C. ADAMS, M.A., F.R.S. Received December 6, 1877.

In the "Proceedings of the Royal Society," vol. xix, pp. 521, 522, Mr. Glaisher has given the values of the logarithms of 2, 3, 5, and 10, and of Euler's constant to 100 places of decimals, in correction of some previous results given by Mr. Shanks.

In vol. xx, pp. 28 and 31, Mr. Shanks gives the results of his re-calculation of the above-mentioned logarithms and of the modulus of common logarithms to 205 places, and of Euler's constant to 110 places of decimals.

Having calculated the value of 31 Bernoulli's numbers, in addition to the 31 previously known, I was induced to carry the approximation to Euler's constant to a much greater extent than had been before practicable. For this purpose I likewise re-calculated the values of the above-mentioned logarithms, and found the sum of the reciprocals of the first 500 and of the first 1000 integers, all to upwards of 260 places of decimals. I also found two independent relations between the logarithms just mentioned and the logarithm of 7, which furnished a test of the accuracy of the work.

On comparing my results with those of Mr. Shanks, I found that the latter were all affected by an error in the 103rd and 104th places of decimals, in consequence of an error in the 104th place in the determination of $\log \frac{81}{80}$. With this exception, the logarithms given by Mr. Shanks were found to be correct to 202 places of decimals.

The error in the determination of $\log_\epsilon 10$, of course entirely vitiated Mr. Shanks' value of the modulus from the 103rd place onwards. As he gives the complete remainder, however, after the division by his value of $\log_\epsilon 10$, I was enabled readily to find the correction to be applied to the erroneous value of the modulus. Afterwards I tested the accuracy of the entire work by multiplying the corrected modulus by my value of $\log_\epsilon 10$.

Mr. Shanks' values of the sum of the reciprocals of the first 500 and of the first 1000 integers, as well as his value of Euler's constant, were found to be incorrect from the 102nd place onwards.

Let S_n, or S simply, when we are concerned with a given value of n, denote the sum of the harmonic series,

$$1+\frac{1}{2}+\frac{1}{3}+\ldots\ldots+\frac{1}{n}$$

Also let R_n, or R simply, denote the value of the semi-convergent series,

$$\frac{B_1}{2n^2}-\frac{B_2}{4n^4}+\frac{B_3}{6n^6}-\ldots\ldots$$

where B_1, B_2, B_3, &c., are the successive Bernoulli's numbers.

Then if Euler's constant be denoted by E, we shall have

$$E=S_n+R_n-\frac{1}{2n}-\log_\epsilon n,$$

and the error committed by stopping at any term in the convergent part of R_n will be less than the value of the next term of the series.

I have calculated accurately the values of the Bernoulli's numbers as far as B_{62}, and approximately as far as B_{100}, retaining a number of significant figures varying from 35 to 20.

When $n=1000$, the employment of the numbers up to B_{61} suffices to give the value of R_{1000} to 265 places of decimals. When $n=500$, it is necessary to employ the approximate values up to B_{74}, in order to determine R_{500} with an equal degree of exactness.

In order to reduce as much as possible the number of quantities which must be added together to find S_{500} and S_{1000}, I have resolved the reciprocal of every integer up to 1000 into fractions whose denominators are primes or powers of primes.

Thus S_{500} and S_{1000} may be expressed by means of such fractions, and by adding or subtracting one or more integers, each of these

fractions may be reduced to a positive proper fraction, the value of which in decimals may be taken from Gauss' Table, in the second volume of his collected works, or calculated independently.

Thus I have found that:—

$$S_{500} = \frac{249}{256} + \frac{2}{81} + \frac{3}{5} + \frac{120}{343} + \frac{3}{121} + \frac{86}{169} + \frac{205}{289} + \frac{58}{361} + \frac{1}{23} + \frac{3}{29}$$
$$+ \frac{21}{31} + \frac{30}{37} + \frac{11}{41} + \frac{15}{43} + \frac{26}{47} + \frac{32}{53} + \frac{24}{59} + \frac{33}{61} + \frac{27}{67} + \frac{67}{71} + \frac{28}{73} + \frac{38}{79}$$
$$+ \frac{73}{83} + \frac{72}{89} + \frac{33}{97} + \frac{61}{101} + \frac{45}{103} + \frac{11}{107} + \frac{102}{109} + \frac{68}{113} + \frac{23}{127} + \frac{111}{131}$$
$$+ \frac{116}{137} + \frac{25}{139} + \frac{126}{149} + \frac{27}{151} + \frac{28}{157} + \frac{29}{163} + \frac{85}{167} + \frac{88}{173} + \frac{91}{179} + \frac{92}{181}$$
$$+ \frac{97}{191} + \frac{98}{193} + \frac{100}{197} + \frac{101}{199} + \frac{107}{211} + \frac{113}{223} + \frac{115}{227} + \frac{116}{229} + \frac{118}{233} + \frac{121}{239}$$
$$+ \frac{122}{241}$$
$$+ (\text{the sum of the reciprocals of the primes from 251 to 499}) - 19.$$

Similarly I have found that:—

$$S_{1000} = \frac{249}{512} + \frac{310}{729} + \frac{181}{625} + \frac{75}{343} + \frac{62}{121} + \frac{35}{169} + \frac{220}{289} + \frac{11}{361} + \frac{300}{529} + \frac{726}{841}$$
$$+ \frac{32}{961} + \frac{34}{37} + \frac{21}{41} + \frac{10}{43} + \frac{40}{47} + \frac{48}{53} + \frac{28}{59} + \frac{56}{61} + \frac{7}{67} + \frac{31}{71} + \frac{40}{73} + \frac{45}{79}$$
$$+ \frac{25}{83} + \frac{49}{89} + \frac{44}{97} + \frac{69}{101} + \frac{82}{103} + \frac{90}{107} + \frac{104}{109} + \frac{12}{113} + \frac{67}{127} + \frac{84}{131} + \frac{121}{137}$$
$$+ \frac{85}{139} + \frac{144}{149} + \frac{10}{151} + \frac{26}{157} + \frac{141}{163} + \frac{83}{167} + \frac{34}{173} + \frac{53}{179} + \frac{132}{181} + \frac{171}{191}$$
$$+ \frac{102}{193} + \frac{196}{197} + \frac{125}{199} + \frac{90}{211} + \frac{95}{223} + \frac{21}{227} + \frac{212}{229} + \frac{138}{233} + \frac{22}{239} + \frac{223}{241}$$
$$+ \frac{211}{251} + \frac{216}{257} + \frac{221}{263} + \frac{226}{269} + \frac{47}{271} + \frac{48}{277} + \frac{236}{281} + \frac{49}{283} + \frac{246}{293} + \frac{53}{307}$$
$$+ \frac{261}{311} + \frac{54}{313} + \frac{266}{317} + \frac{57}{331} + \frac{170}{337} + \frac{175}{347} + \frac{176}{349} + \frac{178}{353} + \frac{181}{359} + \frac{185}{367}$$
$$+ \frac{188}{373} + \frac{191}{379} + \frac{193}{383} + \frac{196}{389} + \frac{200}{397} + \frac{202}{401} + \frac{206}{409} + \frac{211}{419} + \frac{212}{421} + \frac{217}{431}$$
$$+ \frac{218}{433} + \frac{221}{439} + \frac{223}{443} + \frac{226}{449} + \frac{230}{457} + \frac{232}{461} + \frac{233}{463} + \frac{235}{467} + \frac{241}{479} + \frac{245}{487}$$
$$+ \frac{247}{491} + \frac{251}{499}$$
$$+ (\text{the sum of the reciprocals of the primes from 503 to 997}) - 43.$$

This mode of finding S_{500} and S_{1000} is attended with the advantage that if an error were made in the calculation of the former of these quantities, it would not affect the latter.

The logarithms required have been found in the following manner:—

Let $\log \frac{10}{9}=a$, $\log \frac{25}{24}=b$, $\log \frac{81}{80}=c$, $\log \frac{50}{49}=d$, and $\log \frac{126}{125}=e$.

Then we have

$$\log 2=7a-2b+3c,\ \log 3=11a-3b+5c,\ \log 5=16a-4b+7c.$$

Also $$\log 7=\tfrac{1}{2}\,(39a-10b+17c-d)\,;$$

or again, $$\log 7=19a-4b+8c+e,$$

and we have the equation of condition,

$$a-2b+c=d+2e,$$

which supplies a sufficient test of the accuracy of the calculations by which a, b, c, d, and e have been found.

$$\text{Since } \log \frac{10}{9}=-\log\left(1-\frac{1}{10}\right)$$

$$\log \frac{25}{24}=-\log\left(1-\frac{4}{100}\right)$$

$$\log \frac{81}{80}=\log\left(1+\frac{1}{80}\right)$$

$$\log \frac{50}{49}=-\log\left(1-\frac{2}{100}\right)$$

$$\log \frac{126}{125}=\log\left(1+\frac{8}{1000}\right)$$

If we have settled beforehand on the number of decimal places which we wish to retain, and have already formed the decimal values of the reciprocals of the successive integers to the extent required, then the formation of the values of a, b, c, d, and e, will only involve operations which, though numerous, are of extreme simplicity.

In this way have been found the following results:—

Log 10÷9= ·10536 05156 57826 30122 75009 80839 31279 83061 20372 98327
40725 63939 23369 25840 23240 13454 64887 65695 46213 41207
66027 72591 03705 17148 67351 70132 21767 11456 06836 27564
22686 82765 81669 95879 19464 85052 49713 75112 78720 90836
46753 73554 69033 76623 27864 87959 35883 39553 19538 32230
68063 73738 05700 33668 65

Log 25÷24= ·04082 19945 20255 12955 45770 65155 31987 01772 11747 63352
02297 28561 42083 06828 16287 62241 55690 62020 38337 10701
85958 13391 57612 02856 02344 55254 44440 90711 64191 09254
90615 87090 13793 32587 08185 56690 89768 86470 69797 42768
97243 12354 16791 64980 33118 36535 36811 73829 09383 64151
16223 48133 67972 69296

Log 81÷80= ·01242 25199 98557 15331 12931 28631 20890 67623 60339 58145
90685 43409 40510 22236 97287 99924 04408 75833 17607 39941
83907 88915 98331 57135 00593 07313 64880 85644 69078 59065
10006 71375 61155 92285 64823 02773 78467 95356 20673 20672
56121 24774 48623 61600 82118 41837 57253 45313 78157 48027
60627 91715 42041 36587 2

Log 50÷49= ·02020 27073 17519 44840 80453 01024 19238 78525 33383 73356
83210 27195 49256 65918 71880 87170 92908 14086 00703 48551
55810 69865 22995 29709 68602 61790 51909 27000 19877 96234
68586 52194 37909 61418 83597 32774 05301 16399 74760 65371
30928 59153 97434 74168 79079 46094 49807 56880 62620 29129
95963 65850 08854 45

Log 126÷125= ·00796 81696 49176 87351 07973 39067 84478 84307 61916 78206
21803 11515 15228 34251 08036 00862 32503 51700 93221 55597
11104 32429 31908 69430 97326 52573 22928 44338 63827 35942
41437 63883 38664 80785 92159 70835 21671 40563 92519 30299
88730 07233 43319 67047 32333 55315 84852 90164 08154 11413
00140 51668 01463 4832

All these are Napierian logarithms.

The above-mentioned equation of condition is satisfied to 263 places of decimals.

Whence have been deduced the following :—

$\text{Log}_e 2 =$ ·69314 71805 59945 30941 72321 21458 17656 80755 00134 36025
52541 20680 00949 33936 21969 69471 56058 63326 99641 86875
42001 48102 05706 85733 68552 02357 58130 55703 26707 51635
07596 19307 27570 82837 14351 90307 03862 38916 73471 12335
01153 64497 95523 91204 75172 68157 49320 65155 52473 41395
25882 95045 30081 06850 15

$\text{Log}_e 3 = 1$ ·09861 22886 68109 69139 52452 36922 52570 46474 90557 82274
94517 34694 33363 74942 93218 60896 68736 15754 81373 20887
87970 02906 59578 65742 36800 42259 30519 82105 28018 70767
27741 06031 62769 18338 13671 79373 69884 43609 59903 74257
03167 95911 52114 55919 17750 67134 70549 40166 77558 02222
03170 25294 68992 45403 15

$\text{Log}_e 5 = 1$ ·60943 79124 34100 37460 07593 33226 18763 95256 01354 26851
77219 12647 89147 41789 87707 65776 46301 33878 09317 96107
99966 30302 17155 62899 72400 52293 24676 19963 36166 17463
70572 75521 79637 49718 32456 53492 85620 23415 25057 27015
51936 00879 77738 97256 88193 54071 27661 54731 22180 95279
48521 29282 13604 17624 80

$\text{Log}_e 7 = 1$ ·94591 01490 55313 30510 53527 43443 17972 96370 84729 58186
11884 59390 14993 75798 62752 06926 77876 58498 58787 15269
93061 69420 58511 40911 72375 22576 77786 84314 89580 95163
90077 59078 24468 10427 47833 82259 34900 84673 74412 50497
37048 53551 76783 55774 86240 15102 77418 08868 67107 51412
13480 93879 74210 03537 95

$\text{Log}_e 10 = 2$ ·30258 50929 94045 68401 79914 54684 36420 76011 01488 62877
29760 33327 90096 75726 09677 35248 02359 97205 08959 82983
41967 78404 22862 48633 40952 54650 82806 75666 62873 69098
78168 94829 07208 32555 46808 43799 89482 62331 98528 39350
53089 65377 73262 88461 63366 22228 76982 19886 74654 36674
74404 24327 43685 24474 95

M = ·43429 44819 03251 82765 11289 18916 60508 22943 97005 80366
65661 14453 78316 58646 49208 87077 47292 24949 33843 17483
18706 10674 47663 03733 64167 92871 58963 90656 92210 64662
81226 58521 27086 56867 03295 93370 86965 88266 88331 16360
77384 90514 28443 48666 76864 65860 85135 56148 21234 87653
43543 43573 17247 48049 05993 55353 05

where M denotes the modulus of common logarithms.

In these calculations the value of $\log \frac{50}{49}$ has been determined with less accuracy than that of $\log \frac{126}{125}$, and therefore the value of log 7 found by means of the latter quantity has been preferred.

If now in the formula which gives Euler's constant we take $n = 500$, we find the following results:—

$$\frac{1}{2n} = 0{\cdot}001$$

R_{500} = ·00000 03333 33200 00025 39671 87309 34479 09501 49853 06920
81561 41982 03143 98353 10049 47690 35814 25947 82825 73530
80967 33251 23444 83365 27221 32891 79715 39888 78668 70158
11997 43277 84264 18919 84678 56672 58294 26067 37401 94207
08483 64907 04495 03811 66583 11699 18899 16275 81704 82573
08004 99446 91635

S_{500} = 6·79282 34299 90524 60298 92871 45367 97369 48198 13814 39677
91166 43088 89685 43566 23790 55049 24576 49403 73586 56039
17565 98584 37506 59282 23134 68847 97117 15030 24984 83148
07266 84437 10123 70203 14772 22094 00570 47964 42959 21001
09719 01932 14586 27077 01576 02007 28842 06850 09735 01135
74118 52998 6631

Log_e 500 = 6·21460 80984 22191 74263 67422 42594 91605 47278 04331 52606
36739 79303 69340 93242 07062 36272 51021 28288 27237 62074
83901 87110 62880 60166 54305 61594 90289 71296 61913 55661
26910 65179 94054 14829 26073 41092 64585 48079 22114 05716
58115 31635 24264 74180 14925 98528 81625 94504 71489 68628
97329 77937 00975

E = ·57721 56649 01532 86060 65120 90082 40243 10421 59335 93992
35988 05767 23488 48677 26777 66467 09369 47063 29174 67495
14631 44724 98070 82480 96050 40144 86542 83622 41739 97644
92353 62535 00333 74293 73377 37673 94279 25952 58247 09491
60087 35203 94816 56708 53233 15177 66115 28621 19950 15079
84793 74508 5697

Again, if in the same formula we take $n = 1000$, we find the following:—

$$\frac{1}{2n} = 0{\cdot}0005$$

R_{1000} = ·00000 00833 33325 00000 39682 49801 59487 73237 84632 11743
88611 32124 18782 98862 06644 51967 06850 04241 14869 65631
43736 78499 44114 24665 37423 82138 50259 70190 89962 61572
33894 07843 88131 36054 55889 69002 08034 44545 27898 47738
31546 74821 27649 54293 18527 10448 88349 55931 43201 82238
86978 52223 81562

S_{1000}= 7·48547 08605 50344 91265 65182 04333 90017 65216 79169 70880
36657 73626 74995 76993 49165 20244 09599 34437 41184 50813
96798 01438 22544 03715 81484 21958 84703 40431 40398 43368
92966 39178 33827 35905 57913 00071 54692 68403 25933 79804
87809 56515 86955 67800 24804 71415 08712 32350 00711 42865
21027 95267 06455

$\log_\epsilon 1000$= 6·90775 52789 82137 05205 39743 64053 09262 28033 04465 88631
89280 99983 70290 27178 29032 05744 07079 91615 26879 48950
25903 35212 68587 45900 22857 63952 48420 26999 88621 07296
34506 84487 21624 97666 40425 31399 68447 86995 95585 18051
59268 96133 19788 65384 90098 66686 30946 59660 23963 10024
23212 72982 31056

E= ·57721 56649 01532 86060 65120 90082 40243 10421 59335 93992
35988 05767 23488 48677 26777 66467 09369 47063 29174 67495
14631 44724 98070 82480 96050 40144 86542 83622 41739 97644
92353 62535 00333 74293 73377 37673 94279 25952 58247 09491
60087 35203 94816 56708 53233 15177 66115 28621 19950 15079
84793 74508 56961

It will be seen that the two values found for E agree to 263 places of decimals, which supplies another independent verification of the value obtained for $\log_\epsilon 2$.

February 14, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

The Right Hon. William Henry Smith and the Right Hon. Sir William Henry Gregory, whose certificates had been suspended as required by the Statutes, were balloted for and elected Fellows of the Society.

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

The following Papers were read:—

I. "Concerning the Effects on the Heart of Alternate Stimulation of the Vagi." By ARTHUR GAMGEE, M.D., F.R.S., Brackenbury Professor of Physiology in Owens College, and JOHN PRIESTLEY, Assistant Lecturer in Physiology in Owens College. Received December 15, 1877.

In 1869, A. B. Meyer* observed that it was impossible to stop the heart in dogs and rabbits continuously by stimulation of the vagus

* A. B. Meyer "Das Hemmungsnervensystem des Herzens." (Abstract by Schiffer, "Centralblatt," 1869, No. 14, p. 216.)

nerve, even when the nerves were alternately stimulated so as to relieve one another.

In 1875, Tarchanoff and Puelma* stated, "Si l'on excite longtemps *un* des pneumogastriques du chien avec des courants forts jusqu'à épuiser complétement son action sur le cœur, ce qui se manifeste par le retour de ses battements et si l'on passe *immédiatement à l'excitation de l'autre*, on n'obtient plus d'arrêt du cœur, et même on n'observe aucune altération de son activité : et pourtant le nerf excité en dernier lieu n'est alors, on le conçoit, nullement épuisé." From this experiment they concluded, "que chacun des pneumogastriques met en jeu tout l'appareil modérateur situé dans les parois du cœur ; et qu'une fois cet appareil épuisé par l'excitation d'un pneumogastrique, il ne peut être mis en activité par l'excitation de l'autre."

In 1876, Tarchanoff† published observations on the frog, in which he states that the behaviour of the frog's heart to vagus-stimulation is quite different, in respect of the mutual influence of the two nerves, from the behaviour of the heart of dogs or rabbits: "J'ai vu alors que si l'on attend pour exciter le second pneumogastrique que le premier ait été épuisé par l'excitation, l'arrêt du cœur s'obtient de la façon la plus nette ;" and he concludes "Que chez les mammifères les deux nerfs aboutissent à un appareil modérateur commun, tandis que chez la grenouille chaque nerf aboutit à un appareil indépendant."

Between the publication of Tarchanoff and Puelma's note and the publication of Tarchanoff's later observations on the frog, the authors of this paper undertook to check Tarchanoff and Puelma's statement respecting mammals, and to extend the method of experiment to frogs. In the case of mammals, dogs and rabbits were used. They were rendered insensible by chloroform or ether, or subcutaneous injections of hydrochlorate of morphia. Their vagi were exposed in the neck and divided; and the peripheral ends loosely tied by ligatures. Arrangements were made to rapidly shunt an induced, interrupted current from a Du Bois-Reymond's induction coil, from one vagus into the other, the peripheral ends of the nerves being laid over fine platinum electrodes for the purpose. In all the experiments save one a Daniell cell was used to induce the currents. A cannula was placed in the femoral artery (in one case in the carotid artery) and connected with a kymograph, which wrote upon a moving sheet of paper.

In the case of frogs, the brain, and sometimes the spinal cord, were destroyed by pithing; a stout glass rod was thrust down the gullet;

* Jean Tarchanoff et G. Puelma, "Note sur l'effet de l'excitation alternative des deux pneumogastriques sur l'arrêt du cœur." "Archives de Physiologie," serie II, tome II, 1875.

† M. de Tarchanoff, "Innervation de l'appareil modérateur du cœur chez la Grenouille." Marey's "Physiologie Expérimentale," II Année, 1876, p. 289.

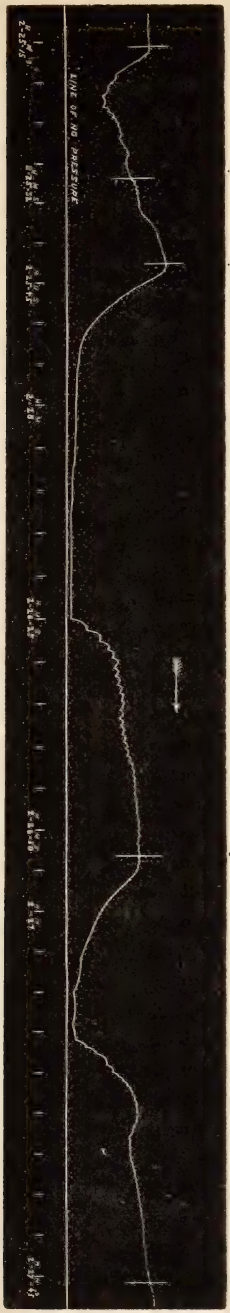
the vagi were exposed and placed each on a pair of fine platinum electrodes; and preparations were made for shunting an interrupted current, by means of a commutator, from one nerve into the other, just as in the case of mammals. Sometimes the heart was watched directly and notes made. At other times care was taken to register the heart's rate, the lapse of time and the moments of stimulation. The heart's rate was indicated by means of Marey's *Myographe du cœur*,* the time by an electromagnet and Ludwig's *Unterbrechungsuhr*, and the moments of stimulation by a marking key, each holding a pointed lever against a smoked revolving cylinder.

The general result of the experiments made by the authors of this paper, is the following: In all the animals hitherto examined (viz., dogs, rabbits and frogs), if one vagus be stimulated powerfully so as at first to arrest the heart, and if after the heart has recommenced to contract, the current be at once shunted to the other vagus, arrest again occurs: in some cases, however, on again reversing to the vagus first stimulated, no effect is produced. This result may be formulated as follows: Stimulation of one vagus never annuls, or even prejudices, the inhibiting powers of the other vagus, *unless when the inhibiting apparatus has been recently under stimulation for some time.* It would therefore seem that Tarchanoff, in asserting the mutual prejudicial action of vagus-stimulations in mammals, and denying it in frogs, has missed one half of the truth in the case of the former, and the other half in the case of the latter.

In illustration of their statement of results of experiments, the authors append a reduced kymographic tracing of a dog. The animal was a young terrier, and the experiment was made in the manner above described. The arterial cannula leading to the kymograph was introduced into the femoral artery.

The upper line is the tracing of the kymograph, the middle line is the line of no pressure, and the lower line is divided into intervals of five seconds. The tracing reads from left to right. Quite at the left of the figure a small portion of the normal tracing is represented. At 2h. 25m. 15s., a stimulus was thrown into the left vagus, the secondary coil being 6 c.m. from the primary. At once the heart stopped and the blood-pressure fell. The heart recommenced, and at 2h. 25m. 32s. the current was shut off from the nerve. It remained off about 10 seconds, and at the expiration of that interval the current was thrown into the right vagus. Again the heart stopped, and remained motionless until 2h. 26m. 25s., a period of 43 seconds, the current, of course, passing the whole of the time. The heart then began to beat, and the blood-pressure rose towards the normal rapidly. At 2h. 26m. 53s. the current was shunted out of the right vagus into the left, the nerve first stimulated, and once more the pulse ceased and

* See Marey's "Physiologie Expérimentale," II Année, 1876, p. 70.



...Excitation of left vagus, II coil at 6 c.m.

....Stop exciting.

....Reversed excitation to right vagus.

....Reversed to left vagus.

....Stop exciting.

(Tracing reduced by photography to about $\frac{1}{3}$.)

the pressure fell. At 2h. 27m. 15s. the pressure quickly rose, notwithstanding the continued stimulation; until at 2h. 27m. 45s. the current was broken and the experiment brought to a close.

It seems to the authors that the generally accepted theory of the connection of the vagus nerves with a common intermediate apparatus is fully competent to cover the facts established by A. B. Meyer, Tarchanoff and Puelma, and themselves. Each vagus abuts against a common nervous apparatus, through which every inhibitory stimulus must pass to reach the cardiac muscle. It is only necessary to suppose—and the supposition, as it will be shown, is not gratuitous—that the exhaustibility of the vagi is far greater than that of the common mechanism they lead to, in order to gain a consistent view of the facts. When one vagus is stimulated with a strong current, the heart, after a longer or shorter period of diastolic rest, gradually escapes from the inhibitory influence of the stimulus. This escape must not be supposed to be due to the gradual exhaustion of the whole inhibiting apparatus, but merely to that of the vagus which is stimulated. The heart escapes, not because its guard is enfeebled, but because the warrant for detention miscarries. On shunting the current into the other vagus, the intermediate apparatus is at once roused and the heart stopped.

So much greater are the enduring powers of the intermediate apparatus that, even in the mammal, where, presumedly, they are more easily exhausted than in the frog, the intermediate apparatus seems capable of tiring out relay after relay of reinvigorated vagi. Indeed, it would appear to be next to impossible, by the most careful and judicious alternate stimulation of the vagi, to cause exhaustion of the intermediate apparatus; for, after stimulating the vagi alternately a few times, the interval during which each vagus can hold the heart in check becomes too brief for the recovery of the resting vagus. Nevertheless, when this stage has been reached, it is always possible (in the frog) at once to stop the heart by applying an induced, interrupted current to the *sinus venosus*, even if the current so applied be weaker than that passing previously through the vagus nerves.

But, although the authors believe that the intermediate apparatus can, in health, easily wear out the organised attacks of the vagi, particularly in the case of frogs, yet they think it not improbable that, in conditions unfavourable to the restorative processes, the intermediate apparatus may occasionally succumb to successive vagus-stimulations, especially in the case of mammals. They are, however, convinced that the latter alternative very rarely occurs.

The assumption of such enormous powers of endurance for the intermediate mechanism is not gratuitous. As long ago as 1869, A. B. Meyer* pointed out that the heart of the frog could be inhibited

* A. B. Meyer, *loc. cit.*

for hours by carefully applying interrupted currents to the *sinus venosus;* and although the authors of this paper, in repeating the experiment, have sometimes failed to inhibit indefinitely—as did also Meyer—they have always found it possible to stop the heart for very long intervals by stimulating the *sinus venosus;* even with weaker currents than had just before sufficed to exhaust simultaneously the inhibiting powers of both vagi.

II. "On Schulze's Mode of Intercepting the Germinal Matter of the Air." By JOHN TYNDALL, F.R.S., Professor of Natural Philosophy in the Royal Institution. Received December 17, 1877.

In "Poggendorf's Annalen" for 1836, Franz Schulze described an experiment which has attained considerable celebrity. He placed in a flask a mixture of vegetable and animal matters and water; through the cork of the flask two glass tubes passed air-tight, each being bent at a right angle above the cork. He boiled the infusion; and while steam issued from the two glass tubes, he attached to each of them a group of Liebig's bulbs, one group being filled with solution of caustic potash, and the other with concentrated sulphuric acid. Applying his mouth on the potash side, he sucked air daily through the sulphuric acid into the flask. But, though the process was continued from the end of May till the beginning of August, no life appeared.

In this experiment, the germs diffused in the atmosphere are supposed to have been destroyed by the sulphuric acid, and doubtless this was the case. Other experimenters, however, in repeating the experiment of Schulze, have failed to obtain his results. The experiments of Dr. Hughes Bennett are a case in point, to which I might add certain failures of my own. Schulze's success is, perhaps, in part to be ascribed to the purity of the air in which he worked; possibly, also, to extreme care in drawing the air into his flask; or, it may be, that the peculiar disposition of his experiment favoured him. Within the flask, as shown by his diagram, both his glass tubes terminated immediately under the cork, so that the air, entering by the one tube, was immediately sucked into the other, thus failing to mix completely with the general air of the flask.

At a very moderate rate of transfer, I found, in 1869, that germs could pass unscathed through caustic potash and sulphuric acid in succession. To render the experiment secure, the air-bubbles must pass so slowly through the sulphuric acid, that the floating matter, up to the very core of every bubble, must come into contact with the surrounding liquid. It must, of course, touch the acid before it can be destroyed.

Reflecting on this experiment, and knowing that a sealed chamber, simply wetted within, suffices to detain the floating matter coming into contact with its interior surface, I thought that the same must hold good for the air-bubbles passing through a group of Liebig's bulbs. Every bubble, in fact, represents a closed chamber of infinitesimal size, and it seemed plain that if the walls of this chamber were formed of water instead of sulphuric acid, the floating matter would be effectually intercepted. This was a conclusion arrived at last summer, in the Alps. On my return to London I tested the conclusion, and found it verified by experiment.

Two large test-tubes, each about two-thirds filled with turnip infusion, were so connected together, that air could be drawn through them in succession. With a view to their sterilization, the tubes were subjected for a sufficient time to the action of boiling water, and permitted to remain afterwards in a room kept at a temperature of 90° Fahr., until it was evident that sterilization had been complete. Two narrow tubes, passed through the cork of each test-tube, in the same manner as in Schulze's flask, and it was so arranged that the tube which delivered the air should end near the surface of the liquid, the exit tube in each case ending immediately under the cork. Two series of Liebig's bulbs, charged with pure water, were attached to the two ends of this arrangement; one series being connected with a large receiver, placed on the plate of an air-pump, and the other left open to the air. The connection between the receiver and the adjacent series of bulbs being first cut off by a pinch-cock, the receiver was exhausted, and, by carefully loosening the pinch-cock, a very slow passage of the air through the test-tubes was secured. The rate of transfer was, however, such, that the air above the infusions was renewed twenty or thirty times in twenty-four hours. At the end of twelve days, during which this process was continued day and night, the turnip juice was perfectly pellucid and free from life. Two days' exposure to ordinary air sufficed to render the infusion muddy.

After twelve days the pinch-cock was opened, so as to allow a momentary inrush of the external air, which was immediately checked by the reclosing of the cock. Three days afterwards, the infusion of the test-tube, into which the air first entered, was muddy and crowded with life. The contamination did not reach the second test-tube, which remains to the present hour intact. Other experiments of a similar character have been executed with precisely the same result. I will not occupy the Society's time by describing them. Suffice it to say, that they completely verify the conclusion that, in Schulze's experiment, water may be substituted for sulphuric acid and caustic potash without any alteration in the result.

III. "Experimental Contribution to the Etiology of Infectious Diseases with special reference to the Doctrine of Contagium vivum." By E. Klein, M.D., F.R S. Received February 4, 1878.

The present communication has for its object to bring before the Royal Society the results of an experimental inquiry * into the etiology of an infectious disease of the pig, known as Hog Plague, Mal Rouge, Red Soldier, Malignant Erysipelas, or also Typhoid Fever of the Pig. There are English and continental writers who describe the disease as Anthrax or Splenic Fever of the Pig. I shall show, however, conclusively, in my Report to the Medical Officer of the Local Government Board, that it is neither typhoid fever nor anthrax, but is an infectious disease of its own kind, which I propose to call "Infectious Pneumo-Enteritis of the Pig" (*Pneumo-enteritis contagiosa*).

Like other infectious diseases, the "Pneumo-Enteritis" possesses an incubation period, followed by constitutional disturbance and certain anatomical changes. These latter are invariably affections of the lung, of the intestine, and of the lymphatic glands, not only of those of the organs of respiration and alimentation, but also those of the inguinal and lumbar regions. In the lung the changes are those known to pathologists as lobular pneumonia. In the alimentary canal the mucous membrane of the large intestine is chiefly affected, being the seat of smaller or larger ulcerations. There is generally also inflammation of the serous membranes, especially the peritoneum, leading to an exudation of lymph into the serous cavity. The skin is occasionally affected with greater or smaller red patches.

There are hæmorrhagic patches to be found in the lung and serous membranes, the endocardium, and the muscle of the heart, the mucous membrane of the intestine (especially duodenum and large intestine), the tongue, and occasionally also the liver and spleen, only seldom in the skin and kidney.

In anatomical respects, therefore, the Pneumo-Enteritis bears undoubtedly a great resemblance to anthrax or splenic fever. There exists, however, a marked difference between the two diseases in the incubation period, the general pathology,† and especially in the anatomical character of the spleen and blood. In splenic fever we find the spleen invariably enlarged, being the principal organ of the affection,

* This being part of a larger research carried out for the medical officer of the Local Government Board.

† In splenic fever the period of incubation ranges from between a few hours to several days, in pneumo-enteritis it varies from two to five days and more. Splenic fever is easily transmissible to man and the domestic animals, whereas the transmissibility of the pneumo-enteritis is much more limited. Hitherto I have succeeded in communicating it to rabbits, guinea-pigs and mice, although only with difficulty.

whereas in pneumo-enteritis it is only occasionally changed. And, likewise, the blood presents entirely different characters in the two diseases; in pneumo-enteritis it is not different in any marked degree from normal blood, whereas in splenic fever it is of dark colour—laky, and does not coagulate at all, or only imperfectly so. Besides, the blood in splenic fever contains the now famous Bacillus anthracis, and hence its conspicuous infectious property, whereas in pneumo-enteritis the fresh blood does not, as a rule, contain any foreign matter, and in most instances does not possess any infectious property.

Another disease with which pneumo-enteritis bears a great resemblance on account of certain anatomical characters, viz., inflammation of serous membranes, lung, intestine, and lymphatic glands, hæmorrhage in lung, serous membranes, endocardium muscle of heart, intestinal mucous membrane, and other organs—is specific septicæmia.*

The resemblance, however, is not greater than to splenic fever, although the differences are not less well marked. Besides others, there is this great distinction, that in pneumo-enteritis the contagion spreads by simple cohabitation, and through the air, which it never does in septicæmia, as in this the virus always requires a broken surface through which to enter a healthy individual.

Pneumo-enteritis is occasionally described as malignant erysipelas (mal rouge, red soldier), but this is in so far inadmissible, as the affection of the skin in the former is a very inconstant symptom, and in milder forms of the disease is invariably absent. More recently the pneumo-enteritis has been regarded as typhoid fever of the pig. From a purely anatomical point of view, the resemblance between real, *i.e.*, human, typhoid fever and pneumo-enteritis is very slight indeed, so slight, in fact, that to mention it requires a total oversight of some of the most prominent symptoms, *e.g.*, inflammation of lung and serous membranes, enlargement of inguinal, lumbar, and bronchial lymphatic glands, hæmorrhages in the endocardium and muscle of the heart in pneumo-enteritis on the one hand, swelling and ulceration of the lymphatic glands of the small intestine, swelling and inflammation of spleen in real typhoid fever on the other hand. The resemblance seems to be limited solely to the fact that in both diseases ulceration occurs in the intestine. But the distribution, the nature, and the development of these ulcerations is totally different in the two diseases.

Having said thus much as a prefatory explanation, I proceed to state the results of the experiments.†

* Specific septicæmia as distinct from septic infection. See Dr. Burdon-Sanderson's lectures at the University of London, 1877.

† In all my experiments of inoculation the materies morbi was used in minimal doses, *i.e.*, a drop of fluid matter, or in the case of solids a particle of less than the size of a pin's head. In both cases the materies morbi was diluted or suspended respectively in a few minims of boiled saline solution of $\frac{3}{4}$ per cent. in order to in-

The experiments refer to the following series:—

1. Experiments showing that the fresh blood of diseased animals does not, as a rule, contain the virus, as it fails to produce the disease when introduced into a healthy animal.

Four animals were inoculated (at different times) with fresh blood of diseased animals. They remained healthy. When subsequently inoculated with virus-containing matter, they became smitten with the disease.

In a fifth instance, however, fresh blood did produce infection. [And this same blood proved active after having been kept sealed up in a capillary tube for several weeks.] This blood was obtained from a very severe case with copious peritoneal exudation, in which were found peculiar abnormally large coarsely granular cells; the same cells were also present in the blood; so that it appears probable that the blood became charged, by absorption during life, with matter from the peritoneal exudation. This latter always contains the virus in an active state.

2. Experiments showing that fluid as well as solid lymph of the diseased peritoneum contains the virus in a very active state.

Six successful inoculations with fluid peritoneal exudation.

There is no difference of activity to be noticed between fresh exudation and one that had been kept sealed up in a capillary tube for several weeks.

Solid lymph obtained from the peritoneal cavity of diseased animals, having been dried at a temperature of about 38° C., proves very active.

3. Experiments showing that parts of the diseased lung, ulcerated intestine, and also diseased spleen, contain the virus in an active state. Diseased parts of lung or intestine, that were dried at a temperature of about 38° C., retain their virulence unaltered.

In all cases of pneumo-enteritis the trachea as well as the bronchi contain frothy blood-containing mucous matter, possessed of infectious property. It must be, therefore, supposed that the breath of a diseased animal is charged with the poison. On account of the diseased state of the intestine also, the dung is to be regarded as infectious.

4. Experiments showing that infection is produced by cohabitation

crease its bulk and thus to facilitate its introduction. The inoculation was invariably carried out by injection into the subcutaneous tissue by means of a fine canula of a hypodermic syringe, necessary care being taken that this had been previously thoroughly cleaned and disinfected. After and before inoculation the animals have always been kept isolated and in clean and disinfected places. In order to insure reliable results (viz., that the disease in a particular case was really a consequence of the inoculation and not of infection through other sources) care was taken that those who attended the isolated animals were not the carriers of infection.

with a diseased animal, or by keeping healthy animals in a place whence a diseased animal had been removed.

5. Several experiments were made to see whether feeding healthy animals on matter obtained from the diseased organs (intestinal ulcers especially) produces the disease. The experiment was always attended with success, if an abrasion existed in the mucous membrane of the mouth or pharynx; this was usually the case when the matter had to be introduced into the mouth while the animal was being held by assistants.

There were, however, two cases which appear to prove that the disease cannot be produced by simple feeding.

This was, unfortunately, at a time when I was not acquainted, yet, with the fact that in many animals the disease is of so mild a form that it can hardly be recognized in the living. I did not make any post-mortem examination of those two animals.

But since then I have made two other experiments, in which the virus was brought directly into the stomach, by means of an india-rubber tube introduced per fauces et œsophagum. In both these instances the animals became diseased and their intestines were most conspicuously affected.

From the last three series of experiments we may conclude that the principal mode by which contagion of pneumo-enteritis is carried out, is through the instrumentality of the air and the food.

6. This series comprises experiments to prove that the virus can be cultivated artificially, *i.e.*, outside the body of an animal; in the case of splenic fever it has been successfully done by Dr. Koch.

The experiments are seven in number, (*a*), two refer to cultivations started with fluid peritoneal exudation; (*b*), in the five others the virus had been obtained by cultivation of dried lymph of the peritoneum of an animal suffering from the disease.

(*a*.) The cultivation of the virus for the first two cases was carried out thus:

Fluid peritoneal exudation of a diseased animal had been collected and sealed up on November 6, in a capillary glass tube. On the following day there was present a small clot due to coagulation. A minute speck of this clot was removed with the point of a clean needle, and with it was inoculated a drop of fresh aqueous humour of a healthy rabbit. This drop had been placed on a thin covering-glass, which, after the inoculation, is inverted over a small "cell," made by fixing a glass-ring* on an ordinary glass-slide. The covering-glass is fastened on the glass-ring by means of a thin layer of pure olive-oil.

* The glass ring I used is 0·5 to 2 millimètres high, about 2 mm. thick, and about 18 mm. wide. If the preparation is to be observed on the hot-stage of the microscope, instead of the ordinary glass-slide, one of only 0·5 mm. thickness is chosen in order to bring the preparation more rapidly up to the desired temperature.

The preparation was then kept in the incubator for twenty-four hours at a temperature of 32–33° C. After this time it was used to inoculate a new drop of humor aqueous in a similar manner as the one just described. We will call this the second generation.

This new specimen was placed in the incubator and kept there at a temperature of 32–33° C., for further twenty-four hours. In the same manner a third generation was started by inoculating a fresh drop of humor aqueous. After having been kept in the incubator for several days it was used to inoculate two animals at different times. Both animals became smitten with the disease.

(*b.*) The other five experiments were carried out with virus cultivated from solid lymph of the peritoneum of a diseased animal. The lymph had been dried at 38° C (See Series 2). A small particle of dried lymph is crushed into fine powder. With a granule of this a drop of fresh humor aqueous is inoculated in the same manner as above described.—First generation.

After having been kept in the incubator for two or three days at a temperature of 32–33° C., it is used to inoculate a second generation, care being taken to use a trace only of the fluid part and not to come in direct contact with the original granule, which may be still discerned in the preparation.

The specimen representing the second generation is kept in the incubator for a day or two. It is then used to inoculate a fresh preparation.—Third generation. And, finally, this is used for establishing a fourth generation. After having been kept in the incubator, a part of it is used for inoculating *two* animals, the inoculation being carried out at different times. Both these animals become smitten with the disease.

Another portion of this fourth generation was used to start a fifth generation, then a sixth, a seventh, and an eighth generation. With this three animals were inoculated at different times. All three animals became diseased in due time.

In order to correctly interpret the results of this last (6th) series of experiments, it is important to mention that inoculation with dried lymph, diluted far less than would correspond to the third generation in the last-named experiments, is followed by a negative result.

The microscopic examination of the cultivated liquids proves that these are the seat of the growth and development of a kind of bacterium, which has all the characters of bacillus subtilis (Cohn). The bacillus in our case is a very fine and delicate rod, thinner than both that described by Professor Cohn in hay-infusion, and the bacillus anthracis so thoroughly investigated by Dr. Koch.

Our bacillus differs also in other respects from that of bacillus anthracis, inasmuch as it possesses a moving stage, the bacillus anthracis described by Dr. Koch is non-moving. Like bacillus subtilis of hay

and bacillus anthracis, our bacillus grows under favourable conditions into long leptothrix-like filaments, which occasionally form more or less complex convolutions.

In these filaments highly refractive spores make their appearance. These become free after the disintegration of the original filamentous matrix. The fully developed spores of our bacillus differ from those of hay-bacillus and anthrax-bacillus by being more distinctly cylindrical and much smaller.* According to Professor Cohn (Beiträge zur Biologie der Pflanzen II, 2, 1876, p. 264) the long diameter of the spores of bacillus of hay and also of anthrax—for both are identical in morphological respects (l.c., p. 275)—amounts to 0·0015—0·0022 mm. or $\frac{15 \text{ to } 22}{25000}$ of an inch, whereas the spores of our bacillus are little less than 0·0005 mm. or $\frac{1}{50000}$ of an inch in their long diameter.† At first I misinterpreted the spores, regarding them as a kind of micrococci, and only after repeated observations have I succeeded in tracing them through their different stages of development.

After many failures—owing to the introduction and development of bacterium termo—I succeeded at last in obtaining, already in the second generation of original virus, a pure crop of bacillus and its spores. With these I started several separate cultivations, in which the germination of the spores into delicate bacillus, the swarming stage, the rapid multiplication by division, their growth into long apparently smooth filaments, and, under sufficient access of air, the formation of the bright cylindrical spores in them could be distinctly traced.†

* In the figures accompanying Dr. Koch's paper on bacillus anthracis (Beitr. z. Biologie d. Pflanzen ii, 2, 1876) the spores are represented in many places as more or less spherical in shape.

† In convolutions of filaments the outlines of these latter become gradually lost after the spores are formed. The spores appear now to be embedded in a transparent gelatinous matrix. At the edges of such masses or where they are in a sufficiently thin layer, the linear arrangement of the spores can be still recognised. But there is undoubtedly a transparent jelly present in these masses forming the ground substance for the spores and fibres. Professor Cohn mentions (l. c., p. 263) a similar jelly in convolutions of hay-bacillus. I entirely differ from Dr. Koch with regard to the mode of germination of the spores of bacillus. Koch states (l. c., p. 289, and also in his latest paper on Bacteriæ in Biol. d. Pfl. 2 Bd. 3 Heft.) that it is not the highly refractive spore which directly produces the bacillus, but that the hyaline gelatinous envelope surrounding each spore elongates so as to form the bacillus, while the bright spore-matter itself gradually diminishes in size and finally disappears. From *à priori* reasons it is impossible to assume that this can be so, viz., that the gelatinous envelope should grow into the bacillus ; for Cohn proved beyond doubt that in the case of hay-bacillus the spores germinate even after having been exposed to boiling heat. Surely this gelatinous envelope, if living protoplasm, must become, under these conditions, deprived of its germinating power. Direct observation proves that in my case the spores possess another membrane within that gelatinous envelope, and during germination this inner membrane is broken at one pole and the contents of the spore protrude and grow out as the bacillus. This

(No other organisms appeared in these cultivations.) These were again used to inoculate other preparations of aqueous humour, and so on, until I succeeded in obtaining considerable quantities of liquid, containing only bacillus and its spores. The last-named animals were infected with liquid of this kind.

Seeing that splenic fever, pneumo-enteritis, and specific septicæmia possess a great affinity in anatomical respects, and seeing that in splenic fever and pneumo-enteritis the materies morbi is a definite species of bacillus—the difference of species being sufficiently great to account for the differences in the two diseases—we may with some probability expect that also the third of the group, viz., specific septicæmia, is due to a bacillus. This, however, remains to be demonstrated. It seems, finally, justifiable to speculate whether or not we have in these three varieties of disease "a variation of species" in the sense of the evolution theory.

IV. "On the Use of the Reflection Grating in Eclipse Photography." By J. Norman Lockyer, F.R.S. Received February 8, 1878.

The results obtained by the Eclipse Expedition to Siam have lead me to think that, possibly, the method of using the Coronal atmosphere as a circular slit, suggested by Professor Young and myself for the Indian Eclipse of 1871, might be applied under very favourable conditions, if the prism, or train of prisms, hitherto employed, were replaced by one of those reflection gratings with which the generosity of Mr. Rutherfurd has endowed so many of our observers.

To test this notion I have made some experiments with a grating, which I owe to Mr. Rutherfurd's kindness, containing 17,280 lines to the inch. The results of these observations I have now the honour of laying before the Royal Society.

In front of the lens of an ordinary electric lamp, which lens was adjusted to throw a parallel beam, I introduced a circular aperture, cut in cardboard, forming an almost complete ring, some 2 inches in interior diameter, the breadth of the ring being about $\frac{1}{8}$ inch. This was my artificial eclipse.

At a distance from the lamp of about 13 yards, I mounted a $3\frac{3}{4}$ inch Cooke telescope, of 54 inches focal length. Some distance short of this focus I placed Mr. Rutherfurd's grating, and, where the first order spectrum fell, I placed a focussing screen. To adjust for sharp focus, in the first instance, the grating was so inclined to the axis of

is also in accordance with the observations of Professor Cohn, for this authority states (l. c., p. 265) "Die Sporen schwollen etwas an und trieben an einem Ende einen kurzen Keimschlauch."

the telescope that the image of the ring reflected by the silver surface adjacent to the grating was thrown on to the screen. This done, the grating was placed at right angles to the axis, and the spectrum of the circular slit, illuminated by sodium vapour and carbon vapour, photographed for the first, second, and third orders on one side. The third order spectrum, submitted to the Society with this communication, showing the exquisite rings due to the carbon vapour flutings, was produced in 42 seconds. The first order spectrum, also submitted to the Society, was produced in the same period of time, and was very much over-exposed. It is, therefore, I think not expecting too much that we should be able to take a photograph of the eclipse, in the third order, in two minutes; but let us make it four. Similarly, we may hope for a photograph of the second order in two minutes, and it is, I think, highly probable also that a photograph of the first order may be obtained in one minute.

It is clear then that, by mounting photograhic plates on both sides of the axis, one solidly mounted equatorial of short focal length may enable us to obtain a large number of photographs, with varying lengths of exposure, of the next eclipse. I have insisted upon the solidity of the mounting because, if any one plate is to be exposed during the whole of totality, the instrument must not be violently disturbed or shaken while the eclipse is going on. I think, however, it is quite possible to obtain several photographs, of the lower order spectra, without any such disturbance. The same plate may be made to record three, or even four, exposures in the case of the first order in an eclipse of four minutes' duration, by merely raising or lowering it after a given time, by means of a rapid screw or other equivalent contrivance, so that a fresh portion of the same plate may be exposed. Similarly, the plates on which the spectra of the second order are to be recorded may be made to perform double duty.

If one equatorial thus mounted were to be devoted to each quadrant of the coronal atmosphere, it is certain, I think, that most important results would be obtained.

February 21, 1878.

Sir JOSEPH HOOKER, K.C.S.I., 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 Alteration of the Thermal Conductivity of Iron and Steel caused by Magnetism." By HERBERT TOMLINSON, B.A., Demonstrator of Natural Philosophy, King's College, London. Communicated by Professor STOKES, Sec. Roy. Soc. Received December 17, 1877.

The writer has, for several months, been engaged in experimenting on the alteration of the electrical and thermal conductivity of iron and steel, produced by magnetism, with the intention of determining such alteration in absolute measure. But as, before the complete determination of the data sufficient for the purpose, a considerable time must elapse, he would venture to offer to the Society, in a preliminary note, some results which have already been obtained by him.

In these experiments, the apparatus chiefly employed to impart magnetism consisted of an electro-magnet, with soft iron cores, 1 inch in diameter and 5 inches in length, surrounded by cotton-covered copper wire, $\frac{1}{16}$th of an inch in diameter, having a resistance of nearly 1 ohm, and a coil constructed as follows:—a thin tube of polished tin, $1\frac{1}{4}$ inch internal diameter and $4\frac{1}{2}$ inches long, was covered with vulcanised caoutchouc to a depth of $\frac{1}{16}$th of an inch, and on this was wound 3 lbs. of cotton-covered copper wire, $\frac{1}{20}$th of an inch diameter, followed by 3 lbs. of wire, $\frac{1}{10}$th of an inch in diameter. Inside the first tube was placed a second, 1 inch internal diameter, connected by ebonite with the first at the two ends, the second tube being concentric with the first, and of the same length, so that, between the inner tube and the outer, should be interposed a layer of air nearly $\frac{1}{8}$th of an inch in thickness. This arrangement was employed to prevent the heat from the magnetising coil reaching the bars placed inside; also, to prevent ingress or egress of heat; the bars used with the coil were covered with vulcanised caoutchouc, so that they lay with their axes coinciding with the axis of the coil.

To measure the heat conducted along the bars two sets of thermo-elements were constructed. The first was made up of two small strips of copper, $\frac{1}{2}$ inch long and $\frac{1}{4}$ inch broad, of small thickness. To these was soldered about 1 foot of silk-covered german-silver wire $\frac{1}{20}$th of an inch in diameter, the copper strips being also connected

with a galvanometer by copper wires, well covered with caoutchouc, one end of each being soldered to the copper strips, so that either one or both the copper strips with the soldered German-silver wires could be employed as a thermo-element. In case it was found convenient to use only one of these, the other was kept well covered with sawdust. These elements will be called the G. S. elements. The second consisted of two small thermo-piles, each of 12 elements, of antimony and bismuth, and were each fitted into india-rubber tubes, so that they could be inserted into small wooden boxes, containing water, so as to fit water-tight. Sometimes these, like the G. S. elements, were used separately; at others, were made to neutralise each others effect on the galvanometer.

With these elements, a delicate Thomson's reflecting galvanometer, having a resistance of 2 ohms, was employed, the scale being placed about 6 feet from the mirror of the needle.

In the circuit of the magnetising current, a tangent galvanometer was placed, the needle of which was suspended by a very fine platinum wire, attached to a graduated torsion circle. The two thick copper wires which conveyed the current were each 27 centimetres in diameter and 14 inches apart.

A magnetometer was also employed to test the magnetism imparted, but, as the readings will not be given in this paper, no description of the instrument is necessary.

To heat one end of the bars two large Leslie's cubes were employed, having each two apertures projecting about 2 inches into the interior of the cubes. The apertures in one cube were $\frac{1}{2}$ inch square and placed in one side of the cube, about 3 inches apart from each other, and half way up the side. In the other cube the apertures were circular, and $\frac{1}{2}$ inch in diameter, and placed in the centres of opposite sides of the cube. Into these cubes bars of square and circular section were respectively inserted, and water filling the cubes was raised to 100° C.

The first experiment was made on a soft iron bar, $\frac{1}{2}$ inch square in section. The bar was inserted into the Leslie's cube with square aperture, filled with boiling water, and into the other aperture was inserted a brass bar of the same section. The length of the iron bar was about 10 inches, and of the brass one, 18 inches. One of the G. S. elements was fastened on to the iron bar, and the other to the brass, by caoutchouc bands, the elements being insulated from the bars by thin paper. The G. S. elements were made to very nearly counteract each others effect by sliding one or other of them up and down the bars until the heat conducted by the bars to the elements was in both the same as nearly as possible. Underneath the bar was placed the electro-magnet, with a piece of white cardboard lying on the upper extremities of the soft iron cores, so that the cores were about 3 milli-

metres from the iron bar; the bar was also propped by a wooden support at the other end. A preliminary experiment had been made for the purpose of ascertaining whether the heat radiated from the electro-magnet would affect the thermo elements to any appreciable extent, and this was not found to be the case.

The water in the cube was now kept boiling for about nine hours, and after that time had elapsed it was ascertained that the flow of heat from one part of the bars to another compensated the loss from radiation, and the light on the scale remained stationary. (It should be observed that, as it was impossible to cause one thermo-element exactly to neutralise the other, a slight use of the adjusting magnet of the galvanometer was made to bring the light near the zero of the scale.) The current from a single bichromate cell was now sent through the coil of the electro-magnet. In a few moments the spot of light began to move very slowly across the scale in a direction indicating that the flow of heat through the iron bar was being checked, and in about fifteen minutes a deflection of about 25 divisions of the scale was obtained; the current of the cell was then stopped, when the light began slowly to return, and finally settled in apparently the exact position which it held before sending the current through the coil. This experiment was repeated several times on the same day, and on several other days, but always with the same result as regards direction, namely, showing a diminution of the flow of heat from longitudinal magnetisation.

As it was thought, however, that the electro-magnet might when in action perhaps produce an apparent diminution of flow of heat by slightly disturbing the bar in its position in the aperture, though every precaution had been used to prevent the chance of this, the bar in the next set of experiments was securely soldered into the cube, and again tested as before. Here, again, some five or six trials gave the same result, and seemed to show, most conclusively, that the thermal conductivity of soft iron is diminished by longitudinal magnetisation.

As the method of observation, however, was tedious and would give no idea of the amount of alteration of conductivity, other expedients were now adopted.

A piece of soft iron, BC, $\frac{1}{2}$ inch long, $\frac{1}{2}$ inch broad, and 2 millimetres in thickness, was soldered, as in the figure, to two pieces of brass, AB, CD, of the same breadth and thickness.

AB was 2 inches long and CD 1 inch.

The piece AB was also soldered to another piece of brass AE, having a section $\frac{1}{2}$ inch square, and length about 12 inches.

The whole was placed inside a wooden box, 2 feet long, 1 foot wide, and 2 feet high, lined on the outside with tinfoil.

AE passed through a circular hole, 2 inches in diameter, in the middle of one end of the box, and through two corresponding circular holes in a double screen of sheet tin, and thence into one of the square apertures of one of the Leslie's cubes. The electro-magnet was placed underneath BC, and two pieces of soft iron, about $3\frac{1}{2}$ inches in length, $\frac{1}{2}$ inch in breadth, and about 2 millimetres in thickness, were placed on the ends of the soft iron cores, so as to be on the same level with BC, and distant from B and C about 2 millimetres, the pieces of iron being separated from the cores by a piece of white paper. In some experiments the pieces of iron thus used were carefully secured to the electro-magnet by elastic straps, and, in others, by weights placed on them. A preliminary experiment, made with the strongest current that was ultimately used with the electro-magnet, showed that neither were the iron pieces on the cores appreciably shifted on magnetising the coil, nor was the heat radiated from the coil sufficient to produce any effect on one of the G. S. elements fastened by elastic bands to the back of CD. The other G. S. element was in this case buried in sawdust, with which the box was now filled, so as to cover completely the bars and electro magnet. The sawdust also filled the space between AB and CD, and the small portion of the brass bar AE, which was between the box and the Leslie cube, was covered with caoutchouc and cotton wool.

The lid of the box was now put on, and some time allowed to elapse until the light remained steady on the scale.

Boiling water was then put into the cube, and a burner placed underneath. Very shortly the light began to move across the scale, showing that the heat had been conducted along the compound bar to the thermo element, and then the number of divisions on the scale passed over by the light was taken for each minute. The adjusting magnet of the galvanometer had in these and similar experiments to be placed very low down in order that the number of divisions of the scale passed over might not exceed 60 per minute. When the light reached one end of the scale the adjusting magnet was used to bring it back again to the other. In these and similar experiments it was found that, after some time, either the number of divisions passed over per minute increased or diminished very slowly or else remained constant for some minutes. The following observations were made with the specimen in question, the light being near one end of the scale. The number of divisions passed over in consecutive minutes was as follows (the magnetising current will be called M. C.):—

A M.C. not flowing. 1st minute 40	B M.C. flowing. 1st minute 34	C M.C. not flowing. 1st minute 31	D M.C. flowing. 1st minute 28
2nd minute 39	2nd minute 34	2nd minute 33	2nd minute 29
3rd „ 38	3rd „ 33	3rd „ 31	3rd „ 27
4th „ 34	4th „ 31	4th „ 31	4th „ 29
5th „ 36	5th „ 32	5th „ 30	5th „ 28
No. of divisions in last 4 minutes =147.	No. of divisions in last 4 minutes =130.	No. of divisions in last 4 minutes =125.	No. of divisions in last 4 minutes =113.

The light was then near the other end of the scale, so the adjusting magnet was employed to bring the light back again. The set of experiments here given, the first of many, is rather an unfavourable one, most of them giving results much closer together than those here given, and, in several instances, the flow of heat would seem perfectly steady for upwards of fifteen minutes.

The observations of the first minute in each case were not taken, in order to avoid error from very slight deviations of the galvanometer needles caused by the action of the electro-magnet, which however was placed at such a distance from the galvanometer, and in such a position as not, in most cases, to produce any such deflection, and to give time for the magnetism to produce its effect on the bar. It has been determined, that in experiments made in this manner, 1 minute seems quite sufficient for the above-mentioned purpose.

Taking A and C together, we obtain as a mean the number of divisions passed over in 4 minutes with the M. C. not flowing 136 as against 130 from B, with the current flowing; again, from B and D, with the current flowing, we obtained 121·5 as against 125, with the current not flowing, so that, in both cases, there is a less mean flow with the M. C. flowing than when it is not flowing. The mean of this particular set of experiments would give a decrease of flow, when the bar is magnetised longitudinally, of about 3·6 per cent. of the whole for a magnetising current, causing a deflection of the needle of the galvanometer of 18·6°. The mean of all the observations for the specimen, and for this current, gave a decrease of flow amounting to 3·3 per cent. of the whole.

The electro-magnet was now turned through 90°, so as to magnetise the iron transversely, and a similar set of experiments were made.

Here, again, the result was most conclusive, and the mean of several observations showed an *increase* of flow when the bar was magnetised transversely, amounting to about 3·2 per cent. of the whole for the same current strength.

Thus, the decrease of conductivity in one case seems roughly to be

equal to the increase in the other, but it is the intention of the author to make further researches into this part of the inquiry.

The batteries employed in these and subsequent experiments were slight modifications of Daniell's batteries, which, though having a very small resistance, maintained a constant current for some hours. These batteries the writer hopes to have the honour of describing to the Society on a future occasion.

Similar experiments were next made on a piece of hard steel, of similar dimensions to those of the iron, but the length of the brass bar, AE, was considerably shortened.

The result of the experiments proved that there was a decrease of flow, amounting to about 4 per cent., of the whole, when the bar was magnetised longitudinally with a current producing a deflection of 18° on the tangent galvanometer, and an increase of flow when magnetised by a current of 10° C transversely, amounting to about 3 per cent. of the whole (unfortunately, through accident at the time, the same magnetising current as used for longitudinal magnetisation could not be employed).

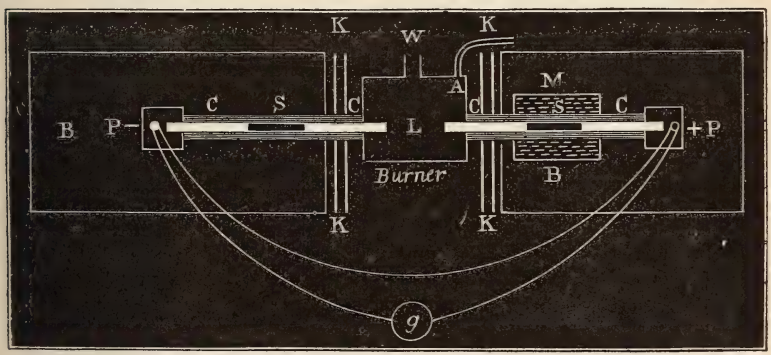
This last result was rather unexpected, as, though Sir William Thomson has shown (Phil. Trans., Feb. 28, 1856) that the electric conductivity of hard steel is diminished when the steel is magnetised longitudinally, some experiments of the writer in the same direction (Proc. of Royal Society, June 17, 1875) seemed to show that, in the case of hard steel, the contrary effect is produced, while Joule has proved (Phil. Mag., 1847) that, while a bar of iron, or even steel which is so hard that a file will just touch it, is lengthened by longitudinal magnetisation, iron under great strain, or steel so hard that a file will not touch it, is shortened.

Of course, the mere act of soldering the steel to the brass would, to a certain extent, soften the steel. A bar of steel, therefore, of circular section, $\frac{1}{2}$ inch in diameter and about 12 inches in length, was made so hard that it could not be touched with a file, and placed, with the usual precautions, on the poles of the electro-magnet, at a height of about 3 millimetres above the cores of the electro-magnet, one of the G. S. elements being secured to one end and the other end inserted in a Leslie's cube. But here, again, there was undoubtedly a decrease of conductivity, when the bar was magnetised longitudinally. Of course, however, the mere fact of heating the bar at one end to the temperature of boiling water would tend to soften the bar, and the writer is not quite satisfied but that it may be ascertained that, for low temperatures, the conductivity of hard steel is increased. It is intended, therefore, to make experiments on iron and steel, at different temperatures, both high and low, with a view of definitely settling this point.

Some experiments were next made with the magnetising coil de-

scribed in the beginning of the paper, but the effects produced were found to be so much less for the hard steel than when the electro-magnet was employed that it was found necessary to use the thermo piles, also mentioned above, and instead of employing the compensating magnet to bring the light on to the scale to cause the two piles, as much as possible, to neutralise each others effects. Accordingly the following arrangements were made:—

Two bars of hard steel, S, each $3\frac{1}{2}$ inches long and $\frac{1}{2}$ inch in diameter, were soldered at each end to two copper rods, each about $4\frac{1}{2}$ inches in length and $\frac{1}{2}$ inch in diameter. One copper terminal of each rod was inserted, as in the figure, into a Leslie's cube, L, to a distance of 2 inches, and the other terminal into a small wooden box, varnished inside with shell-lac, and capable of containing about 120 cubic centimetres of water. The bars were well covered with caoutchouc; KK are double screens, PP the two thermo elements, so arranged as to send their currents through the galvanometer in opposite directions, the wires connecting them with each other and the galvanometer being well covered with gutta-percha, and passing through small holes in the sides of the boxes. The two compound bars were thus made as exactly similar as possible, and also similarly placed, the only difference being that produced by the magnetising coil, M, whose axis coincided with that of the steel bar placed inside it.



The large boxes, B, were well filled with sawdust, and the lids being put on, the whole affair was left for some time, until the light remained steady on the scale. Boiling water was then poured into the hole, W, and a burner lighted underneath the cube, whilst the aperture at W, having been closed with a cork, the steam generated was allowed to pass through a smaller aperture at A, connected by tubing with a large vessel filled with water, for the purpose of condensing the steam.

Both the small boxes had originally the same quantity of water placed in them, and a previous experiment had shown that so exactly similar was the heat conducted along the bars to the water in the

boxes, that when some two hours or so had elapsed, the rise of temperature of the water in each box seemed as measured by an ordinary thermometer, marked off in degrees centigrade, to be exactly the same, namely, 10° C.

But as the same experiment had shown that the pile in the box on the left of the figure was very slightly more powerful than the other, the water in the box on the right was diminished very carefully by means of a small siphon, formed of india-rubber tubing, of very small bore, which was kept closed by a pinch-cock at one end, the tube passing through holes cut in the large box, through the sawdust, and through a small hole cut in the lid of the small box, so as to dip into the water. The little siphon was always kept charged, and, by means of the pinch-cock, any desired small quantity of water could be extracted from the box.

By this means it was found easy to make one pile so neutralise the other that a very slight use (if any is necessary) of the adjusting magnet was required, even when the magnet was near its most sensitive position with respect to the galvanometer needle.

On sending a current through the coil M, it was in a minute or so seen that the conductivity of the hard steel was diminished by the longitudinal magnetisation, and the amount of diminution was roughly determined by finding the amount of water necessary to be extracted from the box to again bring the light to the same slow rate of motion (about 2 divisions per minute) which it had before passing the magnetising current, and comparing this with the original amount of water in the box. The decrease of flow did not amount to 1 per cent. of the whole, even with the strongest battery used (current shown by the tangent galvanometer, 26°).

This plan was found to be highly successful for determining whether there was a diminution or not, but not equally so for determining the amount of decrease, owing to the great difficulty in this method of quickly making the light move at the same rate after magnetisation as before. Another method was therefore tried, which promises to give better results.

The figure will perhaps best explain the plan now adopted for making one pile completely neutralise the effect of the other.

P_1, P_2 are the two piles.

g the galvanometer.

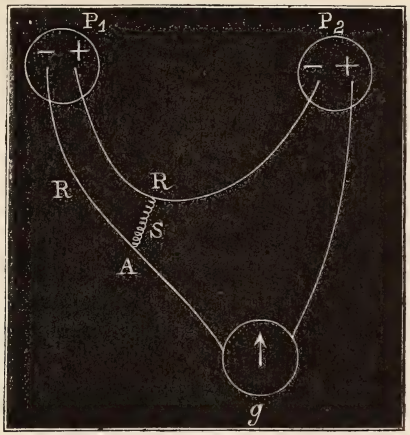
S the resistance of a shunt placed between the points AB, which could be varied at pleasure.

R the resistance of circuit AP_1B, including the resistance of the pile.

The same quantity of water was now put into each box, and the pile P_1 being slightly more powerful than P_2, the shunt S was adjusted until there was little or no motion of the light on the scale.

The magnetising current was then passed through the coil, and the shunt again adjusted, until the piles neutralised each other.

It can be readily shown that if x_1, x_2 be the units of heat imparted to the water in the two boxes in the same time, $\frac{x_2}{x_1} = \frac{S}{R+S}$ when the piles neutralise each other.



If, then, dx_2 be the diminution of flow caused by magnetisation, and dS the diminution of shunt resistance necessary to again make one pile neutralise the other,

$$\frac{x_2 - dx_2}{x_1} = \frac{S - dS}{R + S - dS}$$

$$\therefore \frac{dx_2}{x_2} = \frac{R \times dS}{S(R + S - dS)}$$

and thus the percentage of diminution of flow may be determined.

A few resistance coils were very roughly constructed for use in the shunt, S, and an attempt made to ascertain the actual amount of diminution of conductivity of the hard steel. This, again, appeared to be comparatively small, but as the coils used were not suitable for the purpose, the result is not given. But the writer has little reason to doubt that, with resistance coils suitable for such thermo-electric experiments, he will be able to measure, with fair accuracy, small variations of thermal conductivity in this way.

It should be added that similar experiments to these were made with soft iron bars, both with and without copper terminals, but the results of the experiments, of which altogether upwards of a hundred have now been made, show that in every case the thermal conductivity of soft iron is diminished by temporary longitudinal magnetisation and increased by transverse magnetisation, whilst in the case of steel, of

different degrees of hardness, at any rate, for the temperatures employed, magnetisation evidently produces the same kind of effect as with the soft iron. The amount of the alteration in the case of soft iron must have reached at least as high as 10 per cent., and may be greater. The experiments which the writer has made on electrical conductivity have shown that this is also very appreciably altered by magnetism, at least 6 per cent., even when the magnetisation was evidently not complete (Proc. of R. S., June 17, 1875), and has some reasons for believing that the amount of alteration for thermal and electrical conductivity will be found to be not very different. He hopes, however, to thoroughly investigate the whole subject.

It should be mentioned here that Sir William Thomson (Phil. Trans., Feb. 28, 1856,) expresses a strong opinion that the experiments of Dr. Maggi on this subject, on which he says doubts have been thrown by others, would be found correct, basing his opinion on the results of his own experiments on the alteration of electrical conductivity by magnetism. And, though the writer had some years ago made some attempts in the present direction (unfortunately before perusing Sir William Thomson's exceedingly valuable paper), he cannot conclude without expressing how greatly the suggestions there thrown out have assisted him in these and other experiments.

II. "Chemical Notes." By Dr. Maxwell Simpson, F.R.S., Professor of Chemistry in Queen's College, Cork. Received January 2, 1878.

On the Direct Formation of the Chloro-Bromides of the Olefines and other non-saturated Compounds.

Chloro-bromide of Ethylene (C_2H_4ClBr).

The first step towards the direct formation of this and other chloro-bromides is, of course, the preparation of the solution of chloride of bromine. This I endeavoured to prepare by a process analogous to that by which I had formed the chloride of iodine.* I found, however, that in passing the chlorine into the bromine and water, a larger quantity of the former is carried off in the form of vapour. I continued the passage of the gas, nevertheless, till all the bromine was dissolved and the solution assumed a faint yellow tint. On conducting olefiant gas into this, I obtained a very small quantity of an oily liquid, part of which boiled between 106° and 110° Cent. Altogether, it is by

* "Annalen der Chemie," cxxxvi, 141.

no means a productive progress. I made an ineffectual effort to improve it by surrounding the vessel containing the bromine with ice. This, however, only made matters worse, as it caused the formation of solid hydrate of chlorine and of bromine, which prevented the completion of the process. After several unsuccessful experiments I at last hit upon the following process, which answers remarkably well:—500 grains of bromine are dissolved in 4 fluid ounces of a mixture of equal volumes of strong hypochloric acid and water. This solution is introduced into a flask with a long neck, and surrounded with ice. Washed chlorine is then passed into it with repeated agitation, till it ceases to be absorbed. In this way not a trace of bromine is lost, nor are any solid hydrates formed during the passage of the gas. On conducting olefiant gas into this solution, which should be surrounded with cold water and repeatedly agitated, I obtained a large quantity of an oily liquid, which I separated from the acid solution, washed with dilute potash, and distilled. Almost the entire quantity passed over between 106° and 110°, most between 108° and 110°.

The following analysis proves that this was the body in question in a state of purity:—

C_2H_4ClBr.

	Theory.	Experiment.
Carbon	16·72	16·57
Hydrogen	2·78	3·01

This process yields nearly 80 per cent. of the theoretical quantity.

Chloro-bromide of propylene (C_3H_6ClBr).

This compound is prepared in exactly the same manner as the ethylene compound. The propylene gas was derived from iodide of allyl. The process is very productive. I obtained a large quantity of an oily liquid, boiling between 118° and 120°, which proved on analysis to be pure chloro-bromide of propylene.

Chlor-dibrom Allyl ($C_3H_5Br_2Cl$).

This compound may be readily prepared by the action of the solution of chloride of bromine upon bromide of allyl. It is only necessary to leave these bodies in contact for two or three days at the temperature of the air, and to agitate them repeatedly. The product distils between 197° and 199°. It gave on analysis numbers agreeing perfectly with the above formula.

Brom-dichlor Allyl ($C_3H_5Cl_2Br$).

This body may also be formed by the action of chloride of bromine upon bromide of allyl. To secure the production of this, and not the other allyl compound, it is necessary whilst agitating the reacting bodies, to maintain them at a temperature somewhat below 100° Cent.

On submitting the product to distillation, I found that the greater part passed over between 180° to 187° Cent.

This proved on analysis to be pure brom-dichlor allyl.

On the action of Chloride of Acetyle upon Valer-aldehyde.

Many years ago I ascertained that acet-aldehyde enters into direct combination with chloride of acetyle, the compound $C_4H_7O_2Cl$ being formed.* I now find that valer-aldehyde also combines directly with this body. Their union is determined in the following manner :— One molecule of valer-aldehyde is mixed with one of chloride of acetyle (the evolution of heat was observed when they came into contact), and the mixture is subjected in a sealed tube to the temperature of 100° Cent. for about three hours. At the expiration of this time the tube is opened, and its contents subjected to distillation. It commenced to distil at 115° Cent., and between that temperature and 145° a little more than half of the fluid passed over. The remainder refused to distil, although heated to 300°. (A considerable quantity of hydrochloric acid gas was evolved during the distillation, and also when the sealed tube was opened.) On re-distilling I found that the greater part of the distillate passed over between 118 and 128° Cent. This gave on analysis the following numbers, which agree pretty well with the formula $C_5H_{10}O.C_2H_3OCl$:—

	Theory.	Experiment.
Carbon	51·06	50·30
Hydrogen	7·90	8·15

This compound is a little lighter than water, its specific gravity being 0·987 at 17° Cent. It is gradually decomposed when brought into contact with water, hydrochloric acid, acetic acid, and valer-aldehyde being formed.

The compound, which would not distil, was neutral to test-paper. I have not yet succeeded in rendering it sufficiently pure for analysis.

I think we may now safely conclude that all the aldehydes of the fatty series combine directly with chloride of acetyle, and that this reaction is characteristic of this class of compounds.

On a new Double Salt of Iodide of Calcium and Iodide of Silver.

This salt is prepared by heating a concentrated solution of iodide of calcium and adding to it moist iodide of silver till it ceases to be dissolved. The filtered solution crystallizes out on cooling in long white brilliant needles. These were pressed between folds of filtering paper, dried at 100° Cent., and analysed. The following are the results of two analyses :—

$$CaI_2 2(AgI) 6(H_2O).$$

* *Comptes Rendus* séance du 29 Novembre, 1858.

	Theory.	Experiment.	
		I.	II.
AgI	53·89	52·63	52·83
CaI_2	33·72	34·14	34·00
H_2O	12·40	13·23	13·17

This salt is completely dissociated by water, a few drops even being sufficient to separate the whole of the iodide of silver. This reaction enabled me to analyse the salt with facility.

It was only necessary to add water to a known weight of the salt and filter. The iodide of silver may then be determined on the filter, and the calcium in the filtrate in the usual way by means of oxalate of ammonia. The water I had to determine by difference, as the heat necessary to expel the water drove off at the same time some iodine from the iodide of calcium. A known weight of the salt, dried in vacuo over sulphuric acid, did not lose weight when afterwards heated for some time at 100° Cent. It partially fuses at the temperature of boiling water.

Iodide of calcium forms several interesting double salts, which I am at present engaged in examining.

III. "Further Note on Supersaturated Saline Solutions." By Charles Tomlinson, F.R.S. Received January 7, 1878.

In the paper which was read on the 20th of December last, I stated that during some years past I had often recognised the existence of a force in the atmosphere, which seemed to exert a powerful influence on nuclei, in determining the sudden solidification of supersaturated saline solutions. I had attempted to identify this force with evaporation, with desiccation, with crystallisation, with electricity, and with surface tension, and performed many experiments with this view, but without success. I therefore determined to make daily observations on the action of oils, &c., on these solutions, taking the typical one of sodic sulphate of a uniform strength (2 salt to 1 water), and to record the results from day to day, with especial reference to the state of the atmosphere. These observations have now extended over three or four months with this general result, that with a southerly or westerly wind, the action of oils is to throw down the seven-atom salt at temperatures above that at which it is naturally formed. The salt comes down on shaking the solution in the flask. In dull and damp weather it falls in a powdery form; in fine weather in crystals of the modified salt, and the powder previously formed in dull weather grows into well-shaped crystals. Covered flasks with oil in contact with the solutions, have thus been kept for days and weeks together, and although repeatedly

shaken, the only effect was to increase the quantity of the modified salt, while the liquid portion remained supersaturated. With a northerly or easterly wind the oil determined the sudden solidification of the salt either at once, or on shaking the flask, with the usual rise in temperature.

During the last three or four months, however, the occasions have been so rare on which the oils produced this last-named effect that, had I not had sufficient confidence in my published results, I should have been disposed to agree with other observers in the conclusion that oils have no nuclear action on these solutions. But those rare occasions (rare in consequence of the prevalence of southerly and westerly winds) when the oils did act with effect, satisfied me that, whatever the force may be which enables the oils to act, it is a true cause, capable of being discovered by continuing the observations.

The contradictory nature of the results will be understood from one example. On the 18th of December the wind, which had long been westerly and south-westerly, veered to the north-west, when I dropped castor-oil into three flasks, placed in the open air, and the solutions immediately solidified. To meet the objection that an oil that has been exposed to the air always contains minute particles of sodic sulphate, or of a salt isomeric therewith, I rinsed out a clean bottle with hot water, filled it half-full of the same, and poured upon it a little castor-oil. The cork, which had been kept in hot water, was then inserted, and the phial was repeatedly shaken, so that if any nuclear salt had been contained in the oil, it must have gone into solution, in which state, as I have already shown,* it is not nuclear. When the phial was cold, some of the washed oil was taken up with a pipette (previously kept in water), and dropped into six flasks, when the solutions all became solid. On the next day, and the day after, the wind was westerly and south-westerly with a damp fog. The experiment was repeated exactly as on the day before, but now the oil no longer produced solidification of the salt; it only threw down a little seven-atom salt when the flasks were shaken.

In reviewing this long series of experiments, in connection also with the singular activity of the drops of the solution exposed during the periods of bright weather last summer, when the wind was often easterly, I was led to suspect that ozone was the force I had so long been in search of. Accordingly a few days ago, when a damp fog prevailed, and the oils, &c., were singularly inert, I put a bit of scraped phosphorus into a wet tube, and then poured in some oil of cajuput. In the course of a few hours the oil became as singularly active as it had before been inert. The solutions immediately became solid as soon as a drop of the phosphorised oil touched the surface, or on gently shaking

* "Chemical News," 4th February, 1870.

the flask. On the 4th and 5th of January the air was still, but dark with a wet fog, accompanied now and then by a small rain, a state of weather which experience had shown to be hopeless as to any nuclear action on the part of the oils; but on these days drops of phosphorised oil of cajuput, castor-oil, and benzole produced the immediate solidification of solutions of sodic sulphate and of alum. I may also add that a minute bit of phosphorus was eminently nuclear, and also newly prepared phosphoric acid, whether dry or in aqueous solution.

(Addendum.) Received January 22, 1878.

In the series of observations referred to in the above note, the nucleus chiefly employed was oil of cajuput, with occasional reference to castor-oil and benzole. The reason why these three were selected was that after using them, the flasks could be easily cleaned by means of spirits of wine, preparatory to a fresh charge. Another reason was that, by working chiefly with one oil and with solutions of uniform strength, the results were more likely to be comparable than if a number of oils and solutions of different degrees of strength were used. The necessity for clean flasks, as insisted on in my former papers, was abundantly confirmed, especially on days when the wind was northerly and north-easterly. On such occasions, if any traces of the oil used in previous experiments remained in the flasks, the solutions would often solidify in cooling, or on being carried from room to room, although the mouths of the flasks were covered with paper caps or small beakers. The flasks chiefly used have oval bulbs of the capacity of $1\frac{1}{4}$ oz., with stems 3 inches in length and about $\frac{3}{8}$ inch in diameter. Each bulb was filled about half with the filtered solution by means of a small funnel with a long stem, and this was dipped into water after each charge, so as not to soil the neck of the next flask. Each flask was then reboiled until steam issued from the neck, when it was removed from the lamp, its cover was put on, and it was then set upright in a rack to cool. These narrow-necked flasks, if clean, retain the solutions admirably; they may even be left uncovered during many hours, and even days, especially if the necks are inclined towards their support. An uncovered flask has thus remained a fortnight without change.

Several eminent observers have stated with great persistency that oils have no nuclear action on supersaturated saline solutions; or if they appear to be active, their activity is due to minute particles of the salt in the air or in the oils themselves. In order to get rid of these supposed nuclear particles, I distilled a number of active essential oils, and found the distillates to be inactive, thus apparently justifying the objection referred to. I also found that ether, methylated spirits of wine, wood spirit, naphtha, benzole, and a few other liquids, were

active on certain days and inactive on others; that when these were active their fresh distillates were not, or at least that they required time in order to become nuclear. When first added to the solution, the flask could be shaken, and a good deal of the modified salt would be thrown down, and then perhaps after a half-an-hour or an hour's repose the solutions would become solid, although the flasks were covered again after the drops had been added to the solutions. I also found that different specimens of the same oil, &c., in separate bottles, sometimes behaved differently on the same day, and even that an active essential oil might be made inactive simply by filtering.

But no sooner was I led to the conclusion that ozone, or an oxidised condition, has a powerful influence on the nuclear action of oils, &c., than the reason for the various contradictory results became plain to me. Supposing that other observers worked with what would appear to be the best, that is, with pure materials (Löwel, for example, recrystallised his sodic sulphate, and used distilled water for his solutions), the fixed oils used by them would probably be recently refined and the essential oils redistilled. Hence it is now apparent to me how it was that they obtained negative results, while the very precautions against the entrance of nuclei, as taken by M. Viollette and Mr. Liversidge, were such as to deprive the oil of nuclear action, by cutting off the influence of the external air.*

In order to illustrate this influence more fully, I will describe a few out of a large number of experiments that I have recently made.

An oil of turpentine, that had stood on my shelf during many years, was found to be powerfully active. A portion of it was redistilled, and the distillate had no action in solidifying the solution of sodic sulphate (2 to 1), although the flask was repeatedly shaken during the day.†

A portion of the distillate, contained in a dropping-tube, was taken into the open air and allowed to fall drop by drop into a small clean beaker. The oil immediately became active, several of the last-named solutions becoming solid as soon as the oil reached the surface, or on gently shaking the flask.

But in such a case, as it has been affirmed over and over again, the

* Mr. Liversidge ("Proc. Roy. Soc.," xx, 427) took up oil in a dropping-tube, and then closed its upper opening, and passing the tube through a plug fitted it tightly into the neck of the flask containing the supersaturated solution. After waiting an hour or two, lest any nuclear particles should have entered the flask, he allowed a drop of oil to fall upon the solution. Under such conditions he found the oil to be inactive. Using the same method, I found that the phosphorised oil of cajuput, an oxidised oil of turpentine, ether, and absolute alcohol were active: so also was glacial acetic acid, but not so promptly as the others.

† 8th February. I have since noticed that during a north or a north-east wind, the distillate of oil of turpentine may become nuclear while being transferred from the condenser to the dropping-tube. In some cases hydric peroxide may be the force that confers the nuclear action.

oil in falling through the air, must have caught up particles of sodic sulphate, and these, and not the oil, formed the true nucleus.

To meet this objection, I took a portion of the same solution, contained in a wide-necked flask, to the same spot in the open air of my garden, and with a clean dropping-tube, just taken out of water, raised a quantity of the supersaturated solution, and allowed it to fall through the air drop by drop into the flask. This was done repeatedly, a south-easterly wind blowing gently at the time, but the solution remained unchanged.

An old oil of lavender (marked "Mitcham") was active. It was redistilled and treated as in the preceding case, with the same results.

Methylated spirits of wine were inactive during a south wind, but soon became active on being allowed to drop through the air in my garden. Benzole became similarly active, but not quite so readily.

An old paraffin oil (marked "Belmontine") was active. Its distillate was inactive, but on adding to it a few drops of ozonised water, it became active.

A solution of sodic sulphate, to which some drops of oil of cajuput had been added the day before, did not solidify, although the solution had been repeatedly shaken in the flask. A few drops of ozonised water were added, and the solution became solid on gently shaking the flask.

Oil of sweet almonds was inactive; but on being dropped through the outer air, it became active. About twenty old essential oils were all powerfully active.

A piece of apple, cut from the centre of the fruit, was inactive; several pieces, similarly cut and exposed to the air until they had become brown, were active.

Bits of sponge, which were described in my former paper as being inactive, were found to be active during a northerly and north-easterly wind. The same remark applies to charcoal.

These experiments may suffice to show what is really the function of the air with respect to nuclei, and to account for and bring into harmony with a general principle a large amount of contradictory evidence on the part of many observers, which, during three-quarters of a century has disturbed this branch of physico-chemical science.

Why the oils, &c., in their oxidised condition are nuclear, and in their pure condition not, is a question which I am now endeavouring to answer.

IV. "Sur une Equation Différentielle du 3me Ordre." By Prof. Francesco Brioschi. Communicated by W. Spottiswoode, M.A., Treas. Roy. Soc. Received February 18, 1878.

1. Dans une note qui va paraître dans le Vol. IXme des "Annali di Matematica," et de laquelle j'ai l'honneur de présenter un exemplaire à part à la Société Royale, j'ai considéré l'équation différentielle linéaire du second ordre :

$$\frac{d^2y}{du^2}=\left[\frac{n(n+2)}{4}k^2sn^2u+h\right]y \quad \ldots\ldots (1)$$

étant h une constante, et n un nombre positif entier. Cette équation différentielle, qui pour n pair coincide avec celle que Lamé avait rencontrée dans ses études sur les surfaces isothermes, et à laquelle Mr. Hermite a dédié récemment ses importantes recherches, a la propriété très-rémarquable, pour le cas de n impair, d'être intégrable *algebriquement*. Soient y_1, y_2, deux intégrales particulières de l'équation différentielle (1) ; on a, comme il est connu, que :

$$y_2\frac{dy_1}{du}-y_1\frac{dy_2}{du}=C \quad \ldots\ldots (2)$$

C constante, et en conséquence si l'on pose :

$$\eta=\frac{y_1}{y_2}$$

on aura l'équation différentielle du troisième ordre :

$$[\eta]_u+2\left(\frac{n(n+2)}{4}k^2sn^2u+h\right)=0 \quad \ldots\ldots (3)$$

ayant désigné avec Mr. Klein par $[\eta]_u$ l'expression :

$$\frac{d^2\log\frac{d\eta}{du}}{du^2}-\tfrac{1}{2}\left(\frac{d\log\frac{d\eta}{du}}{du}\right)^2$$

2. En posant :

$$x-e_1=(e_2-e_1)sn^2u$$
$$x-e_2=(e_1-e_2)cn^2u$$
$$x-e_3=(e_1-e_3)dn^2u$$

et $k^2=\frac{e_2-e_1}{e_3-e_1}$, on peut transformer l'équation différentielle (3) au moyen de la formule générale :

$$[\eta]_x=[u]_x+[\eta]_u\left(\frac{du}{dx}\right)^2$$

et l'on obtient après quelques réductions l'équation suivante:

$$[\eta]_x=\frac{1}{8\phi^2}[16\phi\psi-4\phi\phi''+3\phi'^2]\ldots\ldots(4)$$

dans laquelle:

$$\phi=4x^3-g_2x-g_3=4(x-e_1)(x-e_2)(x-e_3)$$

$$\psi=-\left[\frac{n(n+2)}{4}(x-e_1)+h(e_3-e_1)\right]$$

Cette équation différentielle (4), qui est de la forme de celle considerée il y a longtemps par Mr. Kummer dans ses recherches sur les séries hypergéométriques de Gauss, peut s'intégrer au moyen des résultats obtenus dans la note rappellée ci-dessus. En effet en posant $y_1y_2=v$, on déduit très-facilement de l'équation (2) que:

$$\frac{d\eta}{du}=\frac{C\eta}{v}$$

ou aussi:

$$\frac{1}{\eta}\frac{d\eta}{dx}=\frac{C}{v\sqrt{\phi(x)}}$$

Mais pour le cas de n pair $=2m$, on a:

$$v=\zeta(x)$$

étant $\zeta(x)$, un polynome en x du dégré m, dont les coefficients sont des fonctions déterminées de h, e_1, e_2, e_3. On aura donc dans ce cas:

$$\eta=e^{CZ(x)}$$

ayant posé:

$$Z(x)=\int\frac{dx}{\zeta(x)\sqrt{\phi(x)}}$$

Dans le cas que n impair $=2m+1$, on a:

$$v=\zeta(x)\sqrt{x-\xi},$$

$\zeta(x)$ étant encore un polynome en x du dégré m, et ξ une racine de l'équation $\phi(x)=0$, c'est-à-dire $\xi=e_1$, e_2, e_3. Dans ce cas en posant $\phi(x)=(x-\xi)\mu(x)$, on aura:

$$Z(x)=\int\frac{dx}{\zeta(x)(x-\xi)\sqrt{\mu(x)}}$$

intégrable par des fonctions logarithmiques. En posant:

$$t_1(a)=\sqrt{\mu(x)}-\sqrt{\mu(a)}-2(x-a)$$

$$t_2(a)=\sqrt{\mu(x)}+\sqrt{\mu(a)}-2(x-a)$$

on a ainsi pour n impair :

$$\eta=\left(\frac{t_1(\xi)}{t_2(\xi)}\right)^{\frac{1}{2}}\Pi_s\frac{t_1(x_s)}{t_2(x_s)}$$

où x_s est une racine de l'équation $\zeta(x)=0$.

3. Si dans l'équation (4) on suppose :

$$h(e_3-e_1)=\frac{n(n+2)}{4}e_1$$

ou : $$h=-\frac{n(n+2)}{12}(1+k^2)$$

et $g_2=0$, l'équation même devient :

$$[\eta]_x=\frac{x}{2(4x^3-g_3)^2}[(n^2+2n+24)g_3-4(n-1)(n+3)x^3].$$

Soit $x^3=\frac{1}{4}g_3z$, on aura :

$$[\eta]_x=\frac{x}{2g_3(1-2)^2}[n^2+2n+24-(n-1)(n+3)z]$$

et $$[x]_z=\frac{4}{\eta}\frac{1}{z^2}$$

en conséquence l'équation de transformation :

$$[\eta]_z=[x]_z+[\eta]_x\left(\frac{dx}{dz}\right)^2$$

donnera : $$[\eta]_z=\frac{1-\lambda^2}{2z^2}+\frac{1-\nu^2}{2(1-z)^2}-\frac{\lambda^2-\mu^2+\nu^2-1}{2z(1-z)}$$

étant : $$\lambda=\frac{1}{3},\ \mu=\frac{n+1}{6},\ \nu=\frac{1}{2}$$

On en déduit que en posant :

$$a=\frac{n+2}{12},\ \beta=-\frac{n}{12},\ \gamma=\frac{2}{3}$$

on aura : $$\eta=\frac{\mathrm{F}(a,\ \beta,\ \gamma,\ z)}{\mathrm{F}(a,\ \beta,\ a+\beta-\gamma+1,\ 1-z)}$$

désignant par $\mp$ une série hypergéométrique.

Ces séries sont donc exprimables, dans le cas que j'ai ici considéré, par la fonction $\mathrm{Z}(x)$ introduite supérieurement.

Analoguement si l'on suppose $g_3=0$, on trouvera en posant :

$$g_2{}^3=4x^2,$$

pour a, β, γ les valeurs suivantes :

$$a=\frac{n+2}{8},\ \beta=-\frac{n}{8},\quad =\frac{2}{3}$$

February 28, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

The Right Hon. Sir William Henry Gregory 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 certain Definite Integrals." By W. H. L. Russell, F.R.S. Received January 10, 1878.

The integrals in the three preceding papers may nearly all be included under the following general theorem:—

Let $\phi(x)=u_0+u_1x+u_2x^r+\ldots u_rx^r+\ldots$ which series is, of course, supposed to be convergent, and let Π be a general functional symbol, such that

$$\Pi\phi(x)=u_0\chi(0),\quad +u_1\chi(1)\,x+u_2\chi(2)\quad x^2+\ldots$$

and let $\chi(\theta)=\int U d\theta$, $\chi(1)=\int UV d\theta\ldots\chi(r)=\int UV^r d\theta\ldots$ the integrals being supposed to be taken within certain limits: then

$$\int U\varphi(Vx)d\theta=\Pi\phi(x). \tag{62.}$$

The following integrals require for the most part other methods to determine them:

$$\int_0^\omega \frac{dx}{(1+x^2)^2(1-2a\cos x+a^2)}=\frac{\pi}{4}\frac{1}{1-a^2}\left\{\frac{\epsilon+a}{\epsilon-a}+\frac{2a\epsilon}{(\epsilon-a)^2}\right\}. \tag{63.}$$

More generally we may obtain

$$\int_0^\infty \frac{dx}{(1+x^2)^r(1-2a\cos x+a^2)}. \tag{64.}$$

$$\int_0^\infty \frac{dx\,.\,x\sin ra}{(1+x^2)^r(1-2a\cos x+a^2)}. \tag{65.}$$

$$\int_0^\infty \frac{dx}{(1+x^2)^r}\cdot\frac{\sin ax}{\sin bx}. \tag{66.}$$

$$\int_0^\infty \frac{dx}{(1+x^2)^r}\cdot\frac{\cos ax}{\cos bx}. \tag{67.}$$

$$\int_0^\infty \frac{dx}{1+x^{2r}}\cdot\frac{\sin ax}{\sin bx}. \tag{68.}$$

$$\int_0^\infty \frac{dx}{1+x^{2r}}\cdot\frac{\cos ax}{\cos bx}. \tag{69.}$$

$$\int_0^\infty \frac{(x-a^2x)dx}{((1+a+a^2)\sin ax-a\sin 3ax)\,(1+x^2)^r}. \tag{70.}$$

$$\int_0^\infty \frac{(1-a^2)dx}{((1+a+a^2)\cos ax+a\cos 3ax)(1+x^2)^r}. \tag{71.}$$

The integral (68.) calls for some particular remarks. Since by the ordinary rules:

$$\sin\theta+\sin 3\theta+\ \ldots\ \sin(2r+1)\theta=\frac{2\sin\theta-\sin(2r+3)\theta+\sin(2r+1)\theta}{2(1-\cos 2\theta)}$$

whence also:

$$\int_0^\infty dx,\ \sin ax\ .\ \frac{2\sin bx-\sin(2n+3)bx+\sin(2n+1)bx}{(1+x^{2r})(1-\cos 2bx)}$$

$$=2\int_0^\infty \frac{dx\sin ax\sin bx}{1+x^{2r}}+2\int_0^\infty \frac{dx\sin ax\sin 3bx}{1+x^{2r}}$$

$$+\ \ldots\ +2\int_0^\infty \frac{dx\sin ax\sin(2n+1)bx}{1+x^{2r}}.$$

Now

$$2\sin ax\sin bx=\cos(b-a)x-\cos(b+a)x$$

$$2\sin ax\sin 3bx=\cos(3b-a)x-\cos(3b+a)x$$

$$\ldots\qquad =\qquad \ldots$$

$$\int_0^\omega \frac{dx\cos mx}{1+x^{2r}}=C_1\epsilon^{-c_1m}+C_2\epsilon^{-c_2m}\ldots+C_r\epsilon^{-c_sm}.$$

Where $-c_1$, $-c_2$, $-c_3\ldots-c_s$ are all the roots of the equation $x^{2r}+1=0$ which have a negative modulus, and C_1, C_2, $C_3\ldots C_s$ are certain constants whose values have been assigned by Poisson: hence

$$\int_0^\omega dx\ .\ \sin ax\ .\ \frac{2\sin bx-\sin(2n+3)bx+\sin(2n+1)bx}{(1+x^{2r})(1-\cos 2bx)}=C_1\phi(c_1)+C_2\phi(c_2)$$

$$+\ \ldots+C_s\phi(c_s)_1,\text{ when }\phi(c)=(\epsilon^{cx}-\epsilon^{-c\alpha})\ .\ \frac{\epsilon^{-cb}-\epsilon^{-c(2n+3)b}}{1-\epsilon^{-2cb}}.$$

Now let (n) increase without limit, then

$$\sin(2n+3)bx=\sin 2n\left(1+\frac{3}{2n}\right)bx=\sin 2nx.$$

$$\sin(2n+1)bx=\sin 2n\left(1+\frac{1}{2n}\right)bx=\sin 2nx.$$

Hence the integral in the second member vanishes, and

$$\int_0^\omega \frac{dx}{1+x^{2r}}\cdot\frac{\sin ax}{\sin bx}=C_1\frac{(\epsilon^{c_1a}-\epsilon^{-c_1a})\epsilon^{c_1b}}{\epsilon^{2c_1b}-1}+C_2\frac{(\epsilon^{c_2a}-\epsilon^{-c_2a})\epsilon^{c_2b}}{\epsilon^{2c_2b}-1}+\ \ldots\ (s\text{ terms}).$$

It is scarcely necessary to observe, that this process implies that (a) must be less than (b), otherwise the integral $\int_0^\omega \frac{\cos mx\ .\ dx}{1+x^{2r}}$ as here used will be discontinuous.

(72.) $$\int_0^\pi \frac{d\theta\sin^{2r}\theta}{\epsilon^{\cos\theta}+1}=\frac{(2r-1)(2r-3\ldots}{2r(2r-2)\ldots}\ \frac{1}{2}\cdot\frac{\pi}{2}.$$

(73.) $$\int_0^\pi \frac{d\theta}{(1+c^2\sin^2\theta)(\epsilon^{\cos\theta}+1)}=\frac{\pi}{2\sqrt{1+c^2}}.$$

$$(74.)\quad \int_0^{\pi} \frac{d\theta\phi(\sin^2\theta)}{\epsilon^{\cos\theta}+1} = \frac{1}{2}\int_0^{\pi} d\theta\phi(\sin^2\theta).$$

Now let us define four quantities, ρ_1, ρ_2, μ_1, μ_2, thus

$$\rho_1 = \frac{1}{\sqrt{2}}\left\{\sqrt{1+a^2+a^4}+\left(1+\frac{a^2}{2}\right)\right\}^{\frac{1}{2}},$$

$$\rho_2 = \frac{1}{\sqrt{2}}\left\{\sqrt{1+a^2+a^4}-\left(1+\frac{a^2}{2}\right)\right\}^{\frac{1}{2}},$$

$$\mu_1 = \rho_2\sqrt{3}-\rho_1+1,\quad \mu_2 = \rho_2+\rho_1\sqrt{3}-\sqrt{3},$$

then we shall have

$$(75.)\quad \int_1^{\omega} \frac{x^3+a^3}{x^3-a^3}\frac{dx}{\sqrt{x^2-1}} = \pi\{\sin^{-1}a+\sin^{-1}(\rho_2\sqrt{3}-\rho_1)a\}.$$

$$(76.)\quad \int_0^{\pi} \frac{d\theta\cos r\theta}{1+a^3\cos^3\theta} = \frac{\pi}{3\sqrt{1-a^2}}\left\{\frac{\sqrt{1-a^2}-1}{a}\right\}^r + \frac{2\pi}{3(2a)^2(\rho_1^2+\rho_2^2)}$$

$$\left\{\rho_1\left(\mu_1^r - r\frac{r-1}{2}\mu_1^{r-2}\mu_2^2 + r\cdot\frac{r-1}{2}\cdot\frac{r-2}{3}\cdot\frac{r-3}{4}\mu_1^{r-4}\mu_2^4 - \ldots\right)\right.$$

$$\left. -\rho_2\left(r\mu_1^{r-1}\mu_2 - r\cdot\frac{r-1}{2}\cdot\frac{r-2}{3}\mu_1^{r-3}\mu_2^3 + \ldots\right)\right\}.$$

$$(77.)\quad \int_0^{\pi} \frac{d\theta\epsilon^{\theta}\cos\theta}{1+a^3\cos^3\theta} = \frac{\pi}{3\sqrt{1-a^2}}\epsilon^{\frac{\sqrt{1-a^2}-1}{a}}$$

$$+\frac{2\pi}{3(\rho_1^2+\rho_2^2)}\epsilon^{\frac{\mu_1}{2a}}\left(\rho_1\cos\frac{\mu_2}{2a}-\rho_2\sin\frac{\mu_2}{2a}\right).$$

$$(78.)\quad \int_0^{\pi} d\theta\log_{\epsilon}(1+a^3\cos^3\theta) = \pi\log_{\epsilon}\frac{1+\sqrt{1-a^2}}{8}$$

$$+\pi\log_{\epsilon}(1+\sqrt{(a^2+2\sqrt{1+a^2+a^4}+2)}+\sqrt{1+a^2+a^4}).$$

$$(79.)\quad \int_0^{\pi} d\theta\cos r\theta\log_{\epsilon}(1+a^3\cos^3\theta) = \frac{1}{r}\left\{\frac{\sqrt{1-a^2}-1}{a}\right\}^r +$$

$$\frac{2}{r(2a)^r}\left(\mu_1^r - r\cdot\frac{r-1}{2}\mu_1^{r-2}\mu_2^2 + r\cdot\frac{r-1}{2}\cdot\frac{r-2}{3}\cdot\frac{r-3}{4}\mu_1^{r-4}\mu_2^2 - \ldots\right).$$

(80.) From (79.) we can immediately deduce

$$\int_0^{\pi} \frac{m^2+m\cos\theta}{1+2m\cos\theta+m^2} d\theta\log_{\epsilon}(1+a^3\cos^3\theta).$$

(81.) If we denote integral (78.) by p, and (80.) by q, we have at once

$$\int_0^{\pi} \frac{d\theta\log_{\epsilon}(1+a^3\cos^3\theta)}{1+2m\cos\theta+m^2} = \frac{p-2q}{1-m^2}.$$

More generally we may obtain

(82.) $\int_1^\omega \frac{x^{2r+1}+a^{2r+1}}{x^{2r+1}+a^{2r+1}} \frac{dx}{\sqrt{x^r-1}}$. (83.) $\int_0^\pi \frac{\cos r\theta \,.\, d\theta}{1+a^r\cos^r\theta}$.

(84.) $\int_0^\pi d\theta \log_\epsilon (1+a^r\cos^r\theta)$. (85.) $\int_0^\pi \frac{d\theta \log_\epsilon (1+a^r\cos^r\theta)}{1+2m\cos\theta+m^r}$.

and many others.

II. "On the Reversal of the Lines of Metallic Vapours." By G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. No. I.

Since the celebrated paper by Kirchhoff, "On the relation between the radiating and absorbing powers of different bodies for light and heat," in which he detailed the remarkable experiments of reversing the lines of lithium and sodium by sunlight and by the vapours of those metals in the flame of a Bunsen's burner, and mentioned the reversal of the brighter lines of potassium, calcium, strontium and barium when the deflagration of the chlorates with milk-sugar was used instead of the flame of a Bunsen's burner, further researches in the same direction have been made by Cornu, Lockyer, and Roberts. The method adopted by Cornu, which had been previously used by Foucault, is one of great ingenuity, dependent upon so arranging the electric arc that the continuous spectrum of the intensely heated poles is examined through an atmosphere of the metallic vapours volatilized around them. By this means Cornu succeeded in reversing several lines in the spectra of the following metals in addition to those above-mentioned, viz., thallium, lead, silver, aluminum, magnesium, cadmium, zinc, and copper. He observed that in general the reversal began with the least refrangible of a group of lines, and gradually extended to the more refrangible lines of the group; and he drew the conclusion that a very thin layer of vapour was sufficient for the reversal. It may be noted that in almost every case the lines seen by him to be reversed were the more highly refrangible of the lines characteristic of each metal, confirming generally the opinion expressed by Stokes in a letter to Lockyer in the Proceedings of the Royal Society for 1876, in which he introduces for the first time the idea of the persistency of different rays with reference to temperature.

The method adopted by Lockyer in the first instance was to view the electric arc through the vapours of the metals volatilized in a stream of hydrogen in a horizontal iron tube.

The iron tube had its ends covered with glass plates, and was heated in a furnace supplied with charcoal. By this means he did not succeed in observing any new reversal of bright lines, but he noticed an unknown absorption line which sometimes appeared when zinc was experimented on. He confirmed, however, the channelled-space absorption spectra observed by Roscoe and Schuster, in the cases of potassium and sodium, and recooded channelled-space spectra in the case of antimony, phosphorus (?), sulphur (previously observed by Salet) and arsenic (probably). "As the temperature employed for the volatilization of the metals did not exceed bright redness, or that at which cast-iron readily melts, the range of metals examined was necessarily limited; and in order to extend these observations to the less fusible metals, as well as to ascertain whether the spectra of those volatilized at the lower temperature would be modified by the application of a greater degree of heat," a new series of experiments were undertaken by Lockyer and Roberts, in which the combined action of a charcoal furnace and the oxy-hydrogen blowpipe was employed. By this means they obtained no new reversal of a metallic line, but they observed channelled-space spectra in the cases of silver, manganese, chromium, and bismuth. They observed, however, that the metal thallium gave the characteristic *bright* green line, the light of the arc not being reversed.

In the above-mentioned experiments the coolness of the ends of the tube, which acted as condensers of the metallic vapours, and the inequality of density and temperatures necessarily produced by the maintenance of a current of hydrogen in the tube, appear to us to account for the complication and uncertainty of the results of the observations.

In order to examine the reversal of the spectra of metallic vapours, we find it more satisfactory to observe the absorptive effect produced on the continuous spectrum emitted by the sides and end of the tube in which the volatilization takes place. For this purpose we find it convenient to use iron tubes about half-an-inch in internal diameter, and about 27 inches long, closed at one end, thoroughly cleaned inside, and coated on the outside with borax, or with a mixture of plumbago and fireclay. These tubes are inserted in a nearly vertical position in a furnace fed with Welsh coal, which will heat about 10 inches of the tube to about a welding heat, and we observe through the upper open end of the tube, either with or without, a cover of glass or mica. To exclude oxygen, and avoid as much as possible variations of temperature, we introduce hydrogen in a gentle stream by a narrow tube into the upper part only of the iron tube, so that the hydrogen floats on the surface of the metallic vapour without producing convection currents in it. By varying the length of the small tube conveying the hydrogen, we are able to determine the height in the tube to which the

metallic vapour reaches, and to prevent further displacement of the vapour, and thus to maintain different lengths of the iron tube full of metallic vapour at a comparatively constant temperature for considerable periods of time.

By this means the following observations have been made up to the present time.

The first metal experimented on was thallium, one of the most volatile of metals. On inserting some pieces into the hot tube, we had at first great difficulty in avoiding oxidation at the upper surface, and we observed no reversal. However, after arranging the current of hydrogen so as to keep the tube free from air, but without any rapid movement of the gas, we succeeded in seeing the characteristic line reversed, and maintained it so for a considerable time.

The metal indium, closely allied in its behaviour and volatility to thallium, was next examined, and although the amount of this metal at our disposal was not large, we observed the bright blue line reversed. This was most plainly visible when that portion of the vapour which was nearest to the sides of the tube was looked through.

We had great difficulty in preventing the oxidation of magnesium in the tube, and in using tubes wider than half-an-inch did not succeed in getting any reversal, but with half-inch tubes the *b* lines were clearly and sharply reversed, also some dark lines, not measured, were seen in the blue. The sharpness of these lines depended on the regulation of the hydrogen current, by which we could cool the upper stratum of vapour at will.

A piece of metallic lithium was introduced, and gave no results. Sodium was next added in the same tube, and this did not bring out the reversal of the lithium lines. Similarly, chloride of lithium and metallic sodium, introduced together, gave no better results. To a tube containing potassium vapour, some lithium chloride was added, but no lithium line appeared. On adding metallic sodium to this atmosphere, and more lithium chloride, the bright-red lithium line appeared sharply reversed, and remained well defined for a long time. It is worthy of observation that the lithium line was only reversed in a mixture of the vapours of potassium and sodium, and it seems highly probable that a very slightly volatile vapour may be diffused in an atmosphere of a more volatile metal, so as to secure a sufficient depth of vapour to produce a sensible absorption. This would be analogous to well-known actions which take place in the attempt to separate organic bodies of very different boiling points by distillation, where a substance of high boiling point is always carried over, in considerable quantity, with the vapour of a body boiling at a much lower temperature. It is a matter for future investigation how far chemical interactions taking place in a mixture of metallic vapours affect the volatility of a third body, and what relation, if any, this may have to

such phenomena as the increased fusibility of mixtures of salts of potassium and sodium, and the well-known fluidity of the alloy of those metals.

As we have had occasion to use sodium and potassium in our tubes, we have had opportunities of observing the absorption spectra of these metals, and we find that there is a great deal yet to be observed in regard to these spectra. Up to the present time, we have not observed any of the appearances noted by Lockyer, "On a new class of absorption phenomena," in the Proceedings of the Royal Society, vol. xxii, but we have repeatedly noted the channelled-space spectrum of sodium described by Roscoe and Schuster in the same volume of the Proceedings. We observed in our tubes no channelled-space absorption by potassium, but continuous absorption in the red, and one narrow absorption band, with a wave-length of about 5,730, not corresponding with any bright line of that metal.

With reference to the absorption spectrum of sodium vapour, we may remark that it is by no means so simple as has been generally represented. The fact that the vapour of sodium in a flame shows only the reversal of the D lines, while the vapour, volatilized in tubes, shows a channelled-space absorption corresponding to no known emission spectrum, appears to be part of a gradational variation of the absorption spectrum, which may be induced with perfect regularity. Experiments with sodium, carried out in the way we have described, exhibit the following succession of appearances, as the amount of vapour is gradually diminished, commencing from the appearance when the tube is full of the vapour of sodium, part of it condensing in the cooler portion of the tube, and some being carried out by the slow current of hydrogen. During this stage, although the lower part of the tube is at a white heat, we have always noticed, as long as the cool current of hydrogen displaced metallic vapour, that, on looking down the tube, it appeared perfectly dark. The first appearance of luminosity is of a purple tint, and, with the spectroscope, appears as a faint blue band, commencing with a wave-length of about 4,500, and fading away into the violet. Next appears a narrow band in the green, with a maximum of light, with a wave-length of about 5,420, diminishing in brightness so rapidly on either side as to appear like a bright line. This green band gradually widens, and is then seen to be divided by a dark band, with a wave-length of about 5,510. Red light next appears, and between the red and green light is an enormous extension of the D absorption line, while a still broader dark space intervenes between the green and the blue light. The dark line in the green (wave-length about 5,510) now becomes more sharply defined. This line appears to have been observed by Roscoe and Schuster, and regarded by them as coinciding with the double sodium line next in strength to the D lines, but it is considerably more refran-

gible than that double line. In the next stage, the channelled-space spectrum comes out in the dark space between the green and blue, and finally, in the red. Gradually the light extends, the channels disappear, the D line absorption narrows, but still the dark line in the green is plainly discernible. Lastly, there is only D lines absorption. So far as we can judge, the blue and the streak of green light at first observed are due to luminosity of the vapour itself, where it is somewhat cooled, the later stages being mixed phenomena of absorption and emission.

As the absorption line, with wave-length about 5,510, which we may call for shortness the second absorption line of sodium, has not been distinctly recorded by other observers, we have endeavoured to trace it under somewhat different conditions from that of the vapour volatilized in white hot iron tubes. This absorption-line is easily seen when a gas flame is observed through a horizontal glass tube, about three inches long, containing sodium volatilized in the middle of the tube by the heat of a Bunsen's burner, and equally well whether the tube contains hydrogen or nitrogen besides sodium. We have also observed the same absorption line when a piece of commercial magnesium ribbon (which always contains sodium) is ignited in a horizontal position, so that the metal melts and produces an elongated flame. It is of some interest to note that absorption lines of about this wave-length, in the solar spectrum, are given by Kirchoff and Ängström not corresponding with emission lines of known elementary bodies.

When potassium vapour is observed, whether in the iron tube or in a glass tube, an absorption line is seen, with a wave-length of about 5,730, which is more refrangible than the yellow double emission line of potassium, and does not correspond to any known bright line of that metal.

We reserve, for a future communication, the discussion of the identity or non-identity of these absorption lines with lines in the solar spectrum and the inferences which may be drawn from such determination.

The method of observation we have described may be used to observe emission spectra as well as absorption spectra, for if the closed end of the tube be placed against the bars of the furnace so as to be relatively cooler than the middle of the tube, the light emitted by the vapours in the hottest part is more intense than that emitted by the bottom of the tube. This succeeds admirably with sodium, but we have not specially observed it with other vapours.

III. "Contributions to the Physiology of Batrachian Lymph-Hearts." By JOHN PRIESTLEY, Assistant-Lecturer in Physiology in Owens College, Manchester. (From the Physiological Laboratory of Owens College.) Communicated by Professor ARTHUR GAMGEE, F.R.S. Received January 31, 1878.

The following paper contains a summary of observations made in order to obtain data for a comparison between the blood-heart and lymph-hearts of frogs. The comparison is to include the actions of electrical currents and of the so-called cardiac drugs, atropia, physostigma, muscaria, antiar, &c.; but this paper will treat only of the electrical portion of the work.

Before proceeding to the collection of facts especially fitted for comparison, the normal anatomical and physiological arrangements of the lymph-hearts were examined; and, as the result of this examination, the author is able to confirm the discoveries of previous observers in the following points:—

1. The lymph-hearts of *Rana temporaria* are muscular sacs, whose walls consist of branched, anastomotic, transversely striated muscular fibres, abundantly nucleated (Leydig,* Waldeyer†). A cursory examination failed to discover any ganglionic nervous elements among the muscular fibres themselves, their absence having already been noticed by Volkmann‡ and Waldeyer;§ but nerve-ganglia are said to be found near the hearts, in their connective tissue environment (Waldeyer).‖

2. The lymph-hearts beat with a mean rate of 60–70 a minute, and are independent in their rhythm of one another, of the blood-heart, and of the respiratory movements (J. Müller)¶. But the normal pulsation is not quite regular, being occasionally broken by sudden pauses or by periods of increased rapidity of beat. These interruptions seem to be partly due to movements of the animal, but in part to hitherto undefined causes.

3. The hearts receive nerve-fibres from the spinal cord, which descend in the second and tenth spinal nerves respectively for the anterior and posterior pairs of hearts (Volkmann,** Eckhard,†† Schiff ‡‡).

* Leydig, Lehrbuch der Histologie, 1857.

† Waldeyer, Henle u. Pfeufer's Zeitsch. für rat. Med., 3rd Series, vol. xxi, 1864.

‡ Volkmann, Müller's Archiv, 1844, p. 419.

§ Waldeyer, *loc. cit.* ‖ Waldeyer, *loc. cit.*

¶ J. Müller, Philosophical Transactions of Roy. Soc. London, 1833. Poggendorff's Annalen, 1832. Müller's Archiv, 1834.

** Volkmann, *loc. cit.*

†† Eckhard, Henle u. Pfeufer's Zeitsch., 1849, vol. viii.

‡‡ Schiff, Henle u. Pfeufer's Zeitsch., 1850, vol. ix.

4. The normal rhythm of the heart is dependent on the soundness of certain regions of the spinal cord, viz., for the anterior pair, of the region opposite the 3rd vertebral body (Volkmann*), for the posterior pair, of the region opposite the 6th. This statement is a correction of the view commonly held in respect of the spinal region concerned in the rhythm of the posterior hearts; for all previous observers seem to have followed Volkmann in saying that the region of the 8th vertebral body is that implicated.

If these regions are injured, or if the nerves proceeding from them to the hearts are intercepted, either the hearts are brought to rest permanently, or their natural pulsation is disorganised and reduced for a long time to more or less irregular fluttering. These spinal centres seem capable of inhibition by strong sensory (afferent) stimuli, even in the absence of the encephalon.

5. The natural pulsation of the lymph-hearts is constantly controlled from an encephalic inhibitory centre in the optic lobes, separation from which is, therefore, at once followed by increased rate of beat (Suslowa.)† This inhibitory centre is said by Goltz ‡ to be roused into stronger action by powerful mechanical stimuli proceeding along the vagi from the heart, or proceeding from the abdominal viscera in the *Klopfversuch.*

6. The disorderly contractions that follow division of the lymph-cardiac spinal nerves may give place to a regular pulsation (Eckhard,§ Schiff,|| Heidenhain ¶). The restoration does not usually take place until after many days, but when established, the rhythmical beating is independent of the spinal cord, not ceasing, like the pulse of normal lymph-hearts, when the central nervous system is destroyed (Goltz,** Waldeyer††). The cause of these movements is not yet fully understood; but the movements themselves are abolished by local or general application of curare in solution.

Action of Electrical Currents on the Lymph-Hearts of Frogs.

A complete description of the action of electrical currents on the lymph-cardiac mechanism includes the action of induced currents and of constant currents. The latter were generated by Grove's or Daniell's elements, the former by a Du Bois' induction coil. The portions of the lymph-cardiac apparatus tested were brought into the circuit by appli-

* Volkmann, *loc. cit.*

† Suslowa, Centralblatt f. med. Wis. 1867, p. 832. Henle u. Pfeufer's Zeitsch., 1868.

‡ Goltz, Centralblatt f. med. Wis. 1863, p. 17; p. 497. Ibidem, 1864, p. 691.

§ Eckhard, *loc. cit.* || Schiff, *loc. cit.*

¶ Heidenhain, Disquisitiones de nervis cordis, etc. Berlin. 1854. (Dissertation.)

** Goltz, *loc. cit.* †† Waldeyer, *loc. cit.*

cation either of common non-polarizable electrodes or of electrodes of platinum wire.

I.—Action of Induced Interrupted Currents.

a. On the lymph-cardiac muscle detached from its nerve (as an ordinary voluntary muscle may be detached) *by means of curare.*—The electrodes were laid on the back after the removal of the skin, as close to the posterior lymph-hearts as possible. Under these circumstances the lymph-hearts behave exactly like the surrounding striated muscles, not differing from them to any remarkable extent, either in minimal stimulus or in mode of contraction.

b. On the lymph-cardiac spinal nerves after anatomical separation from the lymph-cardiac spinal centre.—The spine and cord were snipped through at the level of the 7th vertebra, and one non-polarisable electrode was applied to the exposed lower end of the cord; while the back of the 8th vertebral arch was removed, and the other electrode, well moistened with normal saline solution, was introduced at the window so made. Or a pair of platinum electrodes were slipped beneath the abdominal branch of the tenth spinal nerve running to one of the posterior hearts, the heart having previously been brought to rest by division of the same branch near the coccygeal foramen. Under these circumstances the lymph-cardiac muscle-nerve preparation behaves like any ordinary muscle-nerve preparation, except that it possesses a much larger minimal stimulus.

If contact between nerves and electrodes were not very perfect in the spinal canal, or if currents near the minimal stimulating point were employed, the usual firm tetanus of the lymph-hearts became broken into a series of explosive, beat-like, or twitching movements. But, under similar conditions, ordinary muscle-nerve preparations re-act in a similar manner.

c. On the lymph-cardiac spinal centre separated from the encephalic inhibitory centre.—The frog was decapitated, and all the spinal nerves were divided except the tenth. Windows were cut into the back of the spinal canal, opposite the 5th and 8th vertebræ, and into the windows well-moistened non-polarizable electrodes were pushed. Or the spinal canal was opened from the 5th to the 8th vertebræ, and platinum electrodes slipped beneath the cord or made to touch the cord lightly one at each side at the level of the 6th vertebra. Under these circumstances slight currents, strong enough to contract at the same time the ilio-coccygeal muscles, slowed the lymph-hearts; stronger currents inhibited the hearts in diastole, and still stronger currents caused a tetanic systole of the hearts like the simultaneous tetanus induced in neighbouring nerve-supplied muscles. The strong currents sufficient to cause in this manner prolonged diastolic inhibition of lymph-hearts still dependent on their spinal

centres, are also of a strength to cause powerful tetanus of the same lymph-hearts when applied to their nerves in the spinal canal, as described under *b*.

It should be noticed that the same explosive, pulse-like movements occurred here as under *b*, when the conditions were similar.

This action of the interrupted induced current on the lymph-cardiac spinal centres is not abolished by the local or systemic exhibition of solutions of atropia.

II. Action of Constant Currents.

The currents were applied to the structures tested by means of non-polarizable electrodes of ordinary form.

a. On the lymph-hearts brought to rest by separation from their spinal centres.—After operations involving extensive bleeding, separation from their spinal centres is, in nearly all cases, followed by permanent stand-still of the dependent hearts. In the experiments described the frogs were decapitated and eviscerated, all the spinal nerves except the tenth were divided, and the thighs amputated in order to obviate any disturbing muscular movements. The electrodes were applied to the base of the back near the heart to be examined, sometimes in a direction laterally across the heart, and sometimes in the longitudinal axis of the heart; or the heart was carefully excised with its surrounding connective tissue, placed upon the clay of one electrode, and lightly touched with the point of the other. In the latter way of experimenting no attention was paid to the relation of the current to the axes of the heart, on account of the small size of the organ in *R. temporaria*. Under these circumstances with various strengths of battery from 2–3 small Daniell's cells to 16–18 Grove's, nothing but ordinary opening and closing contractions could be caused.

b. On the lymph-hearts still beating in normal dependence on their spinal centres.—The electrodes were placed either on the lateral or the longitudinal axes of the hearts, as in *a*. Under these circumstances, in whatever direction the current flows, the pulsation of the hearts is hampered or inhibited, the hampering affecting rather the depth than the frequency of the beats.

c. On the lymph-cardiac spinal centres.—The well-softened electrodes were pushed in at windows opening into the spinal canal through the 5th and 7th or 8th vertebral arches, after the frog had been decapitated and the spinal nerves, except the tenth, all divided; or one electrode was placed on the upper end of the cord exposed in decapitation, while the other was placed in the window through the lower vertebral arch.

a. On the acting spinal centre.—Different results were obtained according to the direction and strength of the current, varying from complete stand-still in diastole to pulsation much more rapid than normal; but all seem capable of explanation by the aid of the known laws of

electrotonus, the known effect on the neutral line of varying battery power, and the assumption of a stimulating influence of the current *per se*, which may counteract the depressing effects of the anelectrotonic state.

β. On the spinal centre exhausted by the effects of operative procedure, exposure, etc. Restoration to activity follows the passage up or down the cord of a current for 1–2 Grove's cells. Such restoration the author does not think it possible to attribute in every case to the presence of favouring katelectrotonus; it must be set down to a stimulation by the current *per se*.

γ. On the spinal centres of hearts inhibited by stimulating with strong NaCl solutions the upper exposed end of the cord of a decapitated frog. The electrodes were inserted into the spinal canal through windows in the 5th and 7th or 8th vertebral arches. Under these circumstances the hearts cannot be set going by passing a current through the spinal centres, unless, perhaps, when the current is very strong (from 5–6 Grove's); and then only when the anode is *above* the spinal centres, and, therefore, in a position to intercept the descending inhibitory stimulus by means of the anelectrotonic state which it induces.

IV. "On the Structure of the Stylasteridæ, a Family of the Hydroid Stony Corals." By H. N. Moseley, F.R.S., Fellow of Exeter College, Oxford, late Naturalist on board H.M.S. 'Challenger.' Received January 22, 1878.

[For "Preliminary Note," see Proceedings, vol. xxv. p. 93.]

Presents, January 10, 1878.

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Parkes (Dr. E. A.), F.R.S. A Manual of Practical Hygiene. Fifth Edition. Edited by F. S. B. François de Chaumont. 8vo. *London* 1878. The Editor.

Corrected leaf

March 7, 1878.

F. A. ABEL, C.B., Vice-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, as follows :—

Henry James Alderson, Lieut.-Col. R.A.
Thomas Clifford Allbutt, M.A., M.D.
John Attfield, Ph.D., F.C.S.
John Gilbert Baker, F.L.S.
Francis Maitland Balfour, F.L.S.
Prof. Robert Bentley, F.L.S.
Henry Bessemer, Assoc. Inst. C.E.
Rev. Thomas George Bonney, M.A.
George Stewardson Brady, M.D., F.L.S.
John Syer Bristowe, M.D., F.R.C.P.
George Buchanan, M.A., M.D.
Walter Lawry Buller, Sc.D., F.L.S.
Verney Lovett Cameron, Commander R.N., C.B.
William Chimmo, Capt. R.N.
Cuthbert Collingwood, M.A., M.B., F.L.S.
Prof. James Henry Cotterill, M.A.
George Howard Darwin, M.A.
John Dixon, C.E.
James Matthews Duncan, A.M., M.D.
Sir Walter Elliot, K.C.S.I.
Francis Stephen Bennet François de Chaumont, M.D.
William Galloway.
Henry Haversham Godwin-Austen, Major.
Rev. William Greenwell, M.A., F.S.A.
John Caulfield Hennyngton, Major-General.
Thomas Hawksley, M.I.C.E.
John Deakin Heaton, M.D.
John Hopkinson, M.A., D.Sc.
John Hughlings Jackson, M.D., F.R.C.P.
Henry M. Jeffery, M.A.
Lord Lindsay, M.P., P.R.A.S.
Richard Henry Major, Sec. R.G.S.
William Donald Napier, M.R.C.S.
Prof. Henry Alleyne Nicholson, M.D., Ph.D., D.Sc.
Richard Norris, M.D.
Charles Henry Owen, Col. R.A.
Thomas Bevill Peacock, M.D., F.R.C.P.
William Overend Priestley, M.D., F.R.C.P.
Charles Bland Radcliffe, M.D., F.R.C.P.
George Banks Rennie, C.E.
Samuel Roberts, M.A.
George F. Rodwell, F.R.A.S. F.C.S.
George John Romanes, M.A.

Edward A. Schäfer, M.R.C.S.
Arthur Schuster, Ph.D., F.R.A.S.
Michael Scott, M. Inst. C.E.
Prof. Harry Govier Seeley, F.L.S.
Samuel Sharp, F.G.S., F.S.A.
John Spiller, F.C.S.
Hermann Sprengel, Ph.D.
George James Symons, Sec., M.S.
Sir Henry Thompson, F.R.C.S.
Charles S. Tomes, M.A.
Edwin T. Truman, M.R.C.S.
Benjamin Williamson, M.A.
Prof. Edward Percival Wright, M.A., M.D.

The following Papers were read:—

I. "On the Photometry of the Magneto-Electric Light." By Captain W. de W. Abney, R.E., F.R.S. Received February 5, 1878.

At intervals during the last three years it has been my duty to ascertain the value of the illuminating power of different sources of light, which have been brought before Government for military purposes. As there is nothing secret in the results obtained, it seemed to me that I might communicate them to the Royal Society, I have obtained permission so to do.

There are certain difficulties to be overcome which affect the measurements, as it will be evident that the question of colour enters largely into them, which are not so great when lights produced by comparatively low temperatures are in question.

My first attempt to obtain a comparison of the value of any two lights was made by the method of extinction, that is, by introducing between the light and the eye a screen graduated in opacity. After comparing this method with that which I have recently adopted, I am not inclined to quarrel with it. Originally I employed a wedge of green-black glass for the screen, but owing to a complication in the results due to different coefficients of absorption for different parts of the spectrum, I had resort to strips of photographically graduated glass, the necessary black colour being obtained by toning with a platinum salt, and a consequent deposition of platinum black. With a couple of these slips back to back, and with the ends showing the greatest opacity placed together, a graduation was obtained varying between nearly total transparency and an opacity sufficient to cut off the direct rays of the strongest sunlight even when the eye was in its most sensitive condition. These strips being cemented together, and the relative opacity of every division of a small scale having been ascertained, they were caused to pass in front of a narrow slit which was open to the light to be tested. When all trace of light had disappeared readings were taken, and from the series the value of the light calculated. The great defect in this method is the variation in the sensitiveness of the eye, and when employing it, it is necessary to

be excessively cautious to take the readings under precisely similar circumstances, and also to check them from time to time by the extinction of some standard light. Thus I found that the maximum sensitiveness of the eye was practically attained after it had been excluded from the light for two minutes. Readings taken before that interval had elapsed were invariably too high, while afterwards they were fairly equable.

The annexed series shows this result. The readings were commenced after exposure of the eye to an ordinary reading lamp, and continued for more than ten minutes.

3·16 at the commencement.
3·28 at the end of $\frac{1}{2}$ minute's darkness.
3·38 at the end of $1\frac{1}{2}$,, ,,
3·42 at the end of $2\frac{1}{2}$,, ,,
3·48 at the end of 3 ,, ,,
3·45 at the end of 5 ,, ,,
3·48 at the end of 10 ,, ,,

A similar form of instrument was made for Sir Charles Wheatstone without his knowledge of the existence of mine; but I am unaware of the results he obtained.

In the diagrams which accompany this paper I have given the curve of intensity of the magneto-electric light generated by a machine, similar to that to which reference will subsequently be made, and it will be noticed how close is the agreement between it and the curve which indicates the intensity of the blue light.

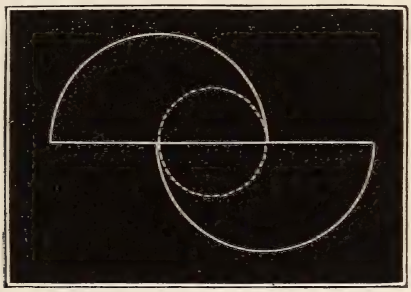
Later it was my endeavour to compare the values of the different coloured components radiated from any source of light, and my original apparatus consisted of a double spectroscopic arrangement by which the lights from the two sources entered by different slits into the spectroscope, and to the eye the spectra appeared vertically one over the other. After screening off all of the two spectra except a narrow strip, and by applying a telescope with a divided lens, as will shortly be described, the intensities of the two strips were equalised by widening or narrowing one or other of the slits. The ratio of the widths of the slits gave approximately the values of the different intensities of the light in the particular part of the spectrum. For light giving a continuous spectrum this method answered admirably, and I believe it was on the same principle that Lord Rayleigh measured the relative values of sky and sunlight; but for lights whose spectra were discontinuous the method failed owing to the labour it involved. This method was then abandoned, and another was sought which should integrate two or three parts of the visible spectrum.

In my hands Bunsen's method of photometry has always been uncertain, perhaps through some personal obliquity, though I believe that

with other persons the same uncertainty has been observed. Rumford's method has never been a favourite, but I believe that if the following modification of it be adopted, there can never be much difficulty in comparing lights of similar colours. The problem which I set myself was to obtain shadows of equal size, and to place them side by side as seen by the eye. The following plan accomplishes this:—

In an opaque screen of some half metre square two equal slits of about half a centimetre in width and 7·5 centimetres in length were cut parallel to one another, the lengths being vertical. These slits were separated by 7·2 centimetres, and were covered with a diaphanous screen, white tissue paper being apparently the best substance to employ, though ground glass was also tried. The tissue paper was placed at a small distance from the opaque screen, so that the texture was invisible when the borders of the slit were sharp when viewed by an observing telescope. A rod of a centimetre in diameter was placed in front of the tissue screen, and so arranged that the shadows thrown by the two sources of light entirely covered the slits. The movable light was caused to travel along a scale fixed at such an angle as to ensure this always occurring. The necessary precautions were taken to ensure that the shadows falling on the screen were at equal distances from the centre of the rod.

In order to cause the two shadows thus thrown to approach each other, a divided lens of about 16 centimetres focus was made to form the objective of a telescope, and by carefully separating the optical centres of the two halves (see figure) when the line of division was



kept in a horizontal position, the images of the two slits could be caused to approach one another, and could be viewed in the same field by an eyepiece. By turning the telescope in its Y's the same result could be obtained, though in this case the image seen of the one slit would be formed by the opposite half of the lens to that which formed it in the first observation. Measurements were taken with standard lights, till the adjustment was found which gave equal illumination of both sides of the lens. When the opaque screen, the telescope, and the head of the observer were carefully shielded from all extraneous light, the

observations are singularly accordant. The following observations made with a standard candle and a paraffin lamp, the light traversing a red medium, will give an idea of the correctness of the results :—

Readings of the Lamp in Candle Power.

	10·25
	10·23
	10·18
	10·23
	10·26
	10·23
Mean	10·23

If there be any drawback to this method, it is that an assistant is required to alter the distances of the movable light.

I may also remark that there are shades of light between which it is sometimes difficult to judge. If too bright the requisite diminution can be obtained by using diaphragms in the telescope, and by altering the magnifying power of the eye-piece. If too dark, an addition to the light which reaches the slits may be given by giving them an equal amount of illumination from an independent source.

To secure the separation of the different portions of the spectrum, resort was had to absorbing media. After many trials I came to the conclusion that nothing seemed preferable to red glass, which absorbed practically every part of the spectrum except the red end, and an ammoniacal solution of copper sulphate. These two absorbing media were employed in the experiments which I am about to describe; very little of spectrum is left unaccounted for, and the results obtained are sufficiently striking to record.

A red glass was obtained which had sufficiently plane surfaces, and a cell with parallel sides was used for holding the copper solution. The absorbing media were placed between the rod and the opaque screen, and absolutely parallel with the latter.

The following is an outline of the experiments to which the attention is to be drawn. The primary object was to ascertain the relation between the horse-power expended, the light produced, and the number of revolutions of the armature. The machine employed was of the Gramme form, and weighed somewhere about 17 cwt. It had three pairs of vertical coils, one of which was used for magnetization. To work it, what is known as the steam sapper was employed. This is a road locomotive from the shops of Aveling and Porter, of Rochester, and is adapted to driving machinery by applying a band to the fly-wheel. It works fairly steadily when not overtaxed. An indicator was attached for taking diagrams from which to calculate the horse-power expended. The number of revolutions of the armature was

taken by a velocimeter, and a large tangent galvanometer was inserted in the circuit, the diameter of the arc being 33 times that of the needle. An automatically working electric lamp having perfectly dry square carbons of $\frac{1}{2}$ inch side was the source of light, and was placed at fixed distances of 75, 150, or 50 feet from the measuring apparatus as circumstances required. It gave a steady light when the engine worked regularly, and the brilliancy of the light was not increased by altering the distance apart of the carbons. When a series of readings was to be taken, the horse-power expended in driving the machine with the collecting brushes up was found, and then the brushes turned down, and the light allowed to burn for ten minutes before any measurement was taken. The number of revolutions of the armature was then approximately ascertained, and a diagram taken when the velocimeter was applied to the machine. The readings of the galvanometer were recorded by an assistant working under my colleague, Captain Armstrong, R.E., and then measurements of the illuminating power taken by myself and checked by an intelligent assistant. The light transmitted through the red medium was first measured, and then through the blue. If the first and last of the readings compared favourably with each other, the series was taken as worthy of confidence. If, on the other hand, great fluctuations were observed, which was sometimes the case owing to the occasional irregularities of the motive power, and to impurities in the carbons themselves, a fresh set of readings was taken. As often as possible readings were taken through the blue and red media at the same number of revolutions per minute of the armature. This was not always practicable, or even necessary. This will explain the reason of the variable revolutions recorded in the tables. I may remark that the revolutions were always taken at the commencement and end of each set of readings, and if these were tolerably close the mean was taken as the correct value for the mean of the readings. With any great variation, say of more than ten revolutions, the readings were rejected altogether. It was found difficult to make a comparison with a standard or pair of standard candles, owing to the small distance from the screen they sometimes occupied; hence an intermediate light from a paraffin lamp was made use of, which before and after each series of readings was compared with the standard candle. The variation of the lamp light was very small, ranging from 10·23 to 10·05. The light from the electric lamp was admitted into the observing-room through an aperture in the wall of about 18 inches square, and great care was taken that any reflected light was cut off the screen.

In addition to the optical readings, an endeavour was made to secure a record of the actinic value of light. Paper sensitized with silver chloride was exposed to the light at a distance of three feet from the carbon points, one strip was placed behind a cell filled with

quinine sulphate, whilst another was exposed to the full action of the unshaded light. The times of exposure varied between one and ten minutes. The intensity of the actinism was measured by Roscoe's method, which need not be described. The curve obtained from the paper exposed behind the quinine cell is practically identical with that obtained optically from the blue components when any one point in the former curve is made to correspond to a point in the latter which has the same abscissa. The following Table gives the results of the experiments made with this particular machine. The numbers or letters in the first column refer to the numbers in the diagram. The horse-power given is the total horse-power recorded less that required to drive the machine with the brushes up.

Expenditure of H.P.

Table I refers to Diagram I.

No.	Revolutions.	H.P.
a.....	240	1·0
b.....	308	1·6
c.....	350	2·5
d.....	400	3·8
e.....	460	5·6
f.....	480	5·7
g.....	520	7·9
h.....	550	8·5
k.....	565	9·0

Integration of Blue Light.

Table II refers to Diagram II.

No.	Revolutions.	Candles.
1....	240	360
2....	308	660
3....	350	750
4....	425	1,700
5....	460	2,500
6....	490	3,000
7....	520	4,860
8....	550	4,800
9....	565	6,500
10....	580	6,000
11....	600 ?*	10,100

Integration of Red Light.

Table III refers to Diagram II.

No.	Revolutions.	Candles.
12....	240	180
13....	308	280
14....	460	860
15....	500	1,080
16....	520	1,300
17....	540	1,620
18....	575	1,520
19....	580	2,100
20....	600 ?	2,400

Integration of Actinic Power.

Table IV refers to Diagram II.

No.	Revolutions.	Candles.
21....	350	890
22....	460	2,750
23....	560	9,000
24....	580	10,050
25....	600 ?	11,020

* A ? is placed against the 600 revolutions, as the engine at that velocity worked somewhat unequally.

Currents. Machine No. I.

Table V refers to Diagram IV.

No.	Revolutions.	Tangents.	Remarks.
26.....	300	1·15	1st set of experiments.
27.....	350	1·31	
28.....	460	1·80	
29.....	540	2·47	
30.....	240	·51	Experiments during which the illuminating power was measured. Tables II, III, and IV.
31.....	308	·90	
32.....	370	1·33	
33.....	425	1·48	
34.....	460	1·66	
35.....	500	1·96	
36.....	580	2·20	
37.....	600 ?	2·30	

Currents. Machine No. II.

Table VI refers to Diagram V.

No.	Revolutions.	Tangents.	Remarks.
38.....	398	1·40	Curve B, fig. V. 1st set of experiments.
39.....	490	1·88	
40.....	846	3·08	
41.....	264	·10	2nd set of experiments. Curve A, fig. VI.
42.....	280	·12	
43.....	288	·17	
44.....	384	·38	
45.....	438	·48	
46	458	·53	
47.....	484	·56	
48.....	534	·67	
49.....	692	·92	
50.....	842	1·25	
51.....	862	1·33	
52.....	1,014	1·45	

In Diagram II the stars show the value of the light [from the method of extinction.

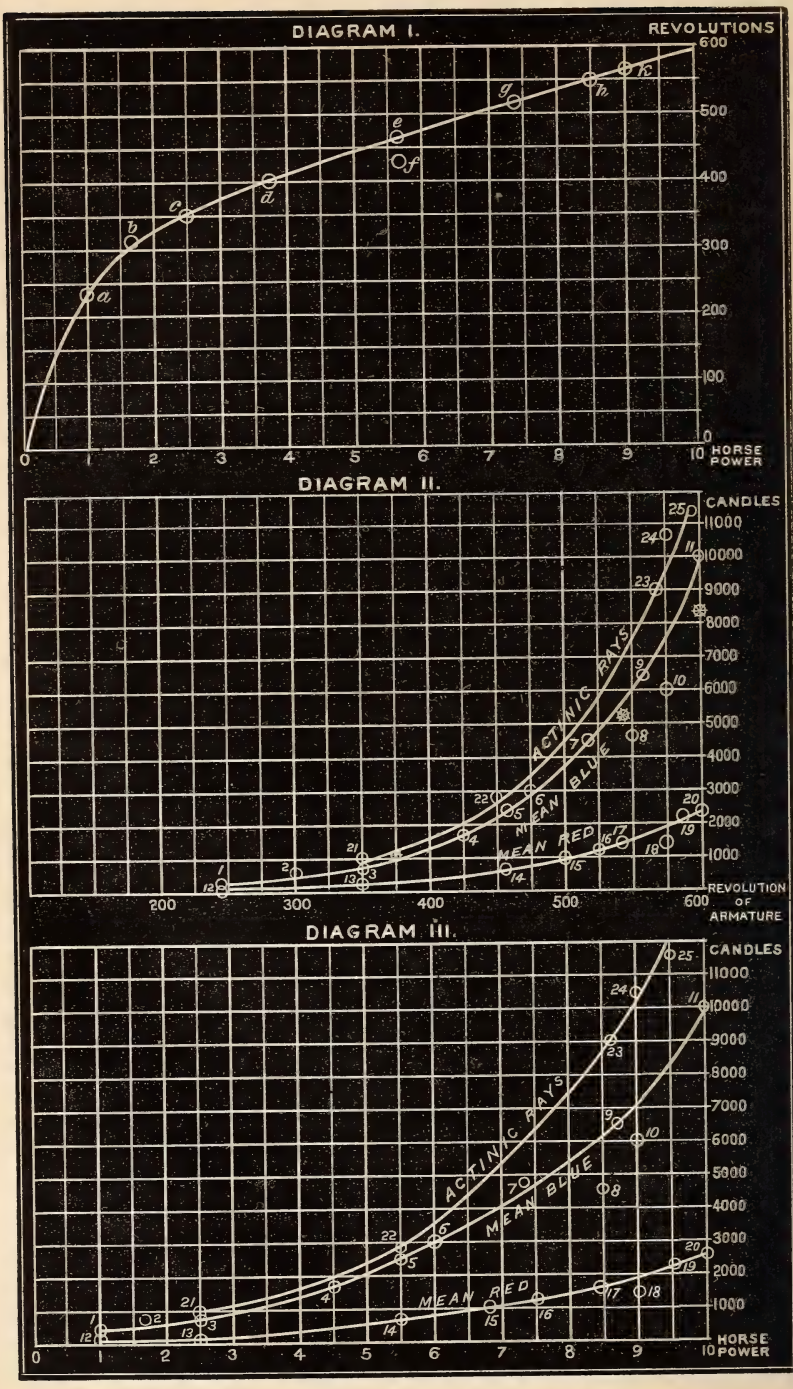
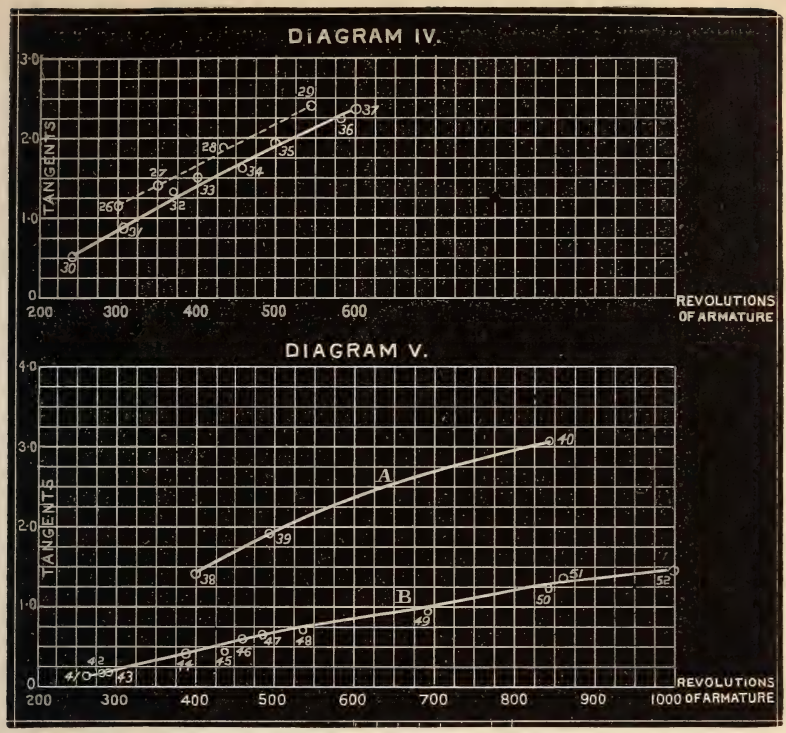


DIAGRAM I.
REVOLUTIONS
600
500
400
300
200
100
0
a
b
c
d
e
f
g
h
k
0 1 2 3 4 5 6 7 8 9 10
HORSE POWER
DIAGRAM II.
CANDLES
11000
10000
9000
8000
7000
6000
5000
4000
3000
2000
1000
ACTINIC RAYS
MEAN BLUE
MEAN RED
200 300 400 500 600
REVOLUTION OF ARMATURE
DIAGRAM III.
CANDLES
11000
10000
9000
8000
7000
6000
5000
4000
3000
2000
1000
ACTINIC RAYS
MEAN BLUE
MEAN RED
0 1 2 3 4 5 6 7 8 9 10
HORSE POWER



The following is a statement, furnished me by Captain R. Y. Armstrong, R.E., of the electrical conditions of the circuit.

The resistance of the stationary wire = 4·46 ohms.
„ „ magnetizing coil = 0·44 ohm.

In the generator the resistance of the light-circuit coil = 0·24 ohm.

The light coils were in divided circuit, as were also the right and left stationary coils.

When driven at from 375 to 383 revolutions, the resistance of the voltaic arc was about 0·18 ohm; the other resistances in the circuit were 0·7 ohm. The electromotive force of the machine when driven at the above speed was 111 volts, a measure which has been arrived at by Captain Armstrong from other experiments. The electromotive force between 375 and 383 revolutions of the armature was assumed to vary as the number of revolutions, an assumption which was borne out as practically correct by other measurements made for the purpose. In order to ascertain what effect the insertion of resistance had on the current, the following experiment was undertaken by Captain Armstrong with a smaller form of gramme machine, called Machine No. II.

Two resistances were inserted in the circuit for two different sets of readings, in the first case 1·1 ohm of total resistance, in the other 2·72.

Table VI refers to these experiments, the curve marked A in the Diagram V being that due to the first, B being that due to the second. It will be noticed that practically the curve becomes a straight line, or, in other words, the electromotive force increases directly as the number of revolutions of the armature, a result which might have been expected theoretically. It will also be seen that the current for any given number of revolutions varies inversely as the resistance in circuit, or, in other words, that the electromotive force for a given number of revolutions is constant.

In any results, therefore, which are given descriptive of the light produced by any machine, the following should be noted :—

Number of revolutions of armature;
Resistance in circuit;
Horse-power expended;
Colour of light measured;
Electromotive forces.

together with the size of the carbons employed, and other obvious details.

II. "Experimental Researches on the Temperature of the Head." By J. S. Lombard, M.D., formerly Assistant-Professor of Physiology in Harvard University, U.S. Communicated by H. Charlton Bastian, M.D., F.R.S., Professor of Pathological Anatomy in University College, London.

(Abstract.)

The present communication forms an abstract of the first portion of a series of investigations having the following primary objects in view :—

1st. To find out, as far as possible, the normal relative temperatures of different portions of the surface of the head, when the brain is comparatively inactive.

2nd. To study the effect of different mental states upon the different portions of the surface of the head previously examined in the condition of comparative cerebral inactivity.

The *ultimate* objects were two-fold; namely :—

1st. To furnish, if possible, some reliable data as a starting point,

for examining the temperature of the surface of the head, with a view to assisting in the diagnosis and localization of cerebral disease.

2nd. To see if, from an examination of the relative temperatures of different portions of the surface of the head during increased mental activity, any information could be obtained as to the comparative importance of the parts played by different portions of the brain in the evolution of thought and the different emotions.

To commence with the first of these ultimate objects of the investigations, it is easy to see that everything depends upon an accurate knowledge of the normal *relative temperatures* of the different portions of the surface of the head, and the variations of such temperatures within healthy limits. Without this knowledge it is impossible to come to any satisfactory conclusion as to the existence of localized cerebral disorder by examining the temperature of the surface of the head.

So far as the writer is aware, Dr. Wm. A. Hammond,* of New York, was the first to indicate that a difference of temperature exists between the two sides of the head in health. In 1875, Dr. Hammond, making use of a thermo-electric apparatus, devised by the writer the year previous, came to the conclusion, from observations made on a large number of individuals, that the left side of the head has a higher temperature than the right side. Unfortunately the notes furnished by Dr. Hammond, in the writer's possession, do not state the *exact locality* examined, a matter of great importance as will be seen further on.

During the past year M. Broca† has brought forward experiments on the same subject. M. Broca, using thermometers, has also come to the conclusion that the left side of the head has the higher temperature. The sources of error in M. Broca's method will be presently noticed.

Turning now to the *second* of the ultimate objects of the present investigations, namely, the connexion between the relative share of mental work done in a given part of the brain, and the relative temperature of the surface over such part, it is evident that here also a thorough acquaintance with the relative temperatures of the different portions of the surface of the head in the quiescent mental condition is requisite.

As regards what has been already done on the subject of the relation between heat and mental work, it may be summed up in a few words.

In 1866 the writer commenced a series of experiments with thermo-electric apparatus on the effect of increased mental activity on the temperature of the head. These experiments showed that the exercise

* Dr. Hammond's paper was read before the New York Neurological Society, October 4, 1875.

† "Revue Scientifique," September 15, 1877.

of the higher intellectual faculties, as well as different emotions, caused a perceptible rise of temperature in the head. Merely arousing the attention could produce the same result. These results were published in June of 1867.* Toward the close of the latter year, Professor Moritz Schiff, who had been working independently of any knowledge of what the writer had been doing, communicated to the Museum of Natural History of Florence results of a similar nature. In 1870 Professor Schiff † published an account of a series of investigations made directly upon the brain of animals, which decisively proved that mental work is accompanied by elevation of temperature in the brain. Lastly, M. Broca has likewise arrived at the same conclusion by experiments made, like the writer's, on the human subject, M. Broca, however, using thermometers instead of thermo-electric apparatus.

The present investigations were commenced in January, 1877, and have been continued almost without interruption to the present time. The first step to be taken having been decided upon, namely, a thorough examination of the normal relative temperatures of the different portions of the surface of head, the next point to be decided was the manner in which the examination could be best carried on. Preliminary observations had satisfied the writer that experiments made upon the heads of individuals taken at random could only lead to confusing and contradictory results. Accordingly, the investigations were limited to a few selected heads, which could be measured and compared, and the different circumstances, both internal and external, affecting which, could be pretty well known. Six subjects, three males and three females, were selected. The next question was the measurement of the head, and its division into regions, and the subdivision of these regions. The following method was decided on: the head was divided into three regions, designated respectively, *anterior*, *middle*, and *posterior*.

Anterior Region.

The anterior region is bounded laterally by a line drawn upward, on each side of the said, from the angle formed by the frontal and zygomatic processes of the malar bone in a direction parallel to the plane of the forehead, taken over the frontal eminences and superciliary ridges.

The superior boundary is formed by the continuation in the same plane, and junction on the top of the head, of the lateral boundaries.

The inferior boundary is formed by a line passing horizontally across the front of the head on a level with the summits of the supra-orbital arches between the external angular processes of the two sides, and

* "New York Medical Journal," June, 1867, and "Archives de Physiologie," September—October, 1868.

† "Archives de Physiologie," t. iii, p. 6, 1870.

thence continued by the outer borders of the malar bones to the points of origin of the lateral boundaries.

The part of the head embraced by the anterior region is, therefore, that which would be cut off anteriorly by a transverse and vertical section, made parallel to the plane specified, between the angles formed by the frontal and zygomatic processes of the malar bones of the two sides.

Middle Region.

The middle region is bounded anteriorly by the lateral and superior boundaries of the anterior region.

The posterior boundary is formed by a line passing over the top of the head parallel to the anterior boundary, uniting the extremities of the mastoid processes of the two sides.

The inferior boundaries commencing at the terminations of the inferior boundary of the anterior region, follow the upper borders of the zygomatic processes of the temporal bones, pass behind the ears, and follow the anterior borders of the mastoid processes to their extremities.

Posterior Region.

The posterior region has for its superior and lateral boundaries the posterior boundary of the middle region.

The inferior boundary is formed by the posterior borders of the mastoid processes and by the superior curved line of the occipital bone.

The longitudinal median line of the head divides each of the three regions into right and left symmetrical halves.

In the present communication we have only to do with the anterior region.

The anterior region is thus subdivided:—

Commencing at the inferior boundary, each lateral half is divided into six parts by five equidistant horizontal lines drawn from the median line to the lateral limit. The tracts thus marked off are designated *tiers*, and are numbered from 1 to 6, commencing at the inferior boundary. Further in each lateral half four equidistant vertical lines are drawn upward parallel to the median line, from the lower limit to the superior boundary, thus dividing each *tier* into smaller spaces. In the first four tiers these spaces are five in number; but in the fifth and sixth tiers, the convergence, over the top of the head, of the lateral limits diminishes the length of the tier, and reduces the number of spaces to four in the fifth tier and to three in the sixth tier. These spaces are designated *districts*, and are numbered from 1 to 5 outward from the median line. The tiers are marked off by means of a string coated with coloured chalk.

As the method of measuring off the tiers and districts by equidistant

lines does not in every head bring a given space into exactly the same anatomical position, each case is referred to one head as a standard. The following are the measurements and anatomical positions of the tiers in the standard head.

The height of the region measured on the median line is 125 mm. (4·92 inches), therefore, each of the six tiers measures 20·83 mm. (0·82 inch) vertically. The upper boundary of the first tier touches the summits of the superciliary ridges. The upper boundary of the second tier passes through the centre of the frontal eminences. The upper boundary of the third tier touches the upper border of the frontal eminences. The upper boundaries of the fourth and fifth tiers have no anatomical landmarks, and their positions can be designated only by their respective distances from the superior limit of the region; this latter touches the coronal suture on the median line, hence the upper boundaries of the fourth and fifth tiers are, respectively, 41·66 mm. (1·64 inch), and 20·83 (0·82 inch) distant from the coronal suture on the median line.

The following are the measurements of breadth:—

Total breadth measured on the horizontal portion of the inferior boundary of the region 186 mm. (7·32 inches).

Total breadth measured over middle of frontal eminences 186 mm. (7·32 inches).

Total breadth measured just above frontal eminences 176 mm. (6·928 inches).

Total breadth measured at a distance of 20·83 mm. (0·82 inch) from the coronal suture on the median line 120 mm. (4·72 inches).

The breadth of each lateral half measured on the inferior boundary line being 93 mm. (3·66 inches), each of the five districts will measure on this line 18·6 mm (0·732 inches). We have then 27 spaces a side to examine, the maximum size of the space being 21 mm. (0·8 inch) by 19 mm. (0·7 inch).

With reference to the instruments employed, both thermometers and thermo-electric apparatus have been used. The writer does not, however, think thermometers reliable in investigations of this kind; for the reason that they cannot be pushed down firmly enough upon the surface to empty the superficial vessels. The piles used by the writer are set in paraffine in an ebonite casing so as to be at some distance from the edges of the casing, and the whole bottom,—faces of piles, paraffine, and ebonite edges,—rendered perfectly flush. Pressure with a pile so constructed empties the superficial vessels, leaving the part pale and bloodless on the removal of the pile. It is easy with such piles to empty the temporal artery and to test the temperature of parts lying beneath it without the least risk of the temperature of the blood in the artery itself interfering. The danger of error in experiments such as those of M. Broca, is that the tempera-

ture of the blood of the superficial vessels comes to affect the thermometer, as well as the temperature of the deeper seated parts.

Sir W. Thomson's galvanometer, and one devised by the writer himself, already described several years ago, were employed.* The rheostat and keys have also been described in 1868.†

We will now proceed to examine the different spaces of the anterior region.

1*st. Comparison of symmetrically situated spaces of the two sides of the head,* 100 *observations on each pair of spaces.*

The first fact of importance demonstrated by this examination is, that in *no one of the spaces into which the anterior region is subdivided is the temperature uniformly higher on one side than on the other: on the contrary, it may be higher on the right side or on the left side in turn.* We have, therefore, to consider only on which side of the head in the *majority of cases* the higher temperature is found in a given pair of spaces. The following is the general distribution of temperature:—

	In favour of	
	Left side.	*Right side.*
	1*st Tier.*	1*st Tier.*
Districts—	1st, 2nd, 3rd.	4th, 5th.
	2*nd Tier.*	2*nd Tier.*
,,	1st, 2nd, 3rd.	4th, 5th.
	3*rd Tier.*	3*rd Tier.*
,,	1st, 2nd, 3rd.	4th, 5th.
		4*th Tier.*
,,		1st, 2nd, 3rd, 4th, 5th.
		5*th Tier.*
,,		1st, 2nd, 3rd, 4th.
		6*th Tier.*
,,		1st, 2nd, 3rd.

Thus in the 27 spaces a side compared, the average relative temperature is higher on the right side than on the left in 18 spaces, or two-thirds of the whole number.

Next, taking the total number of observations, 2700, and deducting

* "British Medical Journal," January 23, 1875.

† "Archives de Physiologie," July—August, 1868.

220 cases of equality of temperature, to be presently considered, the following is the apportionment of the remaining 2480 results :—

In favour of right side	1343
„ „ left „	1137

Hence the percentages of times of occurrence of relative superiority of temperature for the right and left sides respectively are 54·153 and 45·847. But in the nine spaces in which the left side has the majority of cases of higher temperatare, this majority is greater than the majority found in the eighteen spaces in which the right side has the higher average : thus, in the nine spaces specified, the left side shows a mean percentage of 75·069 cases of superiority of temperature ; while in the eighteen spaces in which the right side predominates the mean percentage of cases of superiority of temperature is 68·117 ; that is to say, those spaces which, on an average, are higher in temperature on the left side are more exclusively so than those which, on an average, are higher in temperature on the right side.

Next, as regards equality of temperature of the two sides, the following are the spaces in which equality of temperature is most frequently found :—

	1st Tier.
Districts—	1st, 2nd, 3rd.
	2nd Tier.
„	3rd, 4th.
	3rd Tier.
„	1st, 2nd, 3rd, 4th.
	4th Tier.
„	1st, 2nd, 3rd, 4th.
	5th Tier.
„	1st, 2nd, 3rd, 4th.

Thus equality of temperature of the two sides is found in 16 spaces, or in 59·259 per cent. of the whole number of spaces. Of the total number of observations, 2,700, 220, or 8·148 per cent. show equality of temperature. Taking the total number of observations we have the following percentages of times of occurrence of superiority of temperature on the right side, on the left side, and of equality of temperature :

Right side	50·112 per cent.
Left „	41·740 „ „
Equality	8·148 „ „

The five highest percentages in favour of the right side are distributed as follows:

5th	District of	2nd	Tier ..	— 76 per cent.
5th	„ „	3rd	„ ..	— 70 „ „
2nd	„ „	5th	„ ..	— 70 „ „
4th	„ „	5th	„ ..	— 69 „ „
2nd	„ „	6th	„ ..	— 71 „ „

The five highest percentages in favour of the left side are distributed as follows:

1st	District of	1st	Tier ..	— 74 per cent.
2nd	„ „	1st	„ ..	— 77 „ „
1st	„ „	2nd	„ ..	— 74 „ „
2nd	„ „	2nd	„ ..	— 76 „ „
2rd	„ „	3rd	„ ..	— 75 „ „

The five highest percentages of equality of temperature are distributed as follows:

4th	District of	2nd	Tier ..	— 22 per cent.
1st	„ „	3rd	„ ..	— 22 „ „
2nd	„ „	3rd	„	— 24 „ „
2nd	„ „	4th	„ ..	— 24 „ „
3rd	„ „	4th	„ ..	— 30 „ „

The following shows the distribution of temperature in two cases of common occurrence, the one with increase in extent of the tract of superior temperature on the right side, and the other with increase in extent of the tract of superior temperature on the left side. These deviations from the general order of things often persist for some hours.

Increase of tract of Right Superiority of Temperature.

In favour of

Left side.	Right side.
Districts.	*Districts.*
1st Tier—1st, 2nd,	3rd, 4th, 5th.
2nd Tier—1st, 2nd,	3rd, 4th, 5th.
3rd Tier—1st, 2nd, and part of 3rd,	part of 3rd; 4th, 5th.
4th Tier—	1st, 2nd, 3rd, 4th, 5th.
5th Tier—	1st, 2nd, 3rd, 4th, 5th.
6th Tier—	1st, 2nd, 3rd, 4th, 5th.

Increase of tract of Left Superiority of Temperature.

	In favour of
Left side.	Right side.
Districts.	*Districts.*
1st Tier—1st, 2nd, 3rd, 4th,	5th.
2nd Tier—1st, 2nd, 3rd, 4th,	5th.
3rd Tier—1st, 2nd, 3rd, 4th,	5th.
4th Tier—1st, 2nd, 3rd, 4th,	5th.
5th Tier—parts of 1st, 2nd, 3rd,	parts of 1st, 2nd, 3rd; 4th, 5th.
6th Tier—	1st, 2nd, 3rd, 4th, 5th.

Before leaving this part of the subject it may be well to state that the situation of M. Broca's "frontal" thermometer seems to have been in the neighbourhood of the 5th district, 1st tier, so far as the writer can judge from the abstract of M. Broca's paper, which has come under his (the writer's) notice. Now, for this space, the writer's results are, 68 in favour of the right side, and 32 in favour of the left side; but immediately adjoining is the 1st district 1st tier of the writer's *middle region*, and, in the space specified, the figures are 30 right, 60 left, and 10 neutral. M. Broca's thermometer may have been in this space. The examination of the *middle region* does not enter into this communication, and the above remarks are made principally to show within what narrow limits the balance of superiority of temperature may shift from one side to another.

Quantitative comparisons of the two sides.

The following is a summary of the mean results of 100 examinations of each pair of symmetrically situated spaces.

The mean difference of temperature is pretty nearly the same for both sides of the head; thus, the mean difference of temperature for the eighteen spaces which are, on an average, of higher temperature on the right side than on the left, is 0·255° C. (0·459° F.); while the mean difference of temperature for the nine spaces which are of higher temperature on the left side than on the right, is 0·2411° C. (0·433° F.) The greatest difference noted is in the 3rd district, 3rd tier, left side, namely, 0·461° C. (0·829° F.); the smallest differences noted are in the 1st district, 4th and 5th tiers, and in the 2nd district, 4th tier, all right side, the differences being each 0·076° C. (0·136° F.). The extreme range of difference of temperature is therefore 0·385° C. (0·693° F.). The mean difference of temperature of all the observations taken together, irrespective of sides, is 0·247° C. (0·444° F.).

2nd.—Comparison of spaces situated on one and the same side of the Head.

(*a.*) Comparison of spaces situated in the same district of two adjoining tiers—50 observations on each pair of spaces.

The following is a summary of the principal results;

1st. The whole of the 2nd tier is, in the majority of cases, of higher temperature than the 1st tier, on both sides of the head; with the exception, that on both sides, the 1st district is generally of higher temperature in the 1st tier than in the 2nd tier.

2nd. The whole of the 2nd tier is, in the majority of cases, of higher temperature than the 3rd tier, on both sides of the head; but this majority is much less than that possessed by the 2nd tier over the 1st tier.

3rd. The whole of the third tier is, in the majority of cases, of higher temperature than the 4th tier, on both sides of the head.

4th. The whole of the 4th tier is, in the majority of cases, of higher temperature than the 5th tier, on both sides of the head.

5th. The whole of the 6th tier is, in the majority of cases, of higher temperature than the 5th tier, on both sides of the head.

The mean quantitative results are as follows:

	Left side.	*Right side.*
1st district, 1st tier, superior in temperature to 1st district, 2nd tier, by	0·03 C. (0·054° F.)	0·04° C. (0·072° F).
Remainder of 2nd tier, superior to 1st tier, by	0·155° C. (0·279° F.)	0·165° C. (0·297° F.)
2nd tier, superior to 3rd tier, by	0·08° C. (0·144° F.)	0·068° C. (0·122° F.)
3rd tier, superior to 4th tier, by	0·296° C. (0·532° F.)	0·324° C. (0·583° F.)
4th tier, superior to 5th tier, by	0·312° C. (0·561° F)	0·302° C. (0·543° F.)
6th tier, superior to 5th tier, by	0·26° C. (0·468° F)	0·273° C. (0·491° F.)

It will be seen from the above figures, that the difference between spaces of adjoining tiers on one and the same side of the head is nearly as great as the difference between symmetrically situated spaces of the two sides. The order in which the tiers come, as regards their temperatures, is as follows:

2nd tier—3rd tier—1st tier—4th tier—6th tier—5th tier; this order holding good for both sides of the head.

(*b.*) Comparison of spaces situated in two adjoining districts of the same tier, 50 observations on each pair of spaces.

The following is a summary of the principal results:—

1st. The 1st district is of higher temperature than the 2nd district, in the majority of cases, in the 1st and 5th tiers, on both sides of the head.

2nd. The 2nd district is of higher temperature than the 1st, in the majority of cases, in the 2nd, 3rd, 4th, and 6th tiers, on both sides of the head.

3rd. The 3rd district is of higher temperature than the 2nd district, in the majority of cases, in every tier, on both sides of the head.

4th. The 4th district is of higher temperature than the 3rd district, in the majority of cases, in every tier, on both sides of the head.

5th. The 4th district is of higher temperature than the 5th district, in the majority of cases, in the 1st tier, left side; in the 2nd and 3rd tiers, right side; and in the 4th tier, on both sides of the head.

6th. The 5th district is of higher temperature than the 4th district, in the majority of cases, in the 1st tier, right side; and in the 2nd and 3rd tiers, left side.

The mean quantitative results are as follows:—

Left side.	*Right side.*
1st district superior in temperature to 2nd district by 0·042° C. (0·075° F.)	−0·037° C. (0·066° F.)
3rd district superior to 2nd district by 0·053° C. (0·095° F.)	−0·043° C. (0·077° F.)
4th district superior to 3rd district by 0·114° C. (0·205° F.)	−0·032° C. (0·057° F.)
4th district superior to 5th district by 0·02° C. (0·036° F.)	−0·012° C. (0·021° F.)

From the above it can be seen that the mean difference between districts is much less than that existing between tiers. The order in which the districts come, as regards their temperatures, is as follows:—

4th district, 5th district, 3rd district, 1st district, 2nd district; this order holding good for both sides of the head.

Lastly, if we take the three groups of examinations, namely, those on the comparative temperature of the two sides of the head: those on the comparative temperature of adjoining tiers on one and the same side; and those on the comparative temperature of adjoining districts on one and the same side, we have the following values:—

Comparison of the two sides of the Head.

Average percentage of times of occurrence of superiority of temperature of either side of the head over the other.....	61·074	Average difference of temperature — 0·247° C. (0·444° F.)

Comparison of adjoining tiers of one and the same side.

Average percentage of times of occurrence of superiority of temperature of one tier over another on both sides taken together	65·506	Average difference of temperature — 0·21° C. (0·378° F.)

Comparison of adjoining districts of one and the same side.

Average percentage of times of occurrence of superiority of temperature of one district over another on both sides taken together	Average difference of temperature 65·219 — 0·055° C. (0·099° F.)

According to the above figures, superiority of temperature of a given side of the head over the other, is a little less frequent of occurrence than superiority of temperature of a given tier or district of one and the same side over another tier or district. The two sides of the head are rarely equal in temperature, but the balance of superiority shifts so frequently from one side to the other, that the percentage of superiority for either side, in a given number of observations, is comparatively small.

III. "Addition to Memoir on the Transformation of Elliptic Functions." By A. CAYLEY, F.R.S., Sadlerian Professor of Pure Mathematics in the University of Cambridge. Received February 6, 1878.

(Abstract.)

I have recently succeeded in completing a theory considered in my "Memoir on the Transformation of Elliptic Functions," Phil. Trans., t. 164 (1874), pp. 397–456, that of the septic transformation, $n=7$. We have here

$$\frac{1-y}{1+y}=\frac{1-x}{1+x}\left(\frac{a-\beta x+\gamma x^2-\delta x^3}{a+\beta x^2+\gamma x^2+\delta x^3}\right)^2,$$

a solution of

$$\frac{\mathrm{M}dy}{\sqrt{1-y^2\,.\,1-v^8y^2}}=\frac{dx}{\sqrt{1-x^2\,.\,1-u^8x^2}},$$

where $\frac{1}{\mathrm{M}}=1+\frac{2\beta}{a}$; and the ratios $a:\beta:\gamma:\delta$, and the uv-modular equation are determined by the equations

$$u^{14}a^2=v^2\delta^2,$$
$$u^6(2a\gamma+2a\beta+\beta^2)=v^2(\gamma^2+2\gamma\delta+2\beta\delta),$$
$$\gamma^2+2\beta\gamma+2a\delta+2\beta\delta=v^2u^2(2a\gamma+2\beta\gamma+2a\delta+\beta^2),$$
$$\delta^2+2\gamma\delta=v^2u^{10}(a^2+2a\beta)\ ;$$

or what is the same thing, writing $a=1$, the first equation may be replaced by $\delta=\frac{u^7}{v}$, and then, a, δ having these values, the last three

equations determine β, γ and the modular equation. If instead of β we introduce M, by means of the relation $\frac{1}{M}=1+2\beta$, that is $2\beta=\frac{1}{M}-1$, then the last equation gives $2\gamma=u^3v^3\left(\frac{1}{M}-\frac{u^4}{v^4}\right)$; and a, β, γ, δ having these values, we have the residual two equations

$$u^6(2a\gamma+2a\beta+\beta^2)= v^2(\gamma^2+2\gamma\delta+2\beta\delta),$$
$$\gamma^2+2\beta\gamma+2a\delta+2\beta\delta=v^2u^2(2a\gamma+2\beta\gamma+2a\delta+\beta^2),$$

viz., each of these is a quadric equation in $\frac{1}{M}$; hence eliminating $\frac{1}{M}$, we have the modular equation; and also (linearly) the value of $\frac{1}{M}$ and thence the values of a, β, γ, δ in terms of u, v.

Before going further it is proper to remark that, writing as above $a=1$, then if $\delta=\beta\gamma$, we have

$$1-\beta x+\gamma x^2-\delta x^3=(1-\beta x)(1+\gamma x^2),$$
$$1+\beta x+\gamma x^2+\delta x^3=(1+\beta x)(1+\gamma x^2),$$

and the equation of the transformation becomes

$$\frac{1-y}{1+y}=\frac{1-x}{1+x}\left(\frac{1-\beta x}{1+\beta x}\right)^2,$$

viz., this belongs to the cubic transformation. The value of β in the cubic transformation was taken to be $\beta=\frac{u^3}{v}$, but for the present purpose it is necessary to pay attention to an omitted double sign, and write $\beta=\pm\frac{u^3}{v}$; this being so, $\delta=\beta\gamma$, and giving to γ the value $\mp u^4$, δ will have its foregoing value $=\frac{u^7}{v}$. And from the theory of the cubic equation, according as $\beta=\frac{u^3}{v}$ or $=-\frac{u^3}{v}$, the modular equation must be $u^4-v^4+2uv(1-u^2v^2)=0$, or $u^4-v^4-2uv(1-u^2v^2)=0$.

We thus see *à priori*, and it is easy to verify, that the equations of the septic transformation are satisfied by the values

$$a=1,\ \beta=\frac{u^3}{v},\ \gamma=u^4,\ \delta=\frac{u^7}{v},\ \text{and}\ u^4-v^4+2uv(1-u^2v^2)=0;$$

$$a=1,\ \beta=-\frac{u^7}{v},\ \gamma=-u^4,\ \delta=\frac{u^7}{v},\ \text{and}\ u^4-v^4-2uv(1-u^2v^2)=0;$$

and it hence follows that in obtaining the modular equation for the septic transformation, we shall meet with the factors $u^4-v^4\pm2uv(1-u^2v^2)$. Writing for shortness $uv=\theta$, these factors are $u^4-v^4\pm2\theta(1-\theta^2)$, the factor for the proper modular equation is $u^8+v^8-\Theta$, where

$$\Theta=8\theta-28\theta^2+56\theta^3-70\theta^4+56\theta^5-28\theta^6+8\theta^7$$

[viz., the equation $(1-u^8)(1-v^8)-(1-uv)^8=0$ is $u^8+v^8-\Theta=0$], and the modular equation as obtained by the elimination from the two quadric equations in fact presents itself in the form

$$(u^4-v^4+2\theta-2\theta^3)^2(u^4-v^4-2\theta+2\theta^3)^2(u^8+v^8-\Theta)=0.$$

March 14, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

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

The following Papers were read:—

I. "On Professor Haughton's Estimate of Geological Time." By GEORGE H. DARWIN, M.A., Fellow of Trinity College, Cambridge. Communicated by J. W. L. GLAISHER, M.A., F.R.S. Received February 19, 1878.

In a paper recently read before the Royal Society,* Professor Haughton has endeavoured by an ingenious line of argument to give an estimate of the time which may have elapsed in the geological history of the earth. The results attained by him are, if generally accepted, of the very greatest interest to geologists, and on that account his method merits a rigorous examination. The object, therefore, of the present note is to criticise the applicability of his results to the case of the earth; and I conceive that my principal criticism is either incorrect, and will meet its just fate of refutation, or else is destructive of the estimate of geological time.

Professor Haughton's argument may be summarised as follows:— The impulsive elevation of a continent would produce a sudden displacement of the earth's principal axis of greatest moment of inertia. Immediately after the earthquake, the axis of rotation being no longer coincident with the principal axis, will, according to dynamical principles, begin describing a cone round the principal axis, and the complete circle of the cone will be described in about 306 days. Now, the ocean not being rigidly connected with the nucleus, a 306-day tide will be established, which by its friction with the ocean bed will tend

* "Notes on Physical Geology. No. III. On a New Method of finding Limits to the Duration of certain Geological Periods." "Proc. Roy. Soc.," vol. xxvi, pp. 534—546 (December 20, 1877).

to diminish the angle of the cone described by the instantaneous axis round the principal axis: in other words, the "wabble" set up by the earthquake will gradually die away.

Then by means of Adams and Delaunay's estimate of the alteration of the length of day, which is attributed to tidal friction, Professor Haughton obtains a numerical value for the frictional effect of the residual tidal current. He then applies this to the 306-day tide, and deduces the time required to reduce a "wabble" of given magnitude to any given extent.

He is of opinion that if, at the present time, the instantaneous axis of rotation of the earth were describing a circle of more than 10 feet in diameter at the earth's surface, then the phenomenon could not escape detection by modern astronomical instruments. From the absence of any such inequality he concludes, after numerical calculation, "if Asia and Europe were manufactured *per saltum*, causing a sudden displacement of the axis of figure through 69 miles, that this event cannot have happened at an epoch less than 641,000 years before the present time, and that this event may have occurred at an epoch much more remote."

He then passes on to consider the case where the elevation takes place by a number of smaller impulses instead of by one large one. He treats first the case of "69 geological convulsions, each of which displaced the axis of figure through one mile," and where "the radius of the wabble" is "reduced from one mile to 5 feet in the interval between each two successive convulsions;" and, secondly, the case where "the increase of this radius is exactly destroyed by friction during each wabble, so that the radius of 5 feet continues constant."

In the first case he finds that the total time occupied by the manufacture of Europe and Asia is $27\frac{1}{2}$ millions of years, and also that "no geological change, altering the position of the axis of figure through one mile, can have taken place within the past 400,000 years." And in the second case, he finds that the same elevation would occupy 4,170 millions of years. A little lower he adds: "It is extremely improbable that the continent of Asia and Europe was formed *per saltum*, and therefore our minor limit of time is probably far short of the reality."

It appears from these passages that Professor Haughton is of opinion that a succession of smaller impulses at short intervals will necessarily increase the radius of the "wabble;" but it is not very clear to me whether he means that the radius of the "wabble" would be the same by whatever series of impulses the principal axis was moved from one position to another. Now, I conceive that it is by no means necessary that a second impulse succeeding a first should augment the radius of the "wabble;" it might, indeed, annihilate it. I admit that by properly timed impulses the radius of the "wabble" might be made as

great as if the whole change took place by a single convulsion. But where the impulses take place at hazard there will be a certain average effect on the radius of the "wabble," which, as far as I can see, Professor Haughton makes no attempt to determine. It seems, therefore, an unjustifiable assumption that sufficient time must elapse between the successive impulses to reduce the radius of the "wabble" to 5 feet, for if the impulses took place more frequently they might tend to some extent to counteract one another. If this assumption is unjustifiable, then Professor Haughton's estimate of time falls with it.

In my paper on the "Influence of Geological Changes on the Earth's Axis of Rotation,"* I have considered the effects of a slow continuous distortion of the earth. The results there attained would, of course, have been identical, had I considered the effects of a series of infinitely small and infinitely frequent earthquakes. I presume Professor Haughton will agree with me in thinking this supposition more consonant with geological science than the larger earthquakes which he postulates.

I will now show, from the results of my paper, that *without calling in any effects whatever of tidal friction*, Asia and Europe might have been gradually upheaved in 19,200 years, without leaving any "wabble" sufficiently large to be detected astronomically, and, moreover, that at no time during the elevation could the "wabble" have been detected had astronomers been in existence to make observations; and further, that under certain not improbable suppositions, this estimate of time may be largely reduced. Let a be the angular velocity of the principal axis relatively to the solid earth, arising from the continuous elevation of the continent; n the earth's angular velocity of rotation; C, A the greatest and least principal moments of inertia of the earth; and $\mu = \frac{C-A}{A} n$.

Then, in section 2 of my paper, I show that the extremity of the instantaneous axis describes a circle at the earth's surface in 306 days, and that this circle passes through the extremity of the principal axis, and touches the meridian along which the principal axis is travelling with velocity a in consequence of the postulated geological change. Strictly speaking, the curve described by the instantaneous axis, is a trochoid, because the circle travels in the earth along with the principal axis; but the motion of the circle is so slow compared with that of the instantaneous axis along its arc, that it is more convenient to say that the instantaneous axis describes a circle which slowly changes its position. It must be noticed that this circle is unlike the "wabble" considered by Dr. Haughton, inasmuch as the extremity of the principal axis lies on its arc instead of being at its

* "Phil. Trans.," vol. clxvii, pt. I, p. 271.

centre. It is also shown in the same section that the diameter of the circle is equal to $\frac{2a}{\mu}$.

I will now suppose that the geological changes begin suddenly from rest, and proceed at such a rate that the variations in the position of the principal axis are imperceptible to astronomical observation. I will suppose, therefore, that the extremity of the instantaneous axis is never more than 5 feet distant from the extremity of the principal axis. Now, 5 feet at the earth's surface, subtends very nearly ·05″ at the earth's centre, and, therefore, to find a on this supposition, $\frac{2a}{\mu}$ must be put equal to ·05″.

μ is an angular velocity of 360° in 306 days, and if we wish to express a in seconds of arc per annum, μ must be expressed in those units, and ·05″ must be expressed in circular measure. Thus

$$a=\frac{1}{2}\times\cdot05\times\frac{\pi}{648000}\times360\times60\times60\times\frac{365\cdot25}{306}$$
$$=\frac{18\cdot263}{306}\pi=\frac{3}{16}\text{ very nearly.}$$

Therefore, a is an angular velocity of 1° (or 69 miles) in 19,200 years.

But, according to Professor Haughton, 69 miles is the displacement of the earth's principal axis, due to the elevation of Europe and Asia; hence, at this rate of elevation, Europe and Asia would have been heaved up in 19,200 years.

Now, if the elevation be supposed to stop suddenly, then the instantaneous axis cannot, at the time of the stoppage, be more than 5 feet distant from the axis of figure, and it may even be coincident with it. Therefore the stoppage cannot set up a "wabble" of more than 10 feet in diameter, and it may set up none at all. But even this maximum "wabble" of 10 feet, would, according to Professor Haughton, be imperceptible, and *à fortiori* the circle of 5 feet in diameter, described in the course of the elevation, would be imperceptible.

On any of the following suppositions, the elevation might be much more rapid, without increasing the residual "wabble":—

(1.) The stoppage of the elevation to take place at a time when the instantaneous axis is separated from the principal axis by a small angle.

(2.) The elevation partly counterbalanced by simultaneous elevations in other parts of the world, so that the upheaval of Europe and Asia would not displace the pole of figure by so much as 69 miles.

(3.) The elevation partly or altogether produced by the intumescence of the strata immediately underlying those continents. (See Part VI of my paper above referred to.)

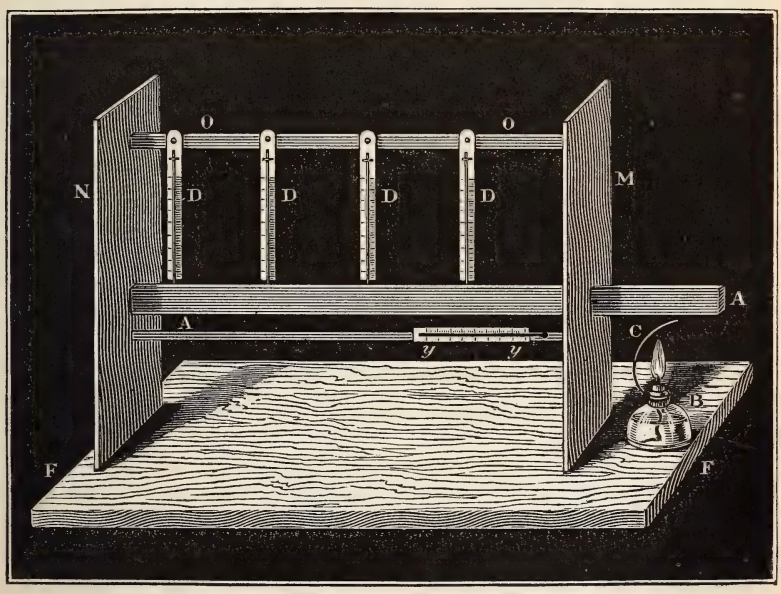
(4.) The elevation not uniform but more rapid in the earlier

portion of the time, so that the magnitude of the "wabble" would be reduced by the friction of the 306-day tide; for we are by no means compelled to believe that that inequality of motion must always have remained as small as it is at present.

It appears to me, from these considerations, that the continents of Europe and Asia might have been elevated in very much less than 20,000 years, and yet leave no record of the fact in the present motion of the earth. Therefore, if my solution of the problem is correct, it is certain that Professor Haughton's method can give us no clue to the times which have elapsed in the geological history of the earth.

II. "Some Experiments on Conductive Properties of Ice, made in Discovery Bay, 1875–6." By Staff Surgeon R. W. Coppinger, M.D. Communicated by Professor Tyndall, F.R.S. Received February 21, 1878.

In attempting a series of experiments for determining the rate of conduction of heat through sea and fresh-water ice, I have endeavoured as closely as possible to follow the suggestions made by Professor Tyndall in page 34 of the "Scientific Instructions for the Arctic Expedition." With this view, I have constructed the simple appa-



ratus shown in the diagram. It consists of a wooden baseboard (F F), 29 inches in length by 9 in breadth, on which are fixed two upright

wooden battens (NM), standing 24 inches apart, and connected above by a light crosspiece (OO). In the upright battens, at 4 inches above their bases, are apertures $1\frac{1}{4}$ inches square, for the reception of the bar of ice (AA) on which the experiment is made, while a series of thermometers (DDDD) are placed 4 inches apart, secured above to the crosspiece (OO), and having their bulbs imbedded in the icebar below.

For the manufacture of ice-bars I had constructed a strong copper tube, 29 inches long by $1\frac{1}{8}$ inches in sectional area, from which on being filled with water, and exposed to a low temperature, bars of fresh or sea-water ice were obtainable. The removal of these bars from the mould was effected by the application of warm water to the outside of the tube, which, melting the surface of the ice-bar, allowed it to be extracted.

The ice-bar, being placed in position, projects, when of its full length, 5 inches beyond the upright batten (M), as shown in the figure. Below the projecting end is placed a spirit-lamp (B), fitted with a sheet-iron plate (C), curved in such a manner as to prevent the bare flame from touching the ice, and to divert the drops of melted ice from the wick of the lamp. This lamp can be so placed that its heat will bear pretty accurately on the under surface of the bar at either 2, 3, or 4 inches as desired from M. The thermometers being placed at intervals of 4 inches apart, their distances from the source of heat are respectively 6, 10, 14, and 18 inches when the lamp is 2 inches from the upright. Their bulbs are sunk in holes bored in the ice, and are packed in with powdered ice, so as to render the connexion with the bar as perfect as possible without incurring the risk of the instruments being broken.

At all low temperatures the instruments used were Hick's spirit thermometers. Although these instruments were supplied to the ship as of good reputation, their errors at temperatures below $-10°$ differed so much in each instrument for every few degrees of the scale, that I found it best in recording the rate of heat-flow along the ice-bar to take the temperature actually indicated by each thermometer *uncorrected*, and to note the *range* of temperature in each, assuming that its error for every two or three degrees, at all events, was uniform.

In the earlier experiments I noted the temperature every 5 minutes for about 2 hours, beginning 5 minutes after the lamp had been lighted; but finding that interval too small to show any decided difference in temperature, I had recourse to 10-minutely, and ultimately to 15-minutely intervals. The entire duration of each experiment was limited to one hour, as by that time so much of the projecting end of the bar had melted away as to lessen the influence of the lamp upon it.

In order to determine whether the rise in temperature indicated by the thermometers might be to any extent due to radiation of heat from the lamp rather than to conduction through the ice-bar, the thermo-

meter YY was placed on the inner side of the upright M, having its bulb exposed to the air and 5 inches distant from the lamp. Having found from experiments I and II that a certain amount of radiation really existed, a broad screen, composed of layers of lint and pasteboard, was placed against the outer side of the upright M, and perforated, so as to closely encircle the bar in its passage through. This screen was used in III and subsequent experiments.

In order to be free from draughts, and yet to be in a place of low and pretty uniform temperature, the observations were made in a closed-in ice-building on the floe (that which was used as a theatre), and the temperatures of the external air and of the air inside the building were on each occasion noted. It was found that whenever the temperature in the open air was below −20° F., the temperature inside the building was about 10° warmer; this difference being accounted for by the heat conveyed through the floe from the sea-water beneath.

From a mean of five experiments on fresh-water ice it appears that in one hour from the time when heat was applied, the temperatures at the distances of 6, 10, 14, and 18 inches from the source of heat had risen 4°·45, 1°·78, 1°·30, and 0°·25, respectively. This result furnishes a rough method of estimating the absolute rate of conduction. It would have been more satisfactory if in all cases the observations could have been continued until the thermometers indicated fixed temperatures, but this was impracticable from the thawing of the end of the ice-bar; and if but a very moderate heat had been applied, the changes in temperature would have been hardly appreciable. In most cases but little change in temperature took place after the hour had elapsed, and in the case of experiment VIII, when a series of observations extending over two hours was carried out, the thermometer indicated no material change in temperature throughout the second hour.

Table VI gives the results of a series of observations upon a bar of sea-water ice, the rise in temperature being respectively 4°, 2°, 1°·5, and 1°·5.

It will be seen that in Tables I, II, and III, the temperatures indicated by thermometer YY are given, and that in I and II its ranges of temperatures are respectively 4° and 2°·5. In III the screen to prevent radiation was used, and every precaution was taken that the temperature of the ice-house might be uniform throughout the course of the experiment. In this case thermometer YY shows a rise in first hour of only 0°·25. To exhibit this more clearly Table VIII (the first half of which is only a repetition of Table III) is given, when the results of observations extending over two consecutive hours are recorded; and from which it appears that the ice thermometers indicated almost fixed temperatures from the end of the first hour. The temperature of

the house varied but half a degree during the course of the experiment, and the thermometer YY showed a rise of only 0°·75, which is small when compared with its short distance (5 inches) from the flame. The results, therefore, afforded by Table VIII may be considered highly satisfactory.

I refrain from making any general observations or drawing any special conclusions as to the subject of this paper, but simply submit the above few observations with the hope that in abler hands than mine, the facts which they contain may be made to add something to our knowledge of the conductive properties of ice.

I. Fresh-water Ice. Flame 6 inches from nearest Thermometer.

Temperature of outer air = − 37°. January 3, 1876.

Time.	Ther. 1.	Ther. 2.	Ther. 3.	Ther. 4.	Temperature in ice house.	Ther. Y.Y.
hours min.						
9 8	−26°·75	−27°·75	−24°·00	−23°·00	−24°	−27°
9 23	−26·00	−27·00	−23·5	−23·00	−23	−25
9 38	−24·5	−26·25	−23·00	−23·00	−23	−24·5
9 53	−22·5	−25·25	−22·5	−22·00	−22	−23·5
10 8	−21·5	−24·75	−22·00	−21·5	−22	−23
Range	5·25	3·00	2·00	1·5	2	4

II. Fresh-water Ice. Flame 6 inches from nearest Thermometer.

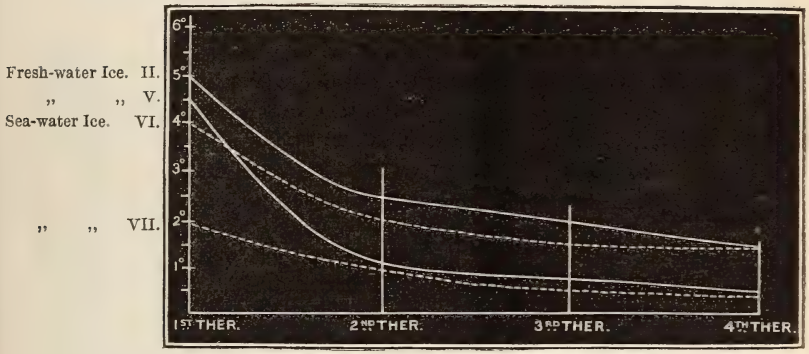
December 28, 1875.

Time.	Ther. 1.	Ther. 2.	Ther. 3.	Ther. 4.	Temperature in house.	Ther. Y.Y.
hours min.						
5 55	−24°·5	−25°·25	−22°	−21°·25	−25°·25	−24°·5
6 10	−23·5	−24	−21·5	−21	..	−23·5
6 25	−22	−24	−20·75	−20·25	..	−23
6 40	−21	−23	−20·5	−19·75	..	−23
6 55	−19·5	−22·75	−20	−19·75	−25·25	−22
Range	5	2·5	2	1·5	0·0	2·5

III. Fresh-water Ice. Flame 6 inches from nearest Thermometer. Temperature of outer air = − 43°. January 7, 1876.

Time.	Ther. 1.	Ther. 2.	Ther. 3.	Ther. 4.	Temperature in house.	Ther. Y.Y.	Remarks.
hrs. min.	°	°	°	°	°	°	
9 46	−23	−24	−21	−20	−21	−23	Screen used to prevent radiation.
10 1	−22·25	−24	−20·75	−20	−20·75	−22	
10 16	−21	−23·5	−20·5	−19·90	−20·5	−22	
10 31	−20·1	−23·25	−20·5	−20	−20·75	−22	
10 46	−19·75	−23	−20·25	−20	−20·5	−22·75	
Range..	3·25	1·0	0·75	0·0	0·5	0·25	

Curve of relative Conductivities.



IV. Fresh-water Ice. Flame 6 inches from nearest Thermometer. Temperature of outer air = − 34°. January 11, 1876.

Time.	Ther. 1.	Ther. 2.	Ther. 3.	Ther. 4.	Temperature of house.
hours minutes					
12 13	−19°25	−16°	−16°	−18°5	−17°
12 28	−18·25	−17·75	−15·25	−18	−16
12 43	−17	−15·1	−15	−17·5	−15·75
12 58	−16	−14·75	−15	−17·5	−15·25
1 13	−15	−14·75	−15	−17·25	−15
Range	4·25	1·25	1·00	1·25	2

V. Fresh-water Ice. Flame 6 inches from nearest Thermometer. Temperature of outer air = − 46°. February 10, 1876.

Time.	Ther. 1.	Ther. 2.	Ther. 3.	Ther. 4.	Temperature of house.
hours minutes					
6 50	−22°	−20°·75	−21°·5	−21°·5	−21°·5
6 5	−21·5	−20·5	−21·25	−21·25	−21·1
6 20	−20	−20	−21	−21	−21
6 35	−18·25	−20	−21	−21	−21
6 50	−17·5	−19·16	−20·75	−21	−21
Range.....	4·5	1·15	0·75	0·5	0·5

VI. Sea-water Ice. Flame 6 inches from nearest Thermometer. March 13, 1876.

Time.	Ther. 1.	Ther. 2.	Ther. 3.	Ther. 4.	Temperature of house.
hours minutes					
11 7	−25°	−24°	−24°·5	−24°·5	−25°·5
11 22	−24·25	−23·25	−24	−24	−24·75
11 37	−23·1	−22·8	−23·75	−23·75	−24
11 52	−22	−22·1	−23·1	−23·1	−23·5
12 7	−21	−22	−23	−23	−23·25
Range.....	4	2	1·5	1·5	2·25

VII. Sea-water Ice. Flame 8 inches from nearest Thermometer. Temperature of outer air = − 55°. February 29, 1876.

Time.	Ther. 1.	Ther. 2.	Ther. 3.	Ther. 4.	Temperature of house.
hours minutes					
6 15	−22°	−21°	−21°·5	−21°·5	−20°
6 30	−21·75	−20·5	−21·4	−21·5	−20
6 45	−21	−20·25	−21·1	−21·2	−20
7 0	−20·75	−20·1	−21	−21·1	−20·5
7 15	−20·1	−20	−21	−21·1	−20·5
Range.....	1·9	1·0	0·5	0·4	—

VIII. Fresh-water Ice. Flame 6 inches from nearest Thermometer. Temperature of outer air = − 37°. January 3, 1876. Heat applied continuously for two hours.

Time.		Ther. 1.	Ther. 2.	Ther. 3.	Ther. 4.	Temperature of ice house.	Ther. Y.Y.
hours	min.						
9	46	−23°	−24°	−21°	−20°	−21°	−23°
10	1	−22·25	−24	−20·75	−20	−20·75	−22
10	16	−21	−23·5	−20·5	−19·9	−20·5	−22
10	31	−20·1	−23·25	−20·5	−20	−20·75	−22
10	46	−19·75	−23	−20·25	−20	−20·5	−22·75
11	1	−19·25	−23	−20·25	−20	−20·5	−23
11	16	−19·5	−23	−20·5	−20	−21	−23
11	31	−19·25	−23·1	−20·5	−20·25	−20·9	−22·5
11	46	−19	−23	−20·25	−20	−20·75	−22·25
Range in 1 hr.		3·25	1·0	0·75	0	0·5	0·25
Range in 2 hr.		4	1	0·75	0	0·25	0·75

III. "On the Function of the Sides of the Vessel in maintaining the State of Supersaturation." By CHARLES TOMLINSON, F.R.S. Received February 21, 1878.

Before any consistent theory can be framed of all the phenomena of supersaturated saline solutions, it is necessary to determine whether the sides of the vessel bear any, and what part, in maintaining the state of supersaturation.

It is remarkable that among the multitude of memoirs and papers that have been published on the subject of supersaturation generally, and of special phenomena in particular, my reading should not have made me acquainted with any special experimental researches conducted with the view of determining the point in question.

In 1819, Gay-Lussac* seeing how easily a supersaturated solution of sodic carbonate can be made to solidify, by merely shaking the vessel, expressed his opinion that the state of supersaturation depends, not on a chemical, but a purely mechanical force. "We cannot," he says, "fix the point at which supersaturation ends, since it is entirely accidental in each experiment, depending, as it does, on the nature of the vessel, its polish, its conducting power, and the agitation of the air."

In 1851 Lamy† arrived at the conclusion that the form, thickness, and nature of the vessel, and the quantity of solution contained therein, have no influence on the state of supersaturation.

* Ann. de Ch. et Phy., 2e Serie, xi, 303. † Comptes Rendus.

Löwel,* whose researches extend over about ten years, namely, from 1848 to 1857, makes frequent reference, in the course of his six memoirs, to the sides of the vessel. He is of opinion that the asperities of the sides exert no action in determining crystallisation; that nothing certain can be affirmed as to the size of the tubes in producing such an effect; that it is not a mechanical action, as Gay-Lussac supposed; that heat deprives the sides of their active, or as he calls it, dynamic power; that even in an open tube the sides are not in a passive state; that nothing is positively decided as to the action of the sides. He also refers to that mysterious unknown force which holds the crystals in an abnormal state; he also speaks of the inner surface of the flasks recovering that particular property of determining crystallisation which heat had deprived it of, the cause of which is unknown; and in his last memoir he supposes that the sides determine the formation of the normal salt by an action of contact.

I have already expressed my opinion† that had Löwel worked with chemically clean flasks and tubes, he would not have encountered so many contradictory results, or have expressed so many uncertain and conflicting opinions as those above quoted.

In 1866 Jeannel‡ threw out an opinion that the state of supersaturation is maintained in closed vessels by reason of the attraction of the sides and the saturation of the interior air.

I had long ago formed an opinion that the adhesion of the solution to the walls of the vessel is an important function in maintaining the state of supersaturation; and it seemed probable if this were so, that any force that could effectually detach the solution from a small portion of the side, below the surface, would cause the whole system to break down.

The most obvious mode of experiment seemed to consist in rubbing the interior surface with a clean wire. About five years ago I obtained a number of results in this way, but did not think them sufficiently important or trustworthy for publication. One source of fallacy soon became obvious. On moving the wire up and down in a nearly vertical direction, a film of the solution is being constantly dragged above the surface, which film, in consequence of rapid evaporation, disengages a molecule of the salt, and this acts as a nucleus to the whole of the solution. Attempts were made to prevent such an effect by tying a piece of linen dipped in hot water loosely round the neck of the tube, and passing the wire through the cloth into the tube, but the result was not satisfactory; for unless the crystals could be seen actually to start from the place rubbed and to spread from this point alone through the solution, no fair conclusion could be formed as to the effect of the rubbing. Friction with platinum, brass and steel wires seemed to

* Ann. de Ch. et de Phy.

† Proc. Roy. Soc., xvi, 408. ‡ Comptes Rendus, 2nd January.

have little or no effect on solutions of sodic sulphate and sodic acetate. The most satisfactory results were obtained with magnesic sulphate; but with this and other solutions the results varied with the state of the weather as the crystallising force varied, and also with the nature of the rubber, whether a glass rod or the wires just named.

At length it occurred to me to line the tubes with some substance which is not wetted by the solution or imperfectly so. Accordingly, two large tubes (5 oz. and 3 oz.) were made chemically clean, and coated with a solution of amber in chloroform. They were three parts filled with a solution of sodic sulphate (2 to 1), covered over, and left until the next day. A stout platinum wire was taken out of hot water and introduced into each tube, when moderately hard friction was made in a vertical line about one-sixth or one-eighth of an inch in length. A crystalline brush immediately diverged from the part rubbed, and spread all through the solution, the mass of which was sufficient for the eye to note the details, and to convey the assurance that crystallisation set in from the part rubbed and from that only.

The advantage of using amber is that the coating, being transparent, allows the progress of the experiment to be watched. The same advantage may, however, be obtained at less cost and trouble, by means of resin, a small quantity of which, in powder, together with a few drops of spirits of wine, heated in the tube over a spirit lamp, enables the operator easily to spread a coat evenly upon the inner surface by moving the tube about while it is still warm.

A number of one-ounce tubes, coated in this way, were filled with a stronger solution of sodic sulphate (3 to 1) than that before employed. The result was satisfactory: the solutions often became solid in cooling, and always when the inner surface was rubbed with the platinum wire.

The experiment was also made comparative by placing an uncoated by the side of a coated tube, and so leaving them to cool covered with glass capsules, or small beakers. The solution in the coated tube often solidified in cooling, and always remained liquid in the uncoated tube.

The results as obtained with supersaturated solutions of other salts also showed how greatly they depend for their stability on adhesion. The system always broke down when a portion of the solution was detached from the side, by the rubbing action of the wire. The solution of each salt, however, presented those characteristic features of its own, which form one of the principal charms in the study of natural objects. In the ammonia alum solution (1 to 1) the octohedral crystals, under the action of rubbing, seemed to bound forth towards the axis of the tube. In the magnesic sulphate solution (3 to 1) the disengaged crystals were too minute to appear, except as a chalky-white line following the motion of the wire. Zincic sulphate (3 to 1) formed small

woolly-looking crystals after a slight rubbing, and although the wire was rapidly withdrawn as soon as the crystals began to appear, it left behind it a curious kind of trail, consisting of an axis of the form of the wire, repeating its bends and irregularities, while small acicular crystals started out at right angles to this line, and thickly studded it soon after the whole of the solution became solid. Zincic acetate (1 to 1) did not display crystals as soon as the side was rubbed, but slowly, in the course of half-an-hour or so, the part rubbed was occupied by a dense crop of beautiful crystals, the points of which passed beyond the axis of the tube.

Sodic acetate solution does not readily yield to the effort of rubbing, except in certain states of the weather when the crystallizing force is strong; but a case occurred to me some years ago which exactly suits my present purpose. Several flasks containing a highly supersaturated solution of sodic acetate, to which oil had been added without any nuclear effect, were emptied into a small, stout cylindrical glass, where it remained during some weeks without change, the layer of oil on its surface preventing it from absorbing moisture. It was several times stirred with the finger without effect, for this, on being introduced, became coated with oil and so prevented contact. The finger was then pressed against the strong side of the vessel with a considerable amount of force and drawn slowly from the bottom upwards. The solution immediately became solid. I attributed this effect at the time to nuclear action, but I now regard it as an excellent illustration of the point which I am seeking to establish.

A supersaturated solution of sodic carbonate is very sensitive to any interference with its adhesion. A speck of carbon or of ferrous oxide, coming to the surface within the tube, is sufficient, on cooling down the solution, to cause the salt to start from the speck in crystalline lines in all directions, producing immediate solidification.*

In addition to amber and resin some tubes were coated with other substances, such as sulphur, shellac, stearine, and paraffin; but as some of these, in addition to the objection of being opaque, melted when the boiling solution came into contact with them, I need not refer to them any further, except perhaps to paraffin, with which an interesting result was obtained. A large tube was lined with paraffin and passed, telescope fashion, over a smaller tube, containing a nearly boiling solution of sodic sulphate. When cold, the tubes were slowly reversed, so as to pour the solution into the coated tube; but it solidified as soon as it came into contact with the paraffin. If, however, it were poured in while still warm, the solution accommo-

* In the experiments which I had the honour of exhibiting to the Society after the reading of my first paper (28th May, 1868), a case of this kind occurred and was pointed out by my late lamented friend, William Allen Miller, as "a very pretty effect."

dated itself to the new circumstances and remained liquid when cold.

The results of these experiments seem to point to the conclusion that adhesion to the side of the vessel is one of the conditions under which the state of supersaturation is maintained; and that whatever interferes with this adhesion, must prevent supersaturation or destroy it.

This conclusion was tested by another mode of experiment, starting from the idea that if the solution were made to expose an amount of free surface, about equal to the surface of contact with the vessel, the adhesive force would scarcely be sufficient to maintain the state of supersaturation, and the solution in cooling would crystallise as in an open evaporating dish. This is what really did happen in the case of ammonia alum (5 to 3). The solution in three covered shallow vessels deposited massive crystals in cooling, and when cold, the mother liquor was no longer supersaturated. A solution of the same degree of strength was boiled in a flask and the opening plugged with cotton-wool: this deposited no salt in cooling, because the area of adhesion was greatly in excess of that of the free surface. The solution under such conditions will remain unchanged for months. In all cases the re-boiling of the solution in the flask into which it is filtered greatly promotes adhesion, and, consequently, the duration of the solution in the state of supersaturation.

But to return. A similar crystallisation took place in the case of sodic sulphate; but that arose chiefly from the kind of vessel used and the mode of covering it, as will be noticed presently. I must first refer to supersaturated solutions of sodic sulphate (3 to 1) in shallow foot glasses, four inches in diameter and one inch deep in the middle. The boiling solution was poured into these glasses, which were immediately covered with shallow glass vessels, inverted over them so as to fit nicely and rest on their edges, or to fit with friction to the sides. The result was curious and interesting. Instead of crystallising in cooling, the solution relieved itself by throwing down large quantities of the seven-watered salt in finely shaped masses, while the liquid portion remained supersaturated.* Next day these solutions were taken into the open air and uncovered. From a point at the extreme edge of each, crystallisation set in and spread like a fan over the surface and through the solution. This was not a case of nuclear

* A three-ounce flask was coated with resin, and into it was poured a boiling solution of magnesia sulphate (3 to 1). The flask was covered with a small beaker and set on the window ledge to cool. A fine display of crystals of the modified salt sprang from the bottom nearly to the surface of the solution, which remained supersatuated. Here again, as in the case of sodic sulphate, the solution relieved itself by this deposit and there was sufficient adhesion on the part of the remaining liquid to maintain a moderate degree of supersaturation.

action derived from the atmosphere (rain having fallen during some hours and having scarcely ceased) but was due to another force. Last summer, as noticed in my former paper,* drops of sodic acetate exposed on glass always started the crystallisation from the edge. So, in these wide shallow glasses in the open air, crystallisation set in from the edge, or capillary curve, where the solution is most drawn out and attenuated. Now, supposing evaporation to be active all over the surface, this thin portion, becoming thinner by the process, would be the first to detach a molecule of the salt, and this, once set free, would act as a powerful nucleus to the whole. Lines, or rather thin planes of crystals, diverge from the point, and this so quickly as not to afford time for the molecules to arrange themselves into crystalline forms.

On one occasion the mass of seven-watered salt rose so near the surface, that the solution resting on it was scarcely more than a film. This, by evaporation, also disengaged a minute portion of the normal salt, which acted as a centre of crystallisation.

Solutions containing two parts of salt to one of water behaved much in the same way as the stronger ones. A weaker solution, containing only one of salt to one of water, accommodated itself to the conditions under which it was placed. It did not deposit any modified salt, and on being uncovered in the open air, crystallised from the edge.

In the case before referred to, where the solution crystallised in cooling, as in an open evaporating dish, namely, in the form of large prisms of the normal salt, the vessel was a shallow dessert dish with a scolloped edge, so that the protecting cover did not confine the air over the surface of the solution, but allowed it to circulate; hence, in cooling down, the molecules were also in a condition to circulate and gradually to arrange themselves into groups, ready to assume the crystalline form when the temperature had sufficiently declined. Whereas, in closely covered vessels, the strong adhesion to the side prevents this circulation and re-arrangement, the adhesion holding the molecules in a forced state, bearing some kind of analogy to the glass in Prince Rupert's drops and the Bologna phial.

It must also be noted, that in the last case also, there was no nuclear action; for, had there been, the solution would have cooled down to a certain point, and then have suddenly crystallised in closely packed planes; whereas, in the case now referred to, the crystals were large and of the usual shape, while the mother liquor was no longer supersaturated.

Solutions of zincic sulphate and of magnesic sulphate (3 to 1 each) in these covered shallow vessels, also threw down a good deal of the modified salt, in the case of the zinc solution the monohydrated salt

* Proc. Roy. Soc., xxvi, 528.

in long clustered cylinders, but being left undisturbed for a day or so, the remaining supersaturated liquor in both cases became solid, the normal salt being formed. Sodic acetate solution (6 to $1\frac{1}{2}$) poured boiling into these large shallow vessels and covered over did not change during twenty-four hours. On being taken into the open air and uncovered, the solutions remained some time in the liquid state, but on touching the bottom of the vessel with the finger, they immediately became solid.

The alum solution, before referred to, deposited the normal salt in large crystals, while the solution was still warm. It could not relieve itself in any other way, because by its constitution it cannot form a salt of a lower degree of hydration than the normal, that is, with 12 aq., and the throwing down of that in considerable masses, so as to get rid of the state of supersaturation altogether, forcibly illustrates the dependence of that state on the adhesion of the solution to the sides of the vessel.

The weather was mild while these experiments were in progress. At lower temperatures the solutions would doubtless have been even more sensitive.

The result of these experiments satisfies me that the state of supersaturation is dependent on the adhesion of the solution to the sides of the vessel, coupled with the tension of the surface. This tension may be lowered, so as to throw more work upon the other force, which it may be able to bear; but anything that effectually detaches a portion of the solution from contact with the sides of the vessel, produces the sudden crystallisation of the solution.

If this view be correct, many of the phenomena of supersaturation are accounted for, and the whole subject is far advanced towards that obedience to law, which can alone invest it with dignity.

March 21, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President in the Chair.

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

The following Papers were read:—

I. "Contact Theory of Voltaic Action." Parts I and II. By W. E. AYRTON and JOHN PERRY, Professors in the Imperial College of Engineering, Tokio, Japan. Communicated by Professor Sir W. THOMSON, F.R.S. Received October 2, 1877.

PART I.

I.

The contact theory of voltaic action seems to have undergone no development since the date of Sir W. Thomson's experiment, which consisted in connecting a plate of zinc and a plate of copper by means of a drop of water, when it was found that the metals were brought to the same electric potential, although when metallically connected they were at different potentials. He believed that any electrolyte would behave in exactly the same way as the water of his experiment, equalizing the potentials of any two metals connected by it. The electromotive force of a simple cell, ought, in accordance with the theory, to be equal to the difference of potentials between zinc and copper in contact. A test founded on this deduction was very difficult to apply, because there was no exact determination of the difference of potential of zinc and copper in contact, Sir W. Thomson, in his experiment, having really measured the difference of potential between air at the surface of a zinc plate, and air at the surface of a copper plate. In the absence of this test, the equality of the electromotive forces of simple cells in which zinc and copper are the metals (the liquids being water, dilute sulphuric acid, and sulphate of zinc) was held as a proof of the theory. Now it is known that when two pieces of the same metal are dipped into any two liquids, which are diffusing into one another, a difference of potentials is established between the metals, and the electromotive force of a cell of this kind can in no way depend on a difference of potentials due to metallic contact. So that although in such a cell there is an action which is somewhat the same as the action in a simple voltaic cell, the theory took no account of it whatever. In fact, the explanation of voltaic action given in the latest

treatises on electricity is felt to be incomplete, even by the writers of such treatises, and the present investigation has been entered upon in consequence.

Sir W. Thomson's result, and our own experiments lead us to imagine that when zinc and copper are immersed in water there are three successive states to be noticed :—At the instant of immersion the zinc and copper may be reduced to the same potential, so that the electromotive force of the voltaic cell E is equal to the difference of potential $\overline{ZC}$ between zinc and copper in contact; the zinc now becomes negative to the copper, so that E reaches a limit which is greater than $\overline{ZC}$; lastly, if a current passes, polarization occurs and the zinc becomes gradually less negative to the copper, E diminishing, therefore, from its maximum value. But when a saturated solution of zinc sulphate is employed instead of water, the first state, if it exists at all, exists for so short a time that practically, zinc and copper in zinc sulphate are never at the same potential. Thus (see Table X) when care is taken to keep the zinc and copper in a water cell well insulated from one another, E is found to increase from a value very little greater than $\overline{ZC}$, the electromotive force of contact of zinc and copper, to a limit, but in a zinc sulphate cell no such great increase is observed.

In our present experiments we are not concerned with instantaneous electromotive forces of contact, a short time elapsing in every case between the contact and the determination of its electromotive force; and with polarization in the ordinary sense we have equally no concern. In fact, our experiments relate to the maximum electromotive forces of simple and compound cells.

We find that zinc and copper connected by the electrolyte zinc sulphate, dilute sulphuric acid, or water are not at the same potential, the zinc being negative to the copper. In the case of dilute sulphuric acid, where polarization cannot be prevented, the negative charge of the zinc rapidly diminishes so that there is an instant at which the metals are reduced to the same potential; from Sir W. Thomson's result we may suppose that there are two such instants.

We did not measure the potentials of the surfaces which we mention but really of air in the neighbourhood of these surfaces, but the discrepancies of our results are sufficiently small for us to neglect whatever difference of potential may exist between a metallic or liquid surface, and the air in contact with it. In fact we have good reason to believe that there is no great difference of potential between a metallic or liquid surface, and the air in contact with it.

Our experiments show, but not with as much accuracy as might be desired, that the electromotive force of contact of two metals or two electrolytes, or of a metal and an electrolyte is in each case a constant; that is to say, if $\overline{AB}$ means the electromotive force of contact of the

metal or electrolyte A, and the metal or electrolyte B (measured when A and B are not in contact with other conducting substances), $\overline{AB}$ being identical with $-\overline{BA}$; then the total electromotive force of any closed heterogeneous circuit composed of the substances A, B, C, N is:—

$$\overline{AB}+\overline{BC}+ \&c. +\overline{NA}.$$

The proof of this law is very important, as it is often denied without experimental data. Profesor F. Jenkin says:—"The following series of phenomena occur when metals and an electrolyte are placed in contact:—1. When a single metal is placed in contact with an electrolyte, a definite difference of potentials is produced between the liquid and the metal. If zinc be plunged in water, the zinc becomes negative, the water positive. Copper plunged in water also becomes negative,* but much less so than zinc. 2. If two metals be plunged in water (as copper and zinc) the copper, the zinc, and the water forming a galvanic cell, all remain at one potential, and no charge of electricity is observed on any part of the system."

It will also, we think, become evident that the natures of the metals and of the electrolytes determine the electromotive force of a voltaic cell, just as the natures of the metals and the distribution of temperature determine the electromotive force of a thermo-electric arrangement, and as the nature of the two metals zinc and copper determines the electromotive force of Sir W. Thomson's mechanical cell in which copper filings fall from a copper funnel through a zinc inductor, in metallic connexion with the copper, into a copper vessel.

II.

At the surface of contact of two conducting substances there is an electromotive force of definite amount which tends to make electricity flow from one of the substances to the other. For instance, at the junction of zinc and copper there is an electromotive force of 0·75 volt which tends to cause electricity to flow from the copper to the zinc across the junction. This flow ceases when the potential of the zinc is 0·75 volt greater than that of the copper, the difference of potentials of the metals balancing the electromotive force of the junction. It is this difference of potential which is measured by the apparatus which is described below. When a substance M is said to have a higher potential than a substance N in contact with it, we mean that this state has been established in virtue of an electromotive force which exists at the junction of the substances, and that the electromotive

* We found that copper is positive to distilled water, zinc being negative to the water.

force of contact tends to produce a current of electricity from N to M across the junction.

Sir W. Thomson's method of measuring the difference of potential between two metals in contact, which consisted in hanging a needle symmetrically over two similar plates of different metals, and observing its deflexions when it was charged, first positively and then negatively, requires, in order to be accurate, all the delicacy of the arrangement and even more complicated contrivances than those employed in the quadrant-electrometer. It will be easily seen that a series of experiments for determining the difference of potentials between pairs of metals, pairs of liquids, and between liquids and metals, could only with the very greatest difficulty be carried out by a method which virtually requires the taking to pieces and refitting of a quadrant-electrometer for every experiment. The authors' attention was, therefore, directed to devising a method of investigation in which a delicate quadrant-electrometer would be employed to measure the difference of potentials between two similar metal plates themselves acted on inductively by parallel and equidistant plane surfaces of the two substances in contact, the difference of potentials between which it was desired to measure. Thus let A and B be two insulated brass plates connected with the electrodes of the electrometer. Let C under A and D under B be the surfaces whose difference of potential is to be measured. A and B are both put to earth and then insulated. C and D are made to change places with one another, C now being under B and D under A. The deflection of the electrometer needle will now give a measure of the difference of potential between C and D.*

The apparatus, by means of which our idea has been carried out, has, by a process of natural selection, assumed the form shown in the accompanying drawing. (Plate 7.) The circular brass plates A and B, 20·4 centims. in diameter, are supported and kept insulated by means of artificially dried glass stems. The leaden cups J, containing pumice stone moistened with sulphuric acid, slide in H by a bayonet-joint, and may be lowered by the handles I so as to rest on the brass plates when the instrument is not in use; thus preventing access to H of the external air.

The permanent adjustment for coincidence of the plane surfaces of the brass plates is effected by means of three wood-screws in each of the brass caps K; temporary adjustment for horizontality of the brass plates is effected by means of a spirit level and three levelling screws

* Suppose C and D to be very large, and that the protecting covering of the apparatus is far away from the insulated plates. Let the coefficients of induction of the two sets of quadrants when there is a difference of potential d between them be $s+dm$ and $s-dm$. We know from the position and shape of the needle that this is approximately true. Let a be the area of each of the insulated brass plates A and B; t the distance from A to C or from B to D. Let α be the potential of the sur-

TT, which support the wooden frame *WPW'*, and the positions of which on the three wooden blocks *T'* are fixed by "hole, slot, and plane." One of these levelling screws has been removed in the drawing to show the lifting arrangement.

F and *G* are strips of brass soldered to the plates *A* and *B*, and connected by means of clips to wires leading to the insulated electrodes of the quadrant electrometer, and to the insulated screws *A'*, *B'*, of the short-circuit key.

In the drawing we have chosen to arrange the apparatus for determining the electromotive force of contact between a metal *D* and a liquid *C*. *VS* is a screw-jack which enables the oblong wooden table *OS'O'* to be raised to any level, and the table may be turned round horizontally at any level. Three levelling screws, working in nuts in the table, support the metal plate *D*, and three others, exactly similar, support the porcelain vessel *G* which contains the liquid, so that the upper surfaces of both *C* and *D* are in the same horizontal plane. A binding screw *a* is soldered to the plate; in the present case it holds the metal strip *E*, which makes contact with the liquid; *E* must have been cut from the same sheet as the plate *D*. When there are two

face *C*, and $\alpha + v$ the potential of *D* at the beginning of the experiment. When *A* and *B* are connected to earth, the charge on *A* and its electrometer-quadrants is

$$-\frac{a\alpha}{4\pi t} - s\mathrm{V}$$

V being the potential of the needle. The charge on *B* and its quadrants is

$$-\frac{a(d+v)}{4\pi t} - s\mathrm{V}$$

After reversal, if *C* has a potential γ and *D*, therefore a potential $\gamma + v$, the charge on *A* and its quadrants is

$$\frac{a(\mathrm{V_A} - v - \gamma)}{4\pi t} + \left\{s - m(\mathrm{V_A} - \mathrm{V_B})\right\}(\mathrm{V_A} - \mathrm{V})$$

And the charge on *B* and its quadrants is

$$\frac{a(\mathrm{V_B} - \gamma)}{4\pi t} + \left\{s + m(\mathrm{V_A} - \mathrm{V_B})\right\}(\mathrm{V_B} - \mathrm{V})$$

where $\mathrm{V_A}$ and $\mathrm{V_B}$ are the potentials of *A* and *B* respectively. Equating the respective charges before and after reversal, and subtracting, we get

$$(\mathrm{V_A} - \mathrm{V_B})\left(\frac{a}{4\pi t} + s\right) - m(\mathrm{V_A}^2 - \mathrm{V_B}^2) = \frac{2av}{4\pi t} \quad . \quad . \quad . \quad (1)$$

If we might neglect the change of capacity of the quadrants, we should have

$$v = \frac{(\mathrm{V_A} - \mathrm{V_B})\left(\frac{a}{4\pi t} + s\right)}{\frac{2a}{4\pi t}} \quad . \quad . \quad . \quad . \quad (2)$$

so that v varies as $(\mathrm{V_A} - \mathrm{V_B})$.

Now our experiments show this to be the case, and consequently (2) gives the correct value of v.

vessels of liquid, contact is made between the liquids by wetting a piece of cotton wick in one of the liquids and allowing its ends to dip below the liquid surfaces.

The brass rods N', insulated where they pass through the wooden frame by glass tubes and bone washers, are gauges which may be lowered or raised until their ends are at fixed distances below the level of the plates A and B. By the reflections of these ends at the surfaces of the liquids or plates, we can judge of the position of the level surfaces C and D. The wooden pegs O and O', and a vertical slot in the stand at $W''W'''$, enable a reversal of C and D to be made with accuracy and rapidity.

The parts of the frame $W'W''W'''$ in view of the brass plates are covered with tin plates or tin foil, connected with the outside of the electrometer. A tin plate door, of which the hinges are seen at MM, shuts up a metal box enclosing the whole apparatus, including the electrometer, short-circuit key, and connecting wires. This box is permanently in connexion with the outside of the electrometer. The plug Z of the short-circuit key is kept connected with this cover through a hole in which it may be withdrawn from its bed to insulate the plates.

We shall now describe a complete operation, to obtain the electromotive force of contact between a metal and a liquid, for example. Suppose the permanent adjustments to have been made, and that the plates A and B are quite bright. The plate D is cleaned and laid on the levelling screws R; the porcelain dish containing the liquid is laid on the levelling screws Q. By turning S and the levelling screws (O moving in its slot), and by the use of a gauge gh of which the h part represents the distance apart determined upon for A and C and B and D, we get the surfaces into their proper relative positions. The brass rod N' is now lowered until its end and its reflection at the surface of D seem to meet, and it is then made fast. Before proceeding further, the insulation of the glass rods, the permanence of the zero of the electrometer, and the permanence of position of the needle when the quadrants are insulated, are ascertained. The strip E which is quite bright is now bent to dip into the liquid, and the cover M is lowered. Insulate by means of the key, and take the zero. Now, lift the cover M, lower the table and reverse C and D; raise the table until the end of N and its reflection in the surface of the liquid nearly meet; lower the cover and take a reading. Now, short-circuit the quadrants, insulate, and proceed to take a new reading. Alternate readings are on opposite sides of the zero.

We found that when slight changes occur in the dryness of the air inside the electrometer, there are changes in the zero which are probably due to slight traces of moisture remaining on the suspension fibres of the needle. It is also to be remarked that when the quadrants of the

electrometer are insulated from one another, any slight change in the charge of the instrument causes a motion of the needle.

The hand must be trained to insulate the plates from one another. After three months' practice our insulations are so perfect that there is seldom even a tremor on removing the plug, whereas in the beginning of our experiments we often had in succession ten bad attempts at insulation. A good insulation depends on the plug leaving both sides of its seat simultaneously. This key gave better results than any of the other arrangements at our disposal.

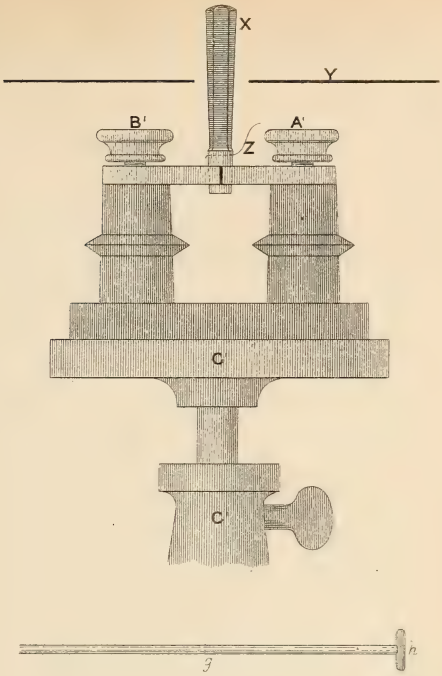
We at first thought, from considering the change of capacity in the quadrants (as the needle moved), that our measurements would be more accurate when very small. This is not the case, but it led us to try a method of observation which may suggest itself. We brought the spot of light to zero in every experiment by acting inductively on the insulated plates A and B by means of small plates placed over them charged to a measurable difference of potential. We may employ this method modified at some future time.

The suspension fibres of the needle were tolerably near one another, and the trap-door gauge was adjusted for as great a charge as it was convenient to give to the jar, and as it was nearly unstable it indicated extremely small changes of charge. The relative positions of the mirror G, the lamp, lens, and scale* are shown in the lower right-hand part of the Plate. The lens forms a virtual image of the wire of the lamp-slit at 60 centims. from the mirror G', and the scale I*, which is two metres from the electrometer, receives the image formed by the mirror. We had supposed that no better definition of the slit could be obtained by the use of a lens, but in reality great benefit was derived from its use, our readings being much more accurate than they would have been had the lamp-slit, without the lens, been put at 60 centims. from the mirror. We consider that our readings (as far as the electrometer itself is concerned) are fairly accurate to the one two-thousandth part of a volt.

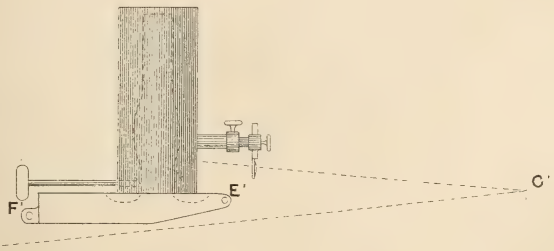
III.

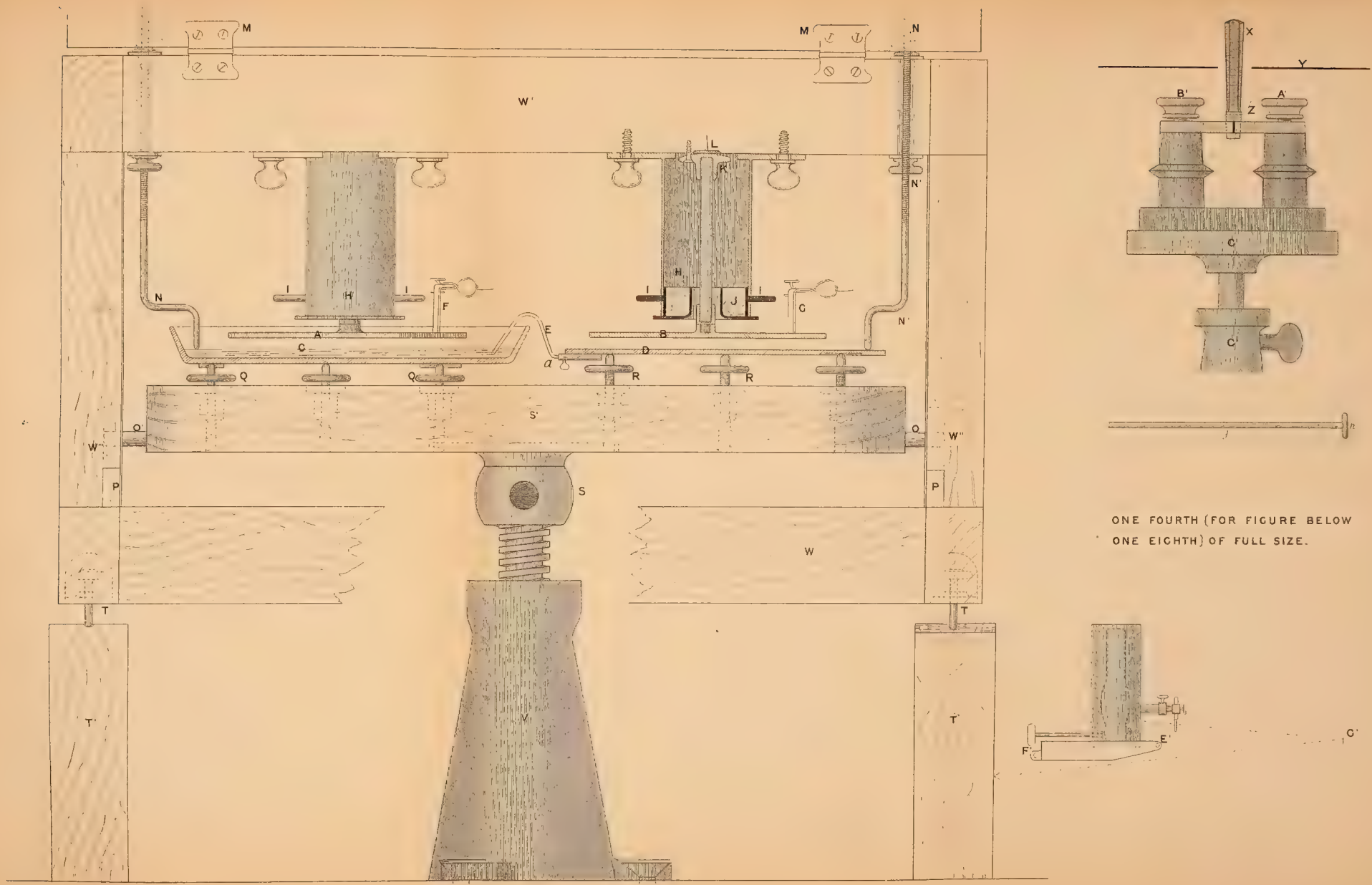
To find the relative values of deflections of the electrometer needle, two brass plates were placed under A and B at the distance of 6·4 mm. We were able to give to the plates a difference of potentials equal to the electromotive force of a Daniell, or to any fraction of this. Reversal is made by means of a key without any actual changes in the positions of the plates.

* Not represented in the Plate.



ONE FOURTH (FOR FIGURE BELOW ONE EIGHTH) OF FULL SIZE.





Harrison & Sons, Lith. St. Martins Lane, W.C.

TABLE I.—9th March, 1876. Plates 6·4 mm. apart.

One Cell.

Zero.	Reading.	Deflection.
1058·0	1127·3	69·3
56·9	990·0	66·9
59·9	1129·8	69·9
64·0	997·0	67·0
65·0	1134·0	69·0
67·1	1000·0	67·1
68·0	1136·6	68·6
68·4	1000·0	68·4
68·6	1137·0	68·4
69·2	1001·5	67·7

Mean.....68·2

0·9 of a Cell.

1074·0	1138·0	64·0
73·0	1016·0	57·0
72·0	1135·0	63·0
71·0	1011·1	59·9
68·9	1129·8	60·9
67·1	1004·9	62·2
64·0	1125·0	61·0
62·8	1001·0	61·8
61·2	1122·5	61·3
60·1	998·5	61·6
59·6	1120·0	60·4
60·0	1000·0	60·0
60·0	1121·0	61·0

Mean.....61·1

0·8 of a Cell.

1063·5	1007·5	56·0
64·1	1119·1	55·0
65·2	1010·0	57·2
65·8	1121·0	55·2
67·0	1012·2	54·8
67·5	1123·2	55·7

Mean.....55·3

0·7 of a Cell.

Zero.	Reading.	Deflection.
1070·0	1022·0	48·0
70·1	1119·0	48·9
71·1	1024·0	47·1
72·0	1121·5	49·5
73·0	1025·0	48·0
73·0	1121·5	48·5
73·8	1025·0	48·8
73·5	1122·0	48·5

Mean.....48·4

0·5 of a Cell.

1073·0	1039·0	34·0
73·0	1107·2	34·2
73·0	1038·1	34·9
72·8	1107·5	34·7
72·3	1038·0	34·3

Mean.....34·4

0·4 of a Cell.

1068·9	1096·0	27·1
68·0	37·0	31·0
67·1	94·1	27·0
67·0	38·2	28·8
66·8	94·0	27·2
66·3	38·5	27·8
66·0	92·5	26·5
66·0	37·1	28·9
65·6	92·0	26·4
65·8	37·5	28·3

Mean.....27·9

0·3 of a Cell.

1066·1	1086·4	20·3
66·1	45·0	21·1
66·1	86·0	19·9
66·1	45·0	21·1
66·9	87·0	20·1
67·0	45·5	21·5
66·3	86·0	19·7
68·1	46·0	22·1

Mean.....20·7

0·2 of a Cell.

Zero.	Reading.	Deflection.
1069·0	1082·2	13·2
69·0	54·1	14·9
68·0	81·0	13·0
67·2	52·5	14·7
66·6	80·0	13·4
66·8	52·0	14·8

Mean.....14·0

0·1 of a Cell.

1063·0	1056·0	7·0
64·0	70·6	6·5
65·0	57·8	7·2
65·5	71·6	6·1
64·8	56·5	8·3
63·4	69·1	5·7

Mean.....6·8

Taking fractions 0·1, 0·2, &c., of 68·2, which is the mean deflection representing the electromotive force of a whole cell, we get the numbers A of Table II. The numbers B are the means of Table I.

Table II.

A..	6·8	13·6	20·5	27·3	34·1	47·7	54·6	61·4	68·2
B..	6·8	14·3	20·7	27·9	34·4	48·4	55·3	61·1	68·2

Thus the deflections are proportional to the differences of potential between the lower plates, a result which could not have been anticipated. At the distance of 16 mm., the results given in Table III were obtained, and here again we find that the deflections are proportional to the difference of potential between the lower plates.

Table III.—9th March, 1876. Plates 16 mm. apart.

One Cell.

Zero.	Reading.	Deflection.
1065·1	1093·0	27·9
61·8	23·5	38·3
59·5	91·5	32·0
60·6	26·5	34·1
61·3	96·0	34·7

Zero.	Reading.	Deflection.
1062·8	1027·8	35·0
62·2	94·1	31·9
64·9	31·5	33·4
64·7	98·0	33·3
62·1	24·5	37·6

Mean.....33·8

0·5 of a Cell.

1064·0	1080·1	16·1
64·6	45·8	18·8
57·1	70·8	13·7
57·0	37·5	19·5

Mean.....17·0

It was not necessary to prove the law of proportionality for other distances than 6·4 and 16 mm.

Table IV.—9th March, 1876. Plates 10·9 mm. apart.

One Cell.

Zero.	Reading.	Deflection.
1055·5	1101·5	46·0
57·0	1006·6	50·4
55·8	1100·6	44·8
56·2	1005·5	50·7
56·1	1100·0	43·9
55·0	1002·0	53·0
53·4	1098·5	45·1
54·0	1004·0	50·0

Mean.....48·0

The Daniell was occasionally connected with the electrometer directly. It constantly gave the deflection 230·4. It was not a new Daniell. Thus the ratio between the deflections obtained by two methods of measuring a difference of potential, one by direct connexion with electrometer, and the other by reversing the induction plates, is 3·4, when the distance between the plates is 6·4 mm., 4·8 for 10·9 mm., and 6·8 for 16·0 mm. Plotting three points on squared paper with these numbers as co-ordinates, and drawing the arc of a circle through them, we found the approximate values of the ratios corresponding with different distances given in Table V. This table was

useful in showing how any error in adjusting the plates affected the deflection.

Table V.

Distance in millimetres	6	7	8	9	10	11	12	13	14
Ratio.....	3·30	3·55	3·84	4·13	4·48	4·83	5·21	5·62	6·05

In subsequent experiments we used the distances 10·9 and 10·0 mm. As the electrometer was twice taken to pieces, its quadrants being altered in position, we took great care in the later experiments to determine the value of our deflections in each case.

Table VI.—12th March, 1876. Plates 10·9 mm. apart.

Zinc and copper metallically connected. Zinc positive to copper.

Zero.	Reading.	Deflection.
1097·1	1133·1	36·0
85·5	1048·0	37·5
77·1	1112·0	34·9
80·0	1040·6	39·4
82·1	1119·1	37·0
86·8	1047·0	39·8
83·0	1114·4	31·4
86·3	1046·0	40·3
91·2	1123·0	31·8
90·0	1047·1	42·9

Mean.....37·1

Zinc and copper connected by an acid solution of zinc sulphate at 10° C. Zinc negative to the copper.

Zero.	Reading.	Deflection.
1034·8	1026·1	8·7
34·1	41·8	7·7
36·9	30·1	6·8
37·1	49·1	12·0
39·7	31·1	8·6
40·0	51·5	11·5
45·0	35·1	9·9
43·0	55·1	12·1
43·0	35·0	8·0
42·3	52·1	9·8

Mean.....9·5

Zinc and copper connected by distilled water at 10° C. Zinc negative to copper.

Zero.	Reading.	Deflection.
1040	1033·1	6·9
40	49·0	9·0
39·9	32·1	7·8
39·8	47·2	7·4
39·3	32·1	7·2
39·2	50·0	10·8
40·8	32·9	7·9
40·2	50·1	9·9
40·0	31·2	8·8
39·8	51·0	11·2
42·1	32·3	10·1
42·0	52·1	10·1

Mean ..... 8·9
Mean of first eight obs. ..... 8·4
Mean of last eight obs. ..... 9·5

The same after two hours without zinc being cleaned.

Zero.	Reading.	Deflection.
1037·0	1027·0	10·0
37·0	48·1	11·1
37·0	25·2	11·8
36·9	47·6	10·7
35·8	25·0	10·8
35·0	46·4	11·4

Mean ..... 11·0

Zinc and copper connected by distilled water at 45° C. cooling during experiment to 20° C. Zinc negative to copper.

Zero.	Reading.	Deflection.
1037·2	1054·0	16·8
37·1	21·5	15·6
37·1	51·2	14·1
37·1	22·8	14·3
37·2	49·2	12·0
..	..	..
37·0	54·0	17·0
36·8	20·2	16·6
35·2	51·0	15·8

Mean ..... 15·3

In the experiments of Table VI the liquids were contained in small flat vessels which were placed below the metal plates near the middle

of the table, strips of copper and zinc dipping into the vessel from the binding screws; these strips may have been of slightly different material from the plates, that is, we are not quite certain that they were cut from the same sheets. In all subsequent experiments, the strips of metal in contact with the liquids were cut from the plates themselves.

TABLE VII.—13th March, 1876. Plates 10 mm. apart.

Zinc and copper plates connected by the acid solution of zinc sulphate of Table VI at 17° C. Zinc negative to copper.

Zero.	Reading.	Deflection.
1068·0	1085·9	17·9
68·4	62·8	5·6
65·8	74·8	9·0
66·1	58·5	7·6
67·1	55·2	11·9
67·1	72·0	4·9
	Cleaned the zinc strip.	
63·0	50·0	13·0
61·5	70·0	8·5
61·1	48·5	12·6
62·0	71·0	9·0
63·3	53·0	10·3
64·8	72·6	7·8

Mean..... 9·8

Zinc and copper plates connected by dilute sulphuric acid. Zinc negative to copper.

Zero.	Reading.	Deflection.
1069·5	1057·1	12·4
68·8	76·4	7·6
68·2	62·0	6·2
68·0	73·9	5·9
68·2	62·2	6·0
68·0	73·2	5·2
67·0	61·9	5·1
66·2	68·9	2·7
	Cleaned the zinc strip.	
61·3	51·0	10·3
62·1	70·1	8·0
63·0	55·0	8·0
63·4	68·0	4·6
63·0	56·0	7·0
62·2	67·3	5·1

The mean deflection for zinc and copper contact at 10° C. being 37·1.

It is to be observed that although there was a motion of the electrometer needle when the quadrants were insulated (shown by the great difference between every two successive readings), still the observations are sufficiently consistent, and when taken two by two they show that polarization has the effect of decreasing the readings. Taking the means of every two successive readings we have in the zinc sulphate, 11·75, 8·3, 8·4, and again, after the zinc is cleaned, 10·75, 10·8, 9·0. In the acid this effect is even more marked, the deflections diminishing rapidly from 10·0 to 3·9, and again from 9·15 to 6·0. Thus we see that polarization in the ordinary sense, so far from producing a difference of potentials between zinc and copper joined by a liquid, in reality tends to reduce the difference of potentials that already existed between them.

The electrometer now began to show the existence of two faults, imperfect contact between the electrodes and the quadrants, and a slight discharge from the needle to the quadrants. The instrument was, therefore, taken to pieces, and the first fault rectified by the addition of contact springs (in addition to the little weights previously employed) between the electrodes and quadrants, and the second by the needle and quadrants being cleaned.

In observations made during the last four months on the constancy of electromotive force of cells, we found that Latimer Clark's cell when tested by an electrometer (kept in a perfectly constant state) altered very little when its poles were kept insulated. The electromotive force of this cell is known to be 1·457 volts, and we have used it as a standard to determine the changes produced in readings by changes in the positions of the quadrants.

When we compared the electromotive force of contact of zinc and copper with the constant cell, it was found in every case to be 0·75 volt at the temperature of 17° C. The deflections in the last two tables might be reduced to volts if we knew the contact electromotive force of zinc and copper at 10° C.

Table VIII.—13th April, 1876. Plates 10 mm. apart.

Latimer Clark's Cell.

Zero.	Reading.	Deflection.
955 ·0	892 ·0	63 ·0
954 ·5	1018 ·5	64 ·0
954 ·5	891 ·8	62 ·7
953 ·1	1017 ·1	64 ·0

Mean..... 63 ·4

Assumed to be 1 ·457 volts.

Direct reading is 355

Therefore *ratio* is $\frac{355}{63 \cdot 4}$ or 5 ·6.

Zinc and copper connected by distilled water at 17° C. Zinc is negative to copper.

Zero.	Reading.	Deflection.
953	960 ·2	7 ·2
952	947 ·0	5 ·0
952	960 ·0	8 ·0
952	946 ·5	5 ·5
951 ·9	961 ·0	9 ·1
952	945 ·0	7 ·0
952	961 ·0	9 ·0
952 ·9	946 ·2	6 ·7
	An interval of 15 minutes.	
953	961 ·0	8 ·0
952 ·8	945 ·1	7 ·7

Mean..... 7 ·32 or 0 ·168 volt.

Zinc and copper metallically connected. Zinc positive to copper.

Zero.	Reading.	Deflection.
953 ·0	926 ·0	27 ·0
952 ·7	990 ·0	37 ·3
951 ·0	920 ·3	30 ·7
950 ·1	985 ·1	35 ·0
950 ·0	919 ·5	30 ·5
950 ·2	984 ·6	34 ·4
951 ·0	918 ·0	33 ·0
951 ·1	985 ·2	34 ·1

Mean..... 32 ·7 or 0 ·751 volt.

Zinc and copper connected by saturated pure zinc sulphate at 17° C. Zinc negative to copper.

Zero.	Reading.	Deflection.
952 ·0	961 ·5	9 ·5
951 ·9	944 ·2	7 ·7
951 ·8	960 ·0	8 ·2
951 ·9	943 ·1	8 ·8
952 ·0	960 ·0	8 ·0
952 ·1	944 ·6	7 ·5

Interval of 10 minutes.

Zero.	Reading.	Deflection.
953 ·1	960 ·0	6 ·9
953 ·2	945 ·0	8 ·2
953 ·1	961 ·2	8 ·1
953 ·3	945 ·2	8 ·1
953 ·7	961 ·0	7 ·3
953 ·9	945 ·2	8 ·7

Mean of first six..... 8 ·3 or 0 ·191 volt.
Mean of last six..... 7 ·9 or 0 ·182 volt.

Table IX.—14th April, 1876. Plates 10 mm. apart.

Zinc and very dilute zinc sulphate (17° C.) in contact. Zinc negative to liquid.

Zero.	Reading.	Deflection.
1066 ·0	1073 ·1	7 ·1
1065 ·3	1057 ·3	8 ·3
1065 ·1	1072 ·3	7 ·2
1065 ·1	1057 ·0	8 ·1

Mean..... 7 ·7 or 0 ·177 volt.

Copper and very dilute zinc sulphate (17° C.) in contact. Copper is positive to liquid.

Zero.	Reading.	Deflection.
1060 ·0	1060 ·9	0 ·9
1060 ·0	1058 ·8	1 ·2

Mean..... 1 ·35 or 0 ·024 volt.

Slightly impure copper sulphate (at 22°·3 C.) and very dilute zinc sulphate (17° C.) in contact. Copper sulphate is positive.

Zero.	Reading.	Deflection.
1058 ·0	1055 ·8	2 ·2
1058 ·4	1061 ·7	3 ·3

Mean..... 2 ·75 or 0 ·066 volt.

Copper and slightly impure copper sulphate (27°·3 C.) in contact.

No perceptible deflection.

Dipped copper in the above copper sulphate and zinc in the zinc sulphate and connected directly with electrometer.

Deflection 246 or 1 ·010 volts.

We were subsequently rather sorry that so few observations were

made for each contact on the 14th of April; as the solutions were impure it did not at the time seem to be specially important to make the experiments with greater accuracy.

The insulated plates *A* and *B* were now cut, as they had hitherto been rather near the sides of the porcelain vessels. It was not until the 21st April that we could get a sufficiently large supply of pure saturated solutions of zinc and copper sulphates. On Friday evening, 21st April, we made our largest and most consistent series of observations. At no time since it arrived in Japan had the electrometer been in such good working order, and, therefore, we shall take the results of this evening, when possible, in preference to any others.

TABLE X.—21st April, 1876. Plates 10 mm. apart.

Latimer Clark's Cell.

Zero.	Reading.	Deflection.
1079 ·0	1019 ·5	59 ·5
72 ·1	1131 ·5	59 ·4
73 ·1	1014 ·0	59 ·1
74 ·7	1135 ·0	60 ·3
79 ·1	1020 ·6	58 ·5
83 ·5	1143 ·7	60 ·2
85 ·8	1026 ·5	59 ·3
86 ·0	1145 ·5	59 ·5
87 ·6	1028 ·3	59 ·3
86 ·7	1145 ·5	58 ·8

Mean..... 59 ·4
Assumed to be 1 ·457 volts.
Direct reading 313
Therefore ratio is $\frac{313}{59 \cdot 4}$ or 5 ·27.

Zinc and copper metallically connected. Zinc positive to copper.

Zero.	Reading.	Deflection.
1085 ·8	1054 ·0	31 ·8
86 ·2	1116 ·4	30 ·2
86 ·0	1054 ·6	31 ·4
85 ·8	1115 ·2	29 ·4
86 ·0	1053 ·9	32 ·1
85 ·6	1115 ·5	29 ·9
86 ·5	1056 ·5	30 ·0
87 ·2	1116 ·6	29 ·4

Mean.....30 ·52 or 0 ·749 volt.

Zinc and nearly pure saturated zinc sulphate (16° C.). Zinc negative to liquid.

Zero.	Reading.	Deflection.
1091·0	1104·7	13·7
88·6	1075·0	13·6
87·5	1100·2	12·7
86·7	1073·5	13·2
..	..	..
90·7	1075·2	15·5
91·2	1106·2	15·0
	Cleaned zinc strip.	
91·8	1075·1	16·7
91·0	1105·0	14·0
90·1	1072·1	18·0
90·3	1104·0	13·7

Mean..... 14·61 or 0·358 volt.

In the first four reversals the distance was a little too great.

Copper and nearly pure saturated zinc sulphate (16° C.). Copper negative to liquid.

Zero.	Reading.	Deflection.
1079·2	1075·2	4·0
78·2	80·2	2·0
78·7	74·5	4·2
79·2	83·2	4·9
80·5	74·8	5·7
80·0	84·5	4·5
80·0	74·1	5·9
82·0	86·0	4·0
81·8	74·6	7·2
81·2	85·8	4·6

Mean..... 4·61 or 0·113 volt.

Copper and distilled water (16° C.). Copper is positive to liquid.

Zero.	Reading.	Deflection.
1083·1	1081·0	2·1
82·8	86·0	3·2
82·9	79·6	3·3

Zero.	Reading.	Deflection.
1082 ·5	1084 ·2	1 ·7
82 ·6	79 ·2	3 ·4
82 ·2	85 ·7	3 ·5
82 ·5	78 ·9	3 ·6
83 ·2	86 ·0	2 ·8
83 ·4	79 ·8	3 ·6
83 ·9	86 ·7	2 ·8

Mean.....3 ·0 or 0 ·074 volt.

Zinc and distilled water (16° C.). Zinc negative to water.

Zero.	Reading.	Deflection.
1083 ·0	1086 ·7	3 ·7
82 ·0	75 ·2	6 ·8
82 ·8	86 ·0	3 ·2
82 ·8	77 ·9	4 ·9
82 ·6	86 ·6	4 ·0
82 ·3	75 ·1	7 ·2
81 ·6	85 ·8	4 ·2
82 ·0	75 ·5	6 ·5
82 ·0	86 ·9	4 ·9
82 ·3	76 ·2	6 ·1

Mean.....5 ·15 or 0 ·126 volt.

Copper and pure saturated copper sulphate (16° C.). Copper positive to liquid.

Zero.	Reading.	Deflection.
1085 ·0	1085 ·1	0 ·1
85 ·1	83 ·1	2 ·0
85 ·0	85 ·7	0 ·7
84 ·8	83 ·0	1 ·8
85 ·0	85 ·1	0 ·1
85 ·0	82 ·5	2 ·5
84 ·9	85 ·0	0 ·1
84 ·7	82 ·5	2 ·2
83 ·7	84 ·1	0 ·4
83 ·1	81 ·5	1 ·6

Mean.....1 ·15 or 0 ·028 volt.

Nearly pure saturated zinc sulphate and pure saturated copper sulphate. Zinc sulphate is positive.

Zero.	Reading.	Deflection.
1083·0	1080·1	2·9
81·9	82·2	0·3
82·2	80·1	2·1
81·7	82·9	1·2
82·7	80·7	2·0
82·8	83·1	0·3
82·5	80·9	1·6
83·1	83·1	0·0
82·6	80·8	1·8
82·7	83·5	0·8

Mean.....1·3 or 0·033 volt.

Distilled water and pure saturated copper sulphate (16° C.). Copper sulphate is positive.

Zero.	Reading.	Deflection.
1081·6	1077·2	4·4
82·1	84·7	2·6
82·0	78·9	3·1
82·2	84·9	2·7
83·0	80·0	3·0
83·5	86·0	2·5
84·0	80·8	3·2
83·0	85·1	2·1
82·5	79·8	2·7
82·3	84·8	2·5

Mean.....2·9 or 0·071 volt.

The sulphates and metals of Table X as a Daniell, connected directly to electrometer 1·068 to 1·081 volts, increasing very slowly.

Simple cell with above zinc sulphate, 1,000 volts. An instantaneous and constant deflection.

Simple cell with distilled water. A rapid increase in electromotive force from the instant of contact of the metals with the liquid. Our first reading was 0·832 volt, but there was a rapid increase when the reading was being taken. 0·942 was our last reading, but there was still a slow increase.

The above copper sulphate and distilled water and the metals as a Daniell. 1·060 to 1·010 volts, decreasing slowly. The same cell tried again, the metals being clean, gave 0·960 to 0·951 volt.

TABLE XI.—Some Collected Results.

	Zinc.	Copper.	Saturated pure copper sulphate.	Saturated nearly pure zinc sulphate.	Slightly impure copper sulphate.	Very dilute zinc sulphate.	Distilled water.	Ordinary water.
Zinc	..	−0·75	..	0·358	..	0·177	0·126	0·118
Copper	0·75	..	−0·028	0·113	0	−0·024	−0·074	
Saturated pure copper sulphate	..	0·028	..	0·033	..	..	−0·071	
Saturated nearly pure zinc sulphate	−0·358	−0·113	−0·033					
Slightly impure copper sulphate	..	0	..	..	..	−0·063		
Very dilute zinc sulphate	−0·177	0·024	..	..	0·063			
Distilled water	−0·126	0·074	0·071					
Ordinary water	−0·118							

A number of results have been collected in Table XI. The numbers in a vertical column below the name of a substance are the differences of potential in volts between that substance and the substance in the same horizontal row as the number, the two substances being in contact. Thus zinc is negative to saturated pure zinc sulphate, the electromotive force of contact being 0·358 volt.

IV.

From Tables VI, VII, and VIII, we see that when zinc and copper are connected by an electrolyte the zinc becomes negative to the copper, the difference of potential seeming to be somewhat less than 0·2 volt for water, zinc sulphate, and dilute sulphuric acid. This difference of potential added to the electromotive force of contact of zinc and copper ought to be equal to the electromotive force of a simple cell.

	From Table VIII.	$\overline{ZC}$.	Sum.	Observed EMF of cell.
Pure saturated zinc sulphate.	0·191 to 0·182	0·75	0·941 to 0·932	0·907, increasing to 0·926, and still increasing slowly.
Pure water	0·168	0·75	0·918	0·832, increasing to 0·942, and still increasing slowly.

The changes which occur in the electromotive force of a cell are difficult to explain. They may be due to impurities in the metals, to air condensed at metallic surfaces, to air and other gases dissolved in the liquid, &c. They seem to be quite arbitrary, and they are often very considerable. The authors have begun an investigation regarding the causes of these peculiar effects.

The discrepancies observable in the following comparisons may also be due to the smallness of the electromotive forces of contact which were measured separately, but they are in some part due to the unexplained changes which occur in the electromotive force of a cell. Let C, Z, and L represent the copper, zinc, and liquid respectively of a simple cell; let L_1 and L_2 be the liquid in contact with the copper, and the liquid in contact with the zinc of a Daniell's cell; let $\overline{CL}$ be the electromotive force of contact of C and L, and let $\overline{CL}$ be identical with $-\overline{LC}$. Taking the five cells of Table XI, and assuming that the electromotive force of a contact of two substances as it exists in a cell is equal to its electromotive force when measured in our apparatus, we get the following results:—

I. Daniell with pure saturated copper sulphate and nearly pure saturated zinc sulphate. Observed EMF of Cell.

$$\overline{CL_1}+\overline{L_1L_2}+\overline{L_2Z}+\overline{ZC}$$
$$=0\cdot028-0\cdot033+0\cdot358+0\cdot750=1\cdot103$$ 1·068 to 1·081, increasing slowly.

II. Daniell with distilled water and pure saturated copper sulphate.

$$\overline{CL_1}+\overline{L_1L_2}+\overline{L_2Z}+\overline{ZC}$$
$$=0\cdot028+0\cdot071+0\cdot126+0\cdot750=0\cdot975$$ 0·995 mean from Table X.

III. Daniell with very dilute zinc sulphate and slightly impure saturated copper sulphate.

$$\overline{CL_1}+\overline{L_1L_2}+\overline{L_2Z}+\overline{ZC}$$
$$=0+0\cdot063+0\cdot177+0\cdot750=0\cdot990$$ 1·010 Table IX.

IV. Simple cell, nearly pure saturated zinc sulphate.

$$\overline{CL}+\overline{LZ}+\overline{ZC}$$
$$=-0\cdot113+0\cdot358+0\cdot750=0\cdot995$$ 1·000

V. Simple cell, distilled water.

$$\overline{CL}+\overline{LZ}+\overline{ZC}$$
$$=0\cdot074+0\cdot126+0\cdot750=0\cdot950$$ 0·832 to 0·942 increasing slowly.

In every case the sum of the separate contact electromotive forces is so nearly equal to the observed maximum electromotive force of the

cell, that we have good reason for believing that the electromotive force of contact of any two substances is constant whatever other substances may be in the circuit. At the same time it becomes evident that there cannot be a very great difference of potential between a metallic or liquid surface and the air in contact with it.

That the electromotive forces of a zinc sulphate cell and of a water cell are so nearly equal seems to be rather accidental. Zinc sulphate is positive to zinc and is less positive to copper, its contact electromotive forces having to be subtracted from one another; whereas water is not so positive to zinc but is negative to copper, the contact electromotive forces being therefore added together.

In any complete conducting circuit (receiving no energy from without) composed of the substances A, B,.... N, the total electromotive force is equal to $\overline{AB}+\overline{BC}+....+\overline{NA}$. Now when no changes are occurring in the circuit such as will produce a supply of energy, we know from the law of conservation of energy that this total electromotive force is nothing. In a compound metallic circuit, which is everywhere of the same temperature, this condition is satisfied. But it is not satisfied if the temperature varies in the circuit, and we find then that the total electromotive force $\overline{AB}+\overline{BC}+....\overline{NA}$ is no longer nothing, its amount depending on the nature of the metals and on the distribution of temperature. Again, we find that even when all parts of the circuit are at the same temperature, and one or more of the substances are liquid or solid electrolytic conductors, the sum of the electromotive force of contact is not nothing, its amount depending altogether on the nature of the supply of energy which exists in the circuit, and therefore on the chemical natures of the electrolytes and of the metals in contact with the electrolytes. In fact, the electromotive force of a voltaic arrangement depends on the chemical natures of the metals and of the electrolytes.

Part II.

"On a Metallic Voltaic Cell, and on Electrolytes of Great Resistance."

When any two pure insulated substances A and B are placed in contact with one another, the electromotive force of contact $\overline{AB}$ charges them to a certain difference of potentials which is numerically equal to $\overline{AB}$, and these charges of electricity represent a certain amount of energy of which the equivalent is a definite amount of chemical action. If the substances are pure there is no other electromotive force that can disturb this establishment of charge. It is probable that the chemical action which might occur at the surface of contact is very

little diminished by this charging, and we have no right to say that the total amount of possible chemical action at the contact of two metallic surfaces is simply equivalent to the energy of the charges. When A and B are solid their rigidity limits the possible amount of chemical action; but even when they are both metals if one is a liquid the total amount may be very considerable. With metals there is a limit produced by the want of mobility of the alloy, and when the liquid is not metallic there is a limit produced by the compound being insoluble, and so preventing free contact. After the charges are established the electromotive force of contact is exactly balanced, and there is no further action of any kind. If, however, A or B be a non-conductor, this establishment of charge may take a considerable time. In any case there can only be an exceedingly small amount of chemical action between any two *pure* substances, as for example, pure zinc and pure sulphuric acid.

If A, B, and C be any three substances insulated from other bodies, A and C being each in contact with B, but not in contact with each other, the electromotive forces of contact $\overline{AB}$ and $\overline{BC}$ charge the three bodies in such a way that the difference of potentials between A and B is equal and opposite to $\overline{AB}$, and the difference of potentials between B and C is equal and opposite to $\overline{BC}$. The energy required to produce this charging of the bodies is due to chemical action at the places of contact. After the three bodies are so charged there cannot be any further chemical action, and the total amount of possible chemical action at these places of contact is very little diminished by the charging.

Primâ facie we cannot compare the present difference of potentials between A and C with the electromotive force of contact that would exist between A and C were they brought together.

The establishment of the above differences of potential takes a shorter or longer time, as the bodies are more or less conducting.

If now we imagine A and C to be brought into contact, as we do not know the value of $\overline{AC}$, we cannot predict the amount of disturbance produced in the charges of A, B, and C; but we may assume that some disturbance is produced, that is, a current of electricity flows in the circuit. We can also say that after the possible chemical energy at the various junctions is exhausted, as well as the potential energy equivalent to bringing A and C in contact, then, if the temperature of the circuit is everywhere the same, there can be no further current, since there is no further supply of energy.

It is known that in a compound *solid* metallic circuit, which is everywhere at the same temperature, there is no continuous chemical action at the junctions, and consequently by the law of conservation of energy there cannot be a continuous current. Hence it has been usual

to assume that if A, B, C of our second example are metals, solid or liquid, the difference of potentials between A and C just before being brought into contact is equal to $\overline{CA}$. Now in the case of one of the metals, B, being a liquid, we have by the experiment described further on shown this assumption to be untrue; and we believe that even when the three metals are all solid, if one or more of them were rather non-conducting, and if the surfaces of contact were large, there would be obtained a result similar to that which we have obtained with solid electrolytes of high resistance. In the case of three ordinary solid metals of high conductivity, the experiment would be very difficult, if not impossible, to perform owing to the previous exhaustion of the possible chemical action through want of perfect insulation of A and C from all substances other than B before being themselves put in contact. The experiments now about to be described, as well as those given in our last paper, lead us to the conclusion that in *all cases* when A and C are brought into contact there will be a disturbance, in some cases, no doubt, only momentary, in the previous charges of A, B, and C.

A Metallic Voltaic Cell.

It may be known that when rods of zinc and copper are placed in mercury and connected with an electrometer no charge is observed. If known, this may have been regarded as a crucial test of the truth of the common theory of compound metallic circuits. Whether the zinc and copper are in contact outside the mercury or not, the amalgamation of the zinc appears to proceed at the same rate. Now, it seemed to us that in this case the impurities and great conductivity of the zinc with the great liquidity of the amalgam, and the close proximity of foreign particles to pure metal, caused the amalgamation to be produced by local action alone, so that the supply of availabl chemical energy for the production of a current from the zinc to copper was exceedingly small; and we have no doubt that at low enough temperatures, when the amalgam loses its liquidity, such an arrangement becomes a simple voltaic cell. It was necessary for us to use instead of zinc a metal of which the amalgam is nearly solid at ordinary temperatures. On enquiry we found that magnesium was such a metal, and on the first trial we obtained a result corroborative of the above theory.

Strips of platinum and magnesium, metallically attached to the electrodes of the electrometer, were dipped into mercury which was, perhaps, slightly impure from the presence of other metals, but which had previously been washed with distilled water, and then well dried. There was a sudden large deflection, the amount of which fluctuated very much afterwards, but which was always considerable and on the same side of the zero. On successive reversals of the electrometer

key the deflections to right and left of zero were found to be nearly equal to one another. In a short time there seemed to be an increased steadiness in the amount of deflection. When the platinum and magnesium were short-circuited for some minutes, the deflection had decidedly increased in amount on renewed insulation, and this occurred after every short-circuiting. To determine the electromotive force of the arrangement, strips of platinum and magnesium, scraped very clean, were dipped into pure mercury which had previously been washed with distilled water, and well dried. The maximum electromotive force obtained was 1·56 volts, that is about one and a half times the electromotive force of a Daniell's cell, the platinum corresponding with the copper plate in the Daniell's cell. It may be possible by mechanical or other means, or by using another metal than magnesium, to give constancy to this arrangement; and as its internal resistance is extremely small the cell may be of great practical use for the production of powerful currents. It may be remarked that as an amalgam may be easily separated into its components by distillation, such a cell by suitable arrangements can be kept in action for an indefinite time.

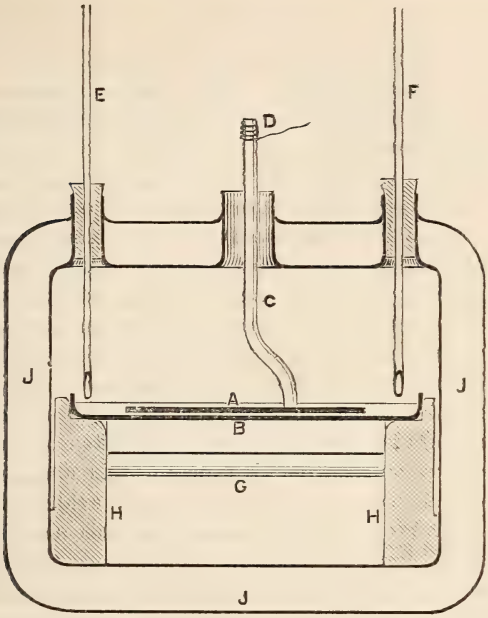
We have examined a circuit composed of the metals platinum, tin, and lead, and the coil of a galvanometer. On raising the temperature of the tin gradually we found that there was a current, and that it gradually increased. Rapid changes in the current were observable at the melting point of the tin, and there seemed to be a small electromotive force independent of thermo-electric effects. When the platinum and lead strips were connected with the electrometer, the deflections were found to be too small for decided conclusions to be drawn. We shall proceed, when time allows us, with the investigation of metallic voltaic cells.

On Electrolytes of Great Resistance.

To measure the electromotive force of the voltaic cell formed by plates of zinc and copper separated by paraffin wax and other electrolytes of great resistance, and to determine the change of conductivity of such electrolytes with temperature, we used the apparatus shown in the figure.

J is an oil-bath which may be clamped firmly to an iron stand resting on the table of such a height that D is on a level with the electrode of the electrometer. The bath was heated by means of spirit-lamps. No artificial means of cooling the bath were employed. B is a shallow dish of copper, quite plane on the bottom inner surface, and containing the electrolyte. A is a circular plate of zinc, or of copper, 12·8 centims. diameter, resting in the experiment in paraffin wax on three small pieces of glass, 0·191 centim. thick; the metal was very clean, and was quite surrounded by paraffin wax poured in in a melted state. The strip C is in one piece with the plate A, both

being cut from the same piece of metal; C is bent upwards and passes through an orifice in the bath. The wire from D to the insulated electrode of the electrometer is protected by a wide tube of metal;



this tube, the outside of the electrometer, and the uninsulated electrode being connected with B. A double door (not shown in the figure) gives access to the inside of the bath. HGH is a wooden support which may be withdrawn from the bath. E and F are thermometers of which the bulbs are near the surface of the paraffin. The difficulty of determining exactly the temperature of the paraffin wax arose from its non-conductivity; and the use of a third thermometer, passing through a tubulure horizontally underneath B, was found to add nothing to the accuracy of the temperature readings.

To obtain continuous time readings of the electrometer, we let the image of the cross fibres of our lamp slit formed by the mirror fall on a horizontal wooden roller covered with paper. This roller was 100 centims. long, and 13 centims. in diameter. A clock causes the roller to make a revolution in two minutes. We are able suddenly to change the rate of this roller to one revolution in 7·2 minutes, and we can also slightly vary the rate by altering the length of the pendulum. The image of the cross fibres is easily followed in its motion on the paper by a pencil. When the paper is cut off at any place parallel to the roller-axis it exhibits the time observations in a compact form well adapted for calculation. The clock being stopped and a scale attached

to the roller, readings may be made in the ordinary way. Such a roller and clock costs very little, and form a valuable addition to every reflecting galvanometer or electrometer.

Paraffin Wax.

The plate A being zinc and connected with the insulated electrometer-electrode, and the paraffin being solid between the two plates, we first short-circuited the arrangement to get zero, then insulated and allowed the electromotive force of the paraffin cell to charge plate A, and the electrometer quadrants in connexion with it.

A detailed account of our experiments on conductivity is given further on. It is sufficent here to state that at low temperatures the conductivity of paraffin wax is exceedingly small, the conductivity increases to the melting point, the increase being extremely rapid during melting, and there seems to be a regular increase during the melted state. Hence in the experiments with the zinc plate at low temperatures, the resistance is so great that although there is a slight charging of the quadrants due to electromotive force, this is somewhat disguised by defects of the electrometer. In fact, at low temperatures, it is impossible to measure the electromotive force of this simple cell, although the existence of an electromotive force is quite evident, but when the conductivity becomes sufficiently great, and this occurs long before there is any appearance of melting on the upper surface of the paraffin, the charge reaches a maximum with sufficient rapidity for its accurate measurement. At a few degrees above the melting point the maximum charge established itself at once, and however often the plates were short-circuited for the purpose of taking the zero, the maximum charge was found to be approximately the same, and to correspond with an electromotive force of 0·73 volt when least, and 0·75 volt when greatest: it was also not much affected by change of temperature. To make these measurements we used as a standard a Latimer Clark's constant cell.*

When the plates are charged to their maximum difference of potentials there is electric equilibrium, and no further current tends to pass through the arrangement, the electromotive force being balanced. If then E_0 represents this maximum charge, and if E_1 is the charge at a time (t_1), and E_2 the charge at the time t_2, then—

$$\frac{1}{t_2-t_1}\log\frac{E_0-E_1}{E_0-E_2}$$

multiplied by a constant depending on the electric capacity of the arrangement represents the conductivity. As the measurement of any particular temperature is rather coarse, we cannot readily compare

* In our last paper we gave 0·75 volt as the correct measurement of the electromotive force of contact of zinc and copper.

this conductivity with the conductivity for that temperature which we have otherwise determined, but we infer from the curve that the above formula gives a result twice or three times as great as the true conductivity: this, however, might have been expected from our knowledge of the phenomena of absorption in dielectrics.

Sir W. Thomson, in the "Proceedings of the Royal Society" for June 10th, 1875, describes an experiment in which plates of zinc and copper, in contact with the two sides of a plate of glass, form a voltaic cell. When the glass was sufficiently heated to become conducting there was evident charging of the plates.* In the glass cell the maximum charge became less and less after every short-circuiting, whereas in the paraffin cell the maximum charge is constant. In the one case all the elements of the circuit were solid, the glass being very far indeed from its liquefying point, so that the amount of possible chemical action at the junctions was small, and after every successive short-circuiting, there was a less and less supply until the arrangement was approaching a state which had hitherto been supposed to be peculiar to a compound metallic circuit; whereas our paraffin possessed mobility, and the amount of possible chemical action at the surfaces of contact was practically infinite in comparison with the currents charging the plates.

It is stated above that when any two pure substances otherwise insulated are brought into contact with one another, the amount of chemical action must be practically *nil*, but, as perfectly pure substances do not exist, when two bodies are brought into contact small complete circuits are formed by three or more different substances, and currents are produced until the possible chemical actions at the surfaces of contact are exhausted. Now if one or more of these substances in such elementary circuits are non-conductors, this exhaustion can only proceed very slowly, and if the substances at the surfaces of contact have immobility, the total amount of possible chemical action will be necessarily small. Hence we see that the amount of chemical action between bodies depends upon their conductivity and mobility, as well as on the electromotive forces of contact, and as the chemical action equivalent to the energy of small charges of electricity is quite incapable of being detected by the ordinary methods of chemical analysis, and as the charges are easily measurable with an electrometer, we have, if our theory be correct, in electrical measurement a method of chemical analysis which is infinitely more sensitive than any purely chemical method. Thus it is evident that in our paraffin cell there is chemical action, but our colleague, Dr. Divers, has found no traces of zinc in English paraffin oil which came out to Japan in galvanized iron cases, nor in paraffin oil heated to a temperature of 60° C., in a

* Our experiments of the increase of conductivity of glass with temperature are described in the "Proceedings of the Royal Society" for the same date.

zinc vessel for many hours. In fact in chemical analysis, something which is equivalent to current is measured by a method very much less sensitive than if the measurement were made by the best galvanometer, whereas a current which is not observable with the best galvanometer may be measured accurately with the electrometer.

It is plain that if we assume an electromotive force of contact between any two substances, there must be possible chemical action between these two substances, and this chemical action can of course only exist at the places of contact. It may proceed slowly through want of conductivity, and the amount possible in any given case may be very small because of want of mobility of either of the substances in contact, or of want of mobility of the compound formed by the action. Thus it is well known that the electromotive force of contact of glass and other substances is often very considerable, and that the chemical action between glass and most other substances is very small. This is due to want of conductivity of the glass, but when glass becomes heated, its conductivity much increases, and it then becomes capable of acting chemically on many substances. When in the melted state it is acted on chemically by the majority of substances. It is possible that this great increase of chemical activity accompanying a rise of temperature is much more due to increased conductivity than to change of electromotive force of contact. Lead glass which insulates so badly, is comparatively easily acted on by chemicals. Chemical action is always slow unless the substances are conductors, and we find that nearly all the substances which chemists find it unnecessary to dissolve before applying re-agents are conductors. In some cases previous solution may be also required for the mere purpose of producing mobility at the surfaces of contact. To expedite chemical action, solutions are stirred together; this increases the area of contact and diminishes resistance as well as rapidly bringing into contact fresh portions previously unacted on.

The alkaline earths in the solid state are very bad conductors, but when melted they are comparatively good conductors, and there is no doubt that we have here an explanation of the fact that the bottoms come out of platinum crucibles containing melted alkaline earths. We might expect that in those cases in which melted metals have a considerable electromotive force of contact, they would combine to form alloys with great rapidity, on account of their great conductivity and sufficient mobility to continue chemical combination. The great conductivity of mercury, and its mobility and the mobility of the compounds which it forms with other metals, enables it to enter rapidly into combination with those metals with which it has a great electromotive force of contact. And it is probable that the electromotive force of contact of mercury with iron or with steel is exceedingly small, or that the amalgam is a solid except at very high tem-

peratures. Cold glass is acted on by hydrofluoric acid, and this is possible on account of the great electromotive force and on account of the silicon tetrafluoride formed by the combination being volatile, and, therefore, possessing great mobility. Hitherto chemists have only employed the two ideas of chemical affinity and the amount of chemical action, but we have shown that these ideas are simplified when regarded as electromotive force of contact and currents of electricity. To connect the two ideas we have a third, viz., *resistance*, and the electrical law of Ohm becomes the chemical law—*the quantity of chemical action in unit time equals the sum of a great number of terms, each of which is an electromotive force divided by a resistance.*

Gutta-Percha.

The metallic plates of the last experiment were carefully cleaned, and the paraffin wax replaced by a sheet of gutta-percha, previously scraped, washed with soap and water, ordinary water, alcohol, sodium carbonate, and hot distilled water, and thoroughly dried. The upper plate A was of clean zinc as before. Our experiments on the conductivity of gutta-percha, described further on, show that it increases with the temperature, not only at the low temperature at which it has hitherto been examined, but also after it has become plastic at higher temperatures.

At the lowest temperature at which we examined the arrangement there was a decided charging of the plates, but it was so slow that we must regard the measurement of the minimum electromotive force 0·23 volt as being too small, for a careful examination of the defects of the electrometer showed that under these circumstances the measurement *might* be too small. At higher temperatures the maximum charge was established with much greater rapidity.

On each day, some time after the commencement of the experiment for that day, the electromotive force having remained constant for some time suddenly increased, and then remained nearly constant at its higher value. This seemed at first to be due to the plates having been short-circuited for intervals of four minutes or more, but we afterwards found that it occurred when the plates were only short-circuited for a few seconds to get to zero. Also, it did not seem to occur at a particular temperature; on one occasion the sudden rise happened at about 35° C., and on another at about 72° C. On the first of these days the mean low electromotive force was 0·584 volt, and the mean high 0·763 volt, the very lowest and very highest being 0·54 and 0·86 volt. On the second day the mean low was 0·534 volt and the mean high 0·773 volt, the lowest and highest being 0·529 and 0·789 volt. We are sure, when these measurements were made, that the conductivity of the gutta-percha was sufficiently great to eliminate any effects due to the minute defects of the electrometer.

As in the case of paraffin, the conductivity calculated from the time readings, whilst the zinc and copper plates were establishing their charge, is greater than the conductivity measured independently by two copper plates charged with a battery for about five minutes and then kept insulated.

Ebonite.

Between the zinc and copper plates was placed an ebonite sheet, having an average thickness of 0·35 millim. It was chemically clean, and there was no surface conduction at even low temperatures. We found, as in the previous cases, that there was evidence of an electromotive force, but the charging proceeded very slowly until about 36° C., when the conductivity had greatly increased. During thirty-five minutes the temperature steadily increased to 74° C., and as there was no indication of a maximum charge, we let it increase without short-circuiting during this thirty-five minutes, at the end of which time the deflection indicated an electromotive force of 3 volts. After short-circuiting the charge again established itself, but it did not increase as rapidly as it had done at first at the lower temperature, although from our independent experiments we know that the conductivity is greater at higher temperatures. Precautions have been taken to avoid any sliding of the ebonite plates during the experiments; for certain preliminary observations showed that such sliding would produce charges of electricity which would mask mere voltaic results. From the mathematical expressions for conductivity and from our experimental determinations of the conductivity at any given temperature, we endeavoured, by calculation, to discover whether the first set of time readings indicated an approach to a maximum, but we obtained inconsistent results; the maximum electromotive force thus calculated varying from 3 to 12 volts. After the experiments the zinc plate was found to be only slightly dulled in its polish and not coloured, neither did the copper plate appear much changed in appearance, but a lead weight which had been laid on the zinc to keep it in close contact with the ebonite was found to be quite red.

It is evident, therefore, that this arrangement had nothing in common with an ordinary simple cell, the great charging of the plates being due to a decomposition of the ebonite by heat.

India-Rubber.

In November, 1875, we endeavoured to find the electromotive force of a simple cell formed by placing a sheet of india-rubber containing sulphur between zinc and copper plates, and we then obtained results analogous to those which have just been described for ebonite. During the present investigation, we expected to obtain the same results, since we used the piece of india-rubber which had previously been

employed. Contrary to our expectations, however, there appeared to be no further decomposition of the india-rubber by heat, and results analogous to those previously obtained with the paraffin wax were met with. Probably, therefore, all the sulphur that could easily be liberated had been expelled by the previous heating. At about 90° C., the conductivity was sufficient to enable us to measure, with some accuracy, the electromotive force, which we found to be 0·168 volt; but somewhat as in the case of the gutta-percha, between 90° and 100° C., the electromotive force increased to 0·356 volt. This increase did not appear to be as sudden as in the case of gutta-percha, but, as before, the electromotive force was fairly constant on each side of this place of changing.

Shell-Lac.

An extremely thin sheet of shell-lac was placed between plates of zinc and copper, as in the preceding experiments. At low temperatures there was evidently an electromotive force, which, at 28° C., we were able to measure, as the conductivity was sufficiently great. From this temperature upwards there was practically no change. In fact, the electromotive force was constant and equal to 1·12 volts. At temperatures higher than 65° C., the arrangement established its charge after short-circuiting with the rapidity of an ordinary liquid voltaic cell.

Resistances of the above Electrolytes.

It has been usual for telegraph engineers to measure the resistance of gutta-percha, india-rubber, &c., after a current has been passing through the material for one or for not more than three minutes. Now, although this may be a very valuable test of a telegraph cable, since the amount of apparent loss, through want of insulation during the first small period of charge, is one of the elements affecting the received current, still since this loss is due to two causes, soaking in and true conductivity, and since the former is at first so very great compared with the latter, the real resistance can only be ascertained after the current has been on for a length of time. The practical time of charging necessary to enable the current to be a true measure of the conductivity seems to be less at higher temperatures, and in our experiments we have endeavoured to approximate to the elimination of soaking in.

Paraffin-Wax.

In November, 1875, we made a great number of time observations of the difference of potentials of two copper plates immersed in paraffin-wax, and kept asunder by three small pieces of glass, 0·191 centim. thick, the upper plate having been originally charged to a certain potential, and the lower plate kept in connexion with the earth. We

found that the conductivity increased with temperature, the increase being very rapid near the melting point, at which place, as we used no auxiliary condenser, the rate of loss of charge was very high. To measure the conductivity of melted paraffin, we employed a very delicate reflecting astatic galvanometer of 20,000 ohms resistance; one Daniell's cell, through a resistance of 120 megohms, gave a deflection of 131·3 divisions on an ordinary scale, at about a metre distance. The current from a battery having a measured electromotive force 47 times that of our standard Daniell passed through the paraffin and the galvanometer. Full precautions were taken to avoid all leakage except through the paraffin, the deflection of the galvanometer needle due to instrumental leakage alone being quite imperceptible. The thermometers did not indicate truly the temperature of the paraffin, being too high when the apparatus was heating, and too low when it was cooling; but as the cooling was very slow, the results which are given in following table obtained during cooling, and which, when plotted, give the curve JK (fig. 1, p. 231), may be regarded as being fairly correct, for the observations seem to be all very consistent with one another.

The area of the upper plate was 56·25 square centims., and hence if x is the specific resistance in megohms per cub. centim. of paraffin, and if D is the deflection on the galvanometer scale,

$$\text{Log } x = 8\cdot3374 - \log D.$$

Temperature.	D.	x in megohms.
90° ·6 C.	167 ·9	$1\cdot295\times10^{6}$
96° ·8	229 ·9	$0\cdot946\times10^{6}$
91° ·0	191 ·6	$1\cdot135\times10^{6}$
84° ·0	151 ·9	$1\cdot432\times10^{6}$
80° ·0	132 ·3	$1\cdot644\times10^{6}$
76° ·0	115 ·9	$1\cdot876\times10^{6}$
70° ·2	94 ·5	$2\cdot301\times10^{6}$
65° ·1	78 ·0	$2\cdot788\times10^{6}$
60° ·0	64 ·0	$3\cdot398\times10^{6}$
55° ·2	55 ·0	$3\cdot954\times10^{6}$
50° ·0	43 ·0	$5\cdot058\times10^{6}$

In the above table we have given only every fifth observation. The curve ML has for co-ordinates the temperature and galvanometer deflection as the temperature increased. The points M, L, K, J, correspond respectively with the temperatures 90° ·6, 94° ·7, 93° ·1, 48° C. In this and in the other curves we have employed such scales as have seemed to us most convenient. In every case we give the true co-ordinates of some points, and from these the scale may be easily determined. Ordinates measured parallel to the line OX represent temperature, the temperature increasing towards O.

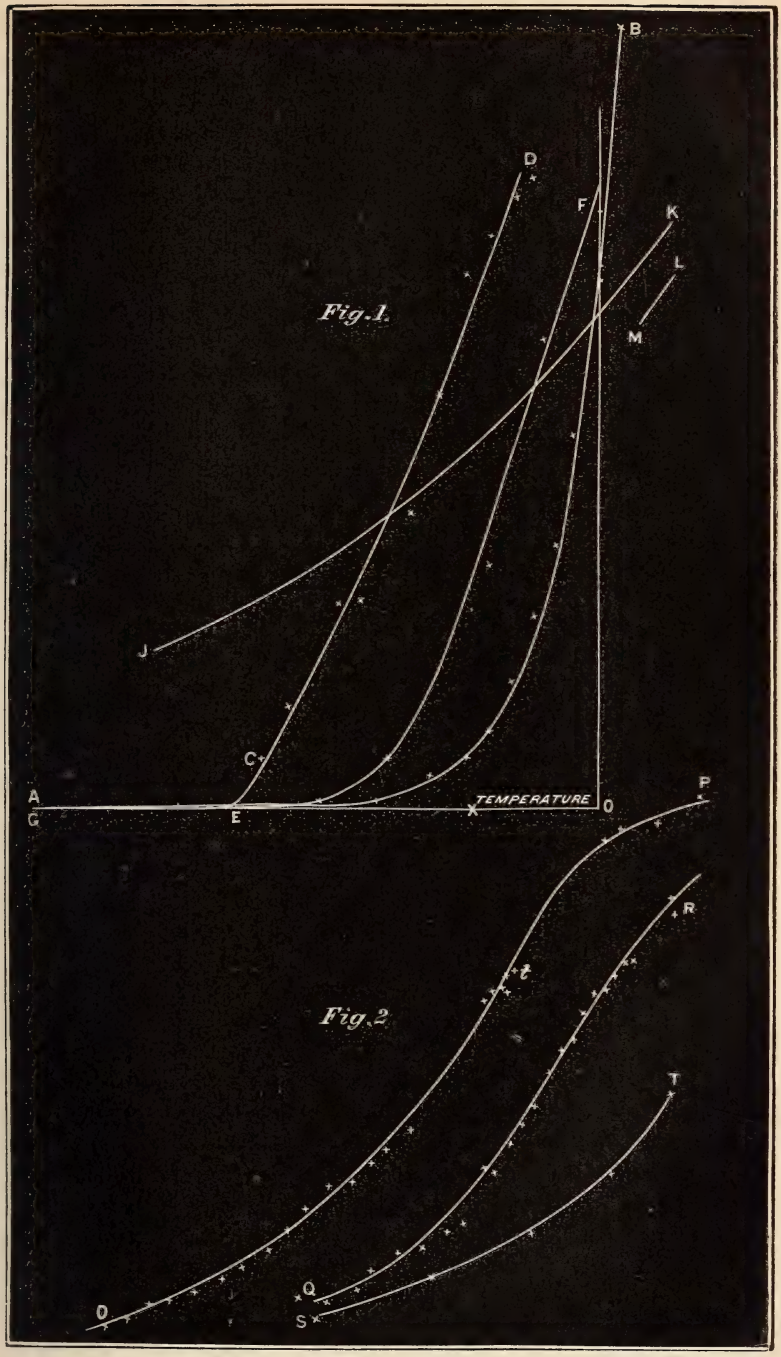


Fig. 1.

Fig. 2

We were unable to obtain galvanometer and electrometer tests at the same temperature, since the galvanometer did not give deflections at low temperatures; and as we had not at this time designed the time roller described above, and as no auxiliary condenser was used with the electrometer, the loss of charge was too rapid at high temperatures to make accurate time observations.

The oil-bath used in November, being small and otherwise inconvenient, we proceeded in May of this year with the investigation, using the apparatus described above. At low temperatures time observations of loss of charge were taken on a fixed scale. Near the melting point time observations were taken on the roller in motion, and at higher temperatures, one terminal of an auxiliary condenser of one-third microfarad capacity and excellent insulation was attached to the plate A, the other terminal of the condenser being connected with B. Four Daniell's cells were used to charge A.

At 20°·9 C., the temperature of the room, A experienced no loss of charge in ten minutes whether or not the condenser was attached.

A number of preliminary experiments showed as before that there was a great increase of conductivity at melting. For instance, on May 15th, when the temperature was rising, the paraffin appeared to be solid at 46° C., and it appeared to be liquid at 52° C., and between these two temperatures we obtained the following results, t being measured in minutes always.

Temperature.	$\frac{1}{t}\log\frac{E_1}{E_2}$	x in megohms.
46° ·5 C.	0 ·00114	34190 $\times 10^6$
48° ·3	0 ·00168	26480 $\times 10^6$
50° ·0	0 ·00225	17320 $\times 10^6$
50° ·4	0 ·00607	6421 $\times 10^6$
50° ·8	0 ·01600	2436 $\times 10^6$
51° ·15	0 ·03970	998 ·2 $\times 10^6$
51° ·2	0 ·07000	556 ·8 $\times 10^6$
51° ·3	0 ·30000	129 ·9 $\times 10^6$
51° ·35	1 ·26000	30 ·93 $\times 10^6$
	Afterwards when cooling.	
48° ·4	0 ·0689	565 ·7 $\times 10^6$
48° ·2	0 ·0308	1266 $\times 10^6$
45° ·1	0 ·0133	2930 $\times 10^6$

These results cannot be compared directly with those previously obtained with the galvanometer, because in the former case the paraffin was cooling from a high temperature, 90° C., whereas in this case the temperature was only raised to 51°, and it is well known that when paraffin is cooled from a high temperature, it will remain liquid

at temperatures at which it would be solid were it being gradually heated from a low temperature.

Curve AB, May 16th, gives the relative conductivities when the paraffin was rising from 54° C. to 56° C. At 54° the paraffin seemed to be solid, and it did not appear to be melted at its upper surface, even for some time afterwards, when the thermometers indicated 60° C. But at 56° C. it was covered with white eruptions. With the condenser, May 16th, we obtained the following tests. As we did not employ the roller the conductivities deduced from detached readings are often inconsistent, and yet each of these conductivities was deduced from a number of observations, in some cases from twenty. This shows the difficulty of making eye observations, for when using the time roller and tracing on it with a pencil the path of the image of the cross fibres, no such inconsistences were met with.

Temperature.	$\frac{1}{t}\log\frac{E_1}{E_2}$	x in megohms.
60° ·2 C.	0 ·1175	$4\cdot482\times10$
The paraffin was quite liquid over the upper plate.		
62°	0 ·0205	$2\cdot569\times10^6$
64° ·5	0 ·0222	$2\cdot372\times10^6$
68° ·5	0 ·0261	$2\cdot018\times10^6$
72° ·5	0 ·0312	$1\cdot689\times10^6$
76° ·6	0 ·0373	$1\cdot412\times10^6$
77° ·8	0 ·0389	$1\cdot354\times10^6$
77° ·4	0 ·0334	$1\cdot577\times10^6$
76° ·4	0 ·0316	$1\cdot666\times10^6$
75° ·3	0 ·0310	$1\cdot699\times10^6$
74°	0 ·0268	$1\cdot736\times10^6$
72° ·4	0 ·0246	$2\cdot141\times10^6$
70° ·7	0 ·0242	$2\cdot177\times10^6$
69° ·6	0 ·0240	$2\cdot195\times10^6$

These results may be compared with the galvanometer results, because in both cases the temperature was cooling from a point much above melting. Considering the great difficulty of measuring the temperature, the agreement of these results with the galvanometer results may be regarded as very satisfactory.

May 20th. Time roller used for the first time, quick motion only employed, that is, one revolution in two minutes. The battery was kept connected with the plates for intervals of time varying from three to ten minutes before plate A was insulated. The roller curves are in themselves extremely interesting, but we think that to give them in the present paper would be superfluous. The curve EF gives the values of $\frac{1}{t}\log\frac{E_1}{E_2}$ without condenser from 52°·6 C. to 58° C.; the co-

ordinates of the points E, a, b, c, &c. . . . F, have the values given in the following table:—

	Temperature.	$\frac{1}{t}\log\frac{E_1}{E_2}$	x in megohms.
	49° ·7 C.	0 ·0044	8857 $\times 10^6$
	51° ·2	0 ·0050	7794 $\times 10^6$
E	52° ·6	0 ·0122	3195 $\times 10^6$
a	54° ·0	0 ·0677	575 ·7 $\times 10^6$
b	54° ·65	0 ·1620	240 ·6 $\times 10^6$
c	55° ·2	0 ·3612	107 ·9 $\times 10^6$
d	57° ·0	1 ·7040	22 ·88 $\times 10^6$
F	58° ·0	3 ·2776	11 ·90 $\times 10^6$
	59°	4 ·1608	9 ·367 $\times 10^6$

Of the curve GCD, the part from G to C was obtained without using the condenser, and for the part from C to D, the condenser was employed. The co-ordinates of the points C, a, b, &c., . . . D are as follows:—

	Temperature.	$\frac{1}{t}\log\frac{E_1}{E_2}$	x in megohms.
C	59° ·8 C.	0 ·0065	8 ·104 $\times 10^6$
a	61° ·5	0 ·0096	5 ·487 $\times 10^6$
b	62° ·85	0 ·0097	5 ·430 $\times 10^6$
c	64° ·4	0 ·0126	4 ·180 $\times 10^6$
d	66° ·45	0 ·0138	3 ·187 $\times 10^6$
e	68° ·5	0 ·0193	2 ·729 $\times 10^6$
f	70° ·2	0 ·0250	2 ·107 $\times 10^6$
g	72° ·2	0 ·0267	1 ·972 $\times 10^6$
h	73° ·8	0 ·0285	1 ·848 $\times 10^6$
D	75° ·2	0 ·0293	1 ·798 $\times 10^6$

Assuming that the paraffin is always in the same state when the thermometer gives the same reading during a regular rise in temperature of the apparatus, we find,

$\frac{1}{t}\log\frac{E_1}{E_2}$ without condenser for 59° C. to be 4 ·36.

$\frac{1}{t}\log\frac{E_1}{E_2}$ with condenser for 59° C. to be 0 ·0059.

Therefore the ratio of the relative conductivities measured without and with the condenser, is about 740, and hence, assuming the specific inductive capacity of paraffin does not change much with temperature, and that the change of capacity of the pair of quadrants can be neglected, we can calculate the specific resistance in megohms of a cubic centimetre of paraffin wax from our time readings by the

following formulæ, which have been employed in obtaining the values of x given above.

Without condenser,

$$\log x = 7\cdot5908 - \log\left(\frac{1}{t}\log\frac{E_1}{E_2}\right),$$

with condenser,

$$\log x = 4\cdot7216 - \log\left(\frac{1}{t}\log\frac{E_1}{E_2}\right),$$

t being as usual measured in minutes.

The changes which occur in the conductivity of paraffin wax when it melts are best seen in the curve AB. It is probable that the curve from E to F represents the continuation of AB to a different scale, and we have found that the further continuation beyond the point F is represented by the curve from C to D, which is again drawn to a different scale.

All the loss of charge observations are combined in the curve GCD, and it is quite evident that there is a decided want of continuity in the curve. It is probable that paraffin follows the logarithmic law until it nears the melting point, but after melting the increase of conductivity would seem to be proportioned to the increase of temperature. The curve CD may be a straight line, but the galvanometer results give a curve which is decidedly concave upwards; it is still, however, a very flat curve.

Solid paraffin during the four degrees before melting alters its specific resistance per cubic centimetre from 34000×10^6 megohms to 1000×10^6 megohms. For the lowest temperature at which it can be said to be melted, its specific resistance seems to be about 10×10^6 megohms, and 77°·8 C., to be pretty accurately $1\cdot354\times10^6$ megohms.

Gutta-Percha.

The effect of heat on the conductivity of gutta-percha was studied by noting the loss of charge of two copper plates separated by a sheet of gutta-percha 0·191 centim. thick. The conductivity even at ordinary temperatures was sufficiently great for the employment of a condenser. The values of $\frac{1}{t}\log\frac{E_1}{E_2}$, are shown in the curve OP (fig. 2), the co-ordinates of the points O, a, b, &c., . . . P of the curve are given in the following table. x, the specific resistance per cubic centimetre in megohms has been calculated from the formula:—

$$\log x = 4\cdot7217 - \log\left(\frac{1}{t}\log\frac{E_1}{E_2}\right).$$

The plastic condition of the gutta-percha, at the higher temperatures has indicated itself in the curve between t and P.

	Temperature.	$\frac{1}{t}\log\frac{E_1}{E_2}$	x in meghoms.
O	83° C.	0·1052	$\cdot5008\times10^6$
a	80°	0·1005	$\cdot5243\times10^6$
b	77°·4	0·0984	$\cdot5355\times10^6$
c	76°·5	0·0964	$\cdot5466\times10^6$
d	70°·7	0·0732	$\cdot7197\times10^6$
e	70°·3	0·0693	$\cdot7603\times10^6$
f	70°·2	0·0723	$\cdot7288\times10^6$
g	70°·0	0·0701	$\cdot7516\times10^6$
h	69°·5	0·0693	$\cdot7603\times10^6$
i	68°·8	0·0676	$\cdot7794\times10^6$
j	64°·4	0·0442	$1\cdot192\times10^6$
k	62°·8	0·0406	$1\cdot298\times10^6$
l	61°·9	0·0381	$1\cdot383\times10^6$
m	60°·6	0·0345	$1\cdot527\times10^6$
n	59°·0	0·0335	$1\cdot573\times10^6$
o	57°·5	0·0290	$1\cdot817\times10^6$
p	56°·4	0·0252	$2\cdot090\times10^6$
q	55°·1	0·0219	$2\cdot406\times10^6$
r	53°·4	0·0180	$2\cdot927\times10^6$
s	52°·0	0·0156	$3\cdot378\times10^6$
t	50°·1	0·0131	$4\cdot022\times10^6$
u	48°·4	0·0112	$4\cdot704\times10^6$
v	47°·0	0·0101	$5\cdot217\times10^6$
w	45°·5	0·0075	$7\cdot025\times10^6$
P	44°·0	0·0059	$8\cdot930\times10^6$

If we may assume that from P downwards in temperature the resistance of gutta-percha follows the well-known logarithmic law, we find at 24° C. the specific resistance per cubic centimetre is 83×10^6 megohms, showing that for a sheet the gutta-percha is fairly good.

Ebonite.

In determining the conductivity of ebonite, we at first proceeded as described above, using a pair of copper plates; but we found that the charge instead of decreasing through conductivity increased apparently indefinitely. On replacing the copper plates by sheets of platinum we obtained consistent results, but this may have been due to the sulphur that could easily be liberated by heat having been driven out in the previous heating. QR is the curve connecting the conductivity, or rather $\frac{1}{t}\log\frac{E_1}{E_2}$, and the temperature. This curve is nearly straight from 65° C. to 96° C., our highest temperature. No rapid change in the conductivity is anywhere perceptible.

As we could not employ the auxiliary condenser in consequence of

the resistance of the ebonite being very great, and the loss, therefore, very slow even at high temperatures, and as the specific inductive capacity of ebonite is unknown, we were compelled to make experiments to determine the electric capacity of our arrangement. This was very difficult on account of the small area, 45·6 square centims., of the ebonite in contact with the platinum. The result, therefore, which we obtained, and from which we deduced the specific inductive capacity of ebonite to be 1·7, must only be accepted as very rough, and merely of use in giving us approximate values of the resistance at various temperatures. The number 1·7 seems too small for a composition of india-rubber and sulphur. Consequently, all our calculated values of x may be too large, and ought to be reduced in the ratio of 1·7 to the true specific inductive capacity of ebonite.

	Temperature.	$\frac{1}{t}\log\frac{E_1}{E_2}$	x in megohms.
Q	36°	0 ·0027	61030×10^6
a	41°	0 ·0026	63390×10^6
b	46°	0 ·0031	53160×10^6
c	48° ·5	0 ·0038	43370×10^6
d	53°	0 ·0045	36630×10^6
e	57°	0 ·0048	34340×10^6
f	61°	0 ·0054	35020×10^6
g	64°	0 ·0057	28920×10^6
h	67°	0 ·0077	21400×10^6
i	68° ·5	0 ·0076	21690×10^6
j	71°	0 ·0086	19160×10^6
k	73°	0 ·0093	21560×10^6
l	75°	0 ·0100	16480×10^6
m	77°	0 ·0112	14720×10^6
n	79°	0 ·0120	13740×10^6
o	80° ·5	0 ·0124	13290×10^6
p	82° ·3	0 ·0133	12400×10^6
q	84°	0 ·0140	11770×10^6
r	85° ·6	0 ·0142	11610×10^6
s	87° ·0	0 ·0147	11210×10^6
t	88° ·6	0 ·0152	10840×10^6
u	90°	0 ·0153	10770×10^6
v	94° ·4	0 ·0175	9419×10^6
R	96° ·8	0 ·0170	9696×10^6

India-Rubber.

The relation of conductivity to temperature of a chemically clean piece of india-rubber containing sulphur is given in the curve ST. As the loss was very slow we could not use the auxiliary condenser, and, therefore, had to measure roughly the capacity of the arrange-

ment, which was found to be 0·000208 microfarad. The area of each of the platinum plates, between which the india-rubber was placed, was 45·6 square centims. The thickness of the india-rubber was 0·106 centim., so that the calculated specific inductive capacity was 4·8. This seems too large; but an error, if there be one, in the determination of the capacity would have made the calculated resistance too small, whereas the resistance is much higher than that hitherto obtained with any of the forms of india-rubber experimented on by electricians. We found that the capacity of the electrometer quadrants was neglectable in these, and in the ebonite observations. We give the temperature and the corresponding values of $\frac{1}{t}\log\frac{E_1}{E_2}$, and of x in megohms for the points S, a, b, c, T of the curve ST.

	Temperature.	$\frac{1}{t}\log\frac{E_1}{E_2}$	x in megohms.
S	67° C.	0·0100	5391×10^6
a	75°	0·0187	2882×10^6
b	81°·6	0·0275	1960×10^6
c	86°·7	0·0390	1382×10^6
T	90°·7	0·0531	1015×10^6

II. "On the Viscosity of Dielectrics." No. I. By W. E. Ayrton and John Perry, Professors in the Imperial College of Engineering, Tokio, Japan. Communicated by Professor Sir W. Thomson, LL.D., F.R.S. Received October 2, 1877.

It is well known that india-rubber has a greater specific resistance and a less specific inductive capacity than gutta-percha. Now a popular explanation of this might be given as follows:—Imagine a portion of the india-rubber in an india-rubber condenser replaced by a metallic or other conducting substance either in a single piece or in the form of grains scattered throughout the mass of the dielectric, then the resistance of the condenser will be diminished while its capacity will be increased. As more and more metal is introduced into the dielectric the capacity will become greater and greater, so that at last, if all the india-rubber be replaced by metal, we ought to consider such a condenser as having an extremely great capacity, although on account of the conduction it would be impossible to experimentally measure the charge. If the above consideration, which of course really applies to a dielectric of heterogeneous constitution, be also applicable to different homogeneous dielectrics, then we might expect to find that for all dielectrics the specific inductive capacity diminishes as the specific resistance increases. Now the experiments we have lately been engaged on concerning specific resistances enable us to prove that in the case of several well-known dielectrics this is really so, as will be seen from the following table:—

Substance.	Specific Inductive Capacity. Air equals 1.	Authority for Capacity.	Approximate resistance per cubic centimetre in ohms after several minutes' electrification.	Temperature, Centigrade.	Authority for Resistance.
Dilute sulphuric acid	700×10^6	C. Varley	Less than 4	24	F. Jenkin.
Mica	5	F. Jenkin	84×10^{12}	20	Ayrton and Perry.
Gutta-percha	4·2	F. Jenkin	450×10^{12}	24	Standard adopted by Mr. Latimer Clark.
Shell-lac	1·95 ?	F. Jenkin	9000×10^{12}	28	Ayrton and Perry.
Hooper's material	3·3	Calculated from numbers given, page 67, Clark and Sabine	15000×10^{12}	24	Recent cable tests.
Ebonite	3·15	Boltzmann	28000×10^{12}	46	Ayrton and Perry.
Paraffin	1·98 2·32	Gibson and Barclay Boltzmann	34000×10^{12}	46	Ayrton and Perry.
Glass	1·90	F. Jenkin	Not yet measured with accuracy, as will be shown further on in this paper, but certainly greater than any of the above resistances.		
Air	1	..	Practically infinite.		

It will be observed that the conventional specific inductive capacity of shell-lac does not follow our law that small specific inductive capacity accompanies high specific resistance. We are inclined, however, to think that the real capacity may be far greater than this conventional one, since M. Boltzmann proved in 1874 that the true specific inductive capacities of sulphur and resin were respectively twice and one and a half times as great as the capacities for these substances given in books. Up to the present time we have not ourselves had an opportunity of finding the true specific inductive capacity of shell-lac.

In the preceding table the resistance of ebonite is given in accordance with the rule stated in our last paper, that is, the resistance given in that paper has been reduced in the ratio of 1·7 to 3·15. The specific inductive capacity of Hooper's india-rubber is usually given as 3·1; using, however, the numbers given on page 67, Clark and Sabine Electrical Tables for the capacities of a sheet of air, and of the india-rubber one square foot in area, and $\frac{1}{1000}$ inch in thickness, we find the ratio to be 3·3.

Following out the relationship shown in the above table, we may consider a highly conducting metal as a dielectric of extremely great specific inductive capacity, just as air, which has an extremely great resistance, is a dielectric having (as far as experiment has yet shown) the least specific inductive capacity. But as the resistance of the air although very great is not infinite, so it is reasonable to conceive the existence of a space more highly insulating than when filled with air, and at the same time having a less specific inductive capacity.

When we examine the phenomena of induction in this way we might expect to find a simple relation between the values of specific resistance and specific inductive capacity, but further consideration shows that the received notions of both these qualities of dielectrics are so badly defined that the first step is to establish clearness in our conceptions before endeavouring to determine in what exact way specific inductive capacity is a function of specific resistance.

As regards the capacity of an air condenser there cannot be much vagueness, for there is neither perceptible absorption nor true conduction, although further on we shall give our reasons for believing there is some absorption. But in all cases in which the dielectric is not a gas there are absorption and conduction phenomena of a complicated kind, and experiment hitherto has not accurately defined what is meant by capacity in such cases.

We are now carrying on a series of observations on induction phenomena in flint glass jars and other condensers with dielectrics having a greater and less specific conductivity than glass. It is an investigation which progresses very slowly, as some of the single experiments last nearly a month. Certain curves obtained for time risings and fallings of potential in condensers bore striking resemblances to the

curve expressing the time increase of strain in a substance subjected to a constant stress, and careful examination shows that all our results up to the present time bear so close an analogy with the stress and strain phenomena in viscous substances, that we feel that this analogy means a physical connexion. We therefore propose, even before the completion of the experiments, to show in the present paper the bearing of this analogy; since not only does it constitute an extension of Faraday's stress theory of induction, but, in addition, it forms a verification of the extension of our knowledge regarding stress and strain phenomena, as afforded by our recent experiments on the viscosity of substances. Besides this, the theory that we are about to sketch roughly, and which we hope to establish in all its details by experiment, is valuable in furnishing an easily conceived image of the internal action taking place in a dielectric.

A complete series of experiments will soon be ready for publication, showing that almost every body which can be examined exhibits a time increase of strain under a constant stress. Thus, when a small couple, much less than that which is required to produce any permanent set, is applied to twist a prism, there is a rapid production of torsion due to a property of the material measured usually by a number called the modulus of rigidity; the rapidity depending mainly on the mass which has to be moved, and also depending on the viscous yielding (or on something which is related to the viscous yielding) of the material. If we could eliminate the effect of inertia, then not an instantaneous but a very rapid production of torsion would be observed, the production of strain along the prism not being instantaneous even at the beginning, but proceeding with a velocity comparable with the velocity of electric induction. After a very short interval the torsion no longer increases rapidly, and only its amount (D) at the end of the interval is what has usually been taken into consideration by physicists and engineers. But it should be known that it is only after a long time that the production of strain ceases, and the additional torsion (d) in glass and steel fibres after some hours or days in some of our experiments became equal to ($\frac{1}{2}$D). If when the production of strain appears to have ceased the twisting couple be suddenly removed, then there is a rapid but not instantaneous reduction of strain (D), and an additional reduction (d) effected after a considerable time.

Thus for all strained substances that we have experimented on, the equilibriated state is one in which there is a free strain or a strain that is almost instantly removable, and there is also an absorbed strain which is only slowly produced, and which is only slowly removable.

Exactly in the same way, when a condenser is charged by a battery of electromotive force (V), the charge at first increases rapidly, due to a property of the condenser called its capacity (S); this rapidity

depends on the supply of electricity by the battery, and on a less or greater resistance to rapid motion offered by the viscosity (or on something related to the viscosity) of the dielectric.

If we could eliminate the effect of slow supply by the battery (and this is always eliminated in the charging of glass condensers by voltaic cells) we should still have not an instantaneous but a very rapid charging of the condenser; the rate of this charging does not at present appear to be experimentally measurable, but ballistic galvanometers measure the time integral of the current. We know from the shape of the curve which shows the increase of charge with time, and from the improbability of any want of continuity, that the rise of charge is not really instantaneous. After a very short interval when the rate of increase of charge is incomparably smaller than the mean rate during the interval, the total amount of charge (SV) is what has usually been taken into account by physicists. But this charge does, however, increase sometimes for many days to a maximum $\{(S+s)V\}$, and in certain experiments with a flint glass condenser (s) at the end of about ten days was equal to ($\frac{1}{2}$S). If, when the charge seems to increase no longer (we must not confound with increase of charge the small flow into the condenser which continues even after a long time, and which measures true conductivity) we connect the coatings of the condenser, there is a rapid but not instantaneous loss of charge (SV), and a further loss (sV) only occurring after a long period. Thus for our condenser (exactly as for the strained viscous prism) the equilibriated state for any given difference of potentials is one in which there is a free charge, that is, a charge almost instantly removable, and an absorbed charge, that is, a charge only slowly produced and slowly removable. It has usually been considered that this absorbed charge is quite distinct from conduction, but we know that when strain is being produced in a viscous substance, and all substances seem to be viscous, some of the energy is converted into heat, therefore if, as we consider, absorption is the production of strain, it must be accompanied by the generation of heat, and the conversion of electric energy into heat is most suitably termed conduction. We cannot, therefore, have absorption without conduction, and further, we consider all true conduction as an absorption in which the whole quantity of electricity is converted into heat, or into some form of energy not electrical. Again, when strain is produced by mechanical stress, we know that the more rapid is the production of strain the greater will be the amount of heat produced by internal friction. Reasoning by analogy, therefore, we may conclude that as the rate of production of electric strain grows less and less as the interval elapsing since charging increases, therefore the rate of conversion of electric energy into heat, that is, conduction, also grows less and less, and, therefore, it is correct to say that the resistance of a dielectric does

really increase by electrification. Besides this there is, of course, some of the absorbed energy which is recoverable, and which, therefore, must not be confounded with conduction, just as the energy recoverable from a deflected beam must be distinguished from that lost through conversion into heat on account of internal friction.

True conduction in a condenser, that is, the amount flowing in after much time has elapsed, and which is entirely lost as electric energy, however difficult it may be to measure with accuracy, is perhaps the only property of dielectrics having no reference to exact time, and, therefore, enabling comparison to be made between different dielectrics; that is, when the flow into a dielectric is steady this flow may possibly be proportional to the electromotive force. We say advisedly "may possibly," because we have reasons for thinking our ideas of stress and strain will not only throw light on the questions of absorption previously referred to, but will in addition suggest an explanation of the hitherto apparent inconsistency between the laws for change of resistance by heat in conductors and in insulators. If that be so, then we think it may be possible that we shall find out that Ohm's law is always but an approximation, and that this approximation is the nearer and nearer the truth the less the resistance of the conductor, and that for insulators the current, after it has become steady, may obey some such law as that experimentally determined by Mr. C. Varley for conduction through rarefied gases. (See "Proceedings Royal Society," October 5th, 1870.)

True conduction is found to be extremely small after some weeks for the flint glass in Sir W. Thomson's electrometers, care being taken that the charge in the jar is left quite undisturbed by induction, from electrified moving neighbouring bodies for example, whereas there is much loss of charge due to conductivity as well as that due to absorption in the first few days after charging. Thus two electrometer jars in our possession when left untouched for some weeks were found, if the first measurement were made not much less than one week after charging, to retain nearly the whole of their charge, the electrometers being in constant use, but the replenisher not touched during the entire time, to avoid alteration of internal distribution. Now when the conductivity of glass is measured in any ordinary way a few hours after charging, it seems to be very great even when we are able to eliminate true absorption, which is all recoverable as electric energy. Inattention to these considerations, we think, led some of the students of Sir W. Thomson to infer that the true conductivity of flint glass was incomparably greater than it really is.

In fact, we believe that up to the present time no experiments have ever been made which determine the amount of the true conductivity of flint glass, that is, the conductivity a long time after charging. Since very hot solid glass is like pitch, a "truly viscous fluid," that is

goes on continually yielding under the action of a constant stress, we must assume, since we cannot imagine a breach of continuity in the phenomenon, that glass even when cold is also a "truly viscous fluid," although the gain of strain per day may be almost infinitely small after the first few days. Similarly, as the true conductivity is very considerable in hot glass, we may conclude that there does exist true conductivity in cold glass, although the amount will probably be so small as to make its separation from surface conduction or other extraneous loss extremely difficult.

From the curves we have obtained of the charging of condensers, and assuming that there is no discontinuity, we must assume that even the first charging is itself a very rapid absorption, and since there is viscosity, even the very first charging must be accompanied with a generation of heat, that is, true conduction. Also since it is known that gases, like all other substances, are to a certain extent viscous, we cannot believe that air and other gaseous condensers show absolutely no absorptive phenomena, in fact, sufficiently accurate experiments have not yet been made on the subject.

We conclude, therefore, that the less the specific resistance of a substance the greater is its molecular plasticity, and the more plastic the substance is the greater will be the first charge; therefore from the stress and strain analogy it follows that the less the specific resistance of a substance the greater will generally be the specific inductive capacity, the result obtained experimentally at the commencement of this paper: according to what law, however, the one increases as the other diminishes, we are not at present in a position to state.

From all that precedes it follows that when the potential of a body surrounded by a dielectric is altered by induction, a portion of the electric energy is converted into heat, the amount being greater as the dielectric is more viscous. Consequently a charged body A, perfectly surrounded by a dielectric, may be discharged without contact with any conductor, by alternately bringing near and withdrawing a distant conductor B; for the capacity of the arrangement is alternately getting greater and less, therefore the potentials must be alternately growing less and greater, and since all alteration of potential is accompanied by an alteration of strain in the viscous substance composing the dielectric, the potential energy of the system must be gradually converted into heat, whereas from the almost infinite resistance of the dielectric, if the bodies were motionless, this result could never have been attained. Consequently since the particles of any body are in rapid motion, they must all tend to acquire the same potential, even if we imagine them separated by a dielectric of very great resistance, provided the relative motions are motions of translation. But if the movements are motions of rotation only, then this equalization of potential will only take place after a very great time, if it takes place

at all. This difference may, perhaps, explain why metals conduct so much better than glass, &c.

III. "Recent Experiments on Fog-Signals." By Dr. Tyndall, F.R.S., Professor of Natural Philosophy in the Royal Institution. Received March 14, 1878.

Our most intense coast-lights, including the six-wick lamp, the Wigham gas-light, and the electric light, being intended to aid the mariner in heavy weather, may be regarded, in a certain sense, as fog-signals. But fog, when thick, is intractable to light; the sun cannot penetrate it, much less any terrestrial source of illumination. Hence the necessity of employing sound-signals in dense fogs. Bells, gongs, horns, guns, and syrens have been used for this purpose; but it is mainly, if not wholly, explosive signals that I have now to submit to the notice of the Society. During the long, laborious, and, I venture to think, memorable series of observations conducted under the auspices of the Elder Brethren of the Trinity House at the South Foreland in 1872 and 1873, it was proved that a short $5\frac{1}{2}$-inch howitzer, firing 3 lbs. of powder, yielded a louder report than a long 18-pounder firing the same charge. Here was a hint to be acted on by the Elder Brethren. The effectiveness of the sound depended on the shape of the gun, and as it could not be assumed that in the howitzer we had hit accidentally upon the best possible shape, arrangements were made with the War Office for the construction of a gun specially calculated to produce the loudest sound attainable from the combustion of 3 lbs. of powder. To prevent the unnecessary landward waste of the sound, the gun was furnished with a parabolic muzzle, intended to project the sound over the sea, where it was most needed. The construction of this gun was based on a searching series of experiments executed at Woolwich with small models, provided with muzzles of various kinds. The gun was constructed on the principle of the revolver, its various chambers being loaded and brought in rapid succession into the firing position. The performance of the gun proved the correctness of the principles on which its construction was based.

It had been a widely spread opinion among artillerists, that a bronze gun emits a specially loud report. I doubted from the outset whether this would help us, and in a letter dated 22nd April, 1874, ventured to express myself thus:—"The report of a gun, as affecting an observer close at hand, is made up of two factors—the sound due to the shock of the air by the violently expanding gas, and the sound derived from the vibrations of the gun, which, to some extent, rings like a bell. This latter, I apprehend, will disappear at considerable distances."

The result of subsequent trial, as reported by General Campbell, is, "that the sonorous qualities of bronze are greatly superior to those of cast-iron at short distances, but that the advantage lies with the baser metal at long ranges."*

Coincident with these early trials of guns at Woolwich gun-cotton was thought of as a probably effective sound-producer. Theoretic considerations caused me to fix my attention persistently on this substance; for the remarkable experiments of Mr. Abel, whereby its rapidity of combustion and violently explosive energy are demonstrated, seemed to single it out as a substance eminently calculated to fulfil the conditions necessary to the production of an intense wave of sound. What those conditions are we shall now more particularly inquire, calling to our aid a brief but very remarkable paper, published by Professor Stokes in the "Philosophical Magazine" for 1868.

The explosive force of gunpowder is known to depend on the sudden conversion of a solid body into an intensely heated gas. The work which the artillerist requires the expanding gas to perform is the displacement of the projectile. Such, however, is not the work that we want our gunpowder to perform. We wish it to transmute its energy not into the mere mechanical translation of the shot, but into vibratory motion. We want *pulses* to be formed which shall propagate themselves to vast distances through the atmosphere, and this requires a certain choice and management of the explosive material.

A sound pulse consists essentially of two parts—a condensation and a rarefaction. Now air is a very mobile fluid, and if the shock imparted to it lack due promptness, the pulse is not produced. Consider the case of a common clock pendulum, which oscillates to and fro, and which therefore might be expected to generate corresponding pulses in the air. When, for example, the bob moves to the right, the air to the right of it might be supposed to be condensed, while a partial vacuum might be supposed to follow the bob. As a matter of fact, we have nothing of this kind. The air particles in front of the bob retreat so rapidly, and those behind it close so rapidly in, that no sound-pulse is formed. A tuning-fork which executes 256 complete vibrations in a second, if struck gently on a pad and held in free air, emits a scarcely audible note. It behaves to some extent like the pendulum bob. This feebleness is due to the prompt "reciprocating flow" of the air between the incipient condensations and rarefactions, whereby the formation of sound-pulses is forestalled. Stokes, however, has taught us that this flow may be intercepted by placing the

* General Campbell assigns a true cause for this difference. The ring of the bronze gun represents so much energy withdrawn from the explosive force of the gunpowder. Further experiments would, however, be needed to place the superiority of the cast-iron gun at a distance beyond question.

edge of a card in close proximity to one of the corners of the fork. An immediate augmentation of the sound of the fork is the consequence.

The more rapid the shock imparted to the air, the greater is the fractional part of the energy of the shock converted into wave motion. And as different kinds of gunpowder vary considerably in their rapidity of combustion, it may be expected that they will also vary as producers of sound. This theoretic inference is completely verified by experiment. In a series of preliminary trials conducted at Woolwich on the 4th of June, 1875, the sound-producing powers of four different kinds of powder were determined. In the order of size of grain they bear the names respectively of Fine-grain (F.G.), Large-grain (L.G.), Rifle Large-grain (R.L.G.), and Pebble-powder (P.). The charge in each case amounted to $4\frac{1}{2}$ lbs.; four 24-pound howitzers being employed to fire the respective charges. There were eleven observers, all of whom, without a single dissentient, pronounced the sound of the fine-grain powder loudest of all. In the opinion of seven of the eleven the large-grain powder came next; seven also of the eleven placed the rifle large-grain third on the list; while they were again unanimous in pronouncing the pebble-powder the worst sound-producer. These differences are entirely due to differences in the rapidity of combustion. All who have witnessed the performance of the 80-ton gun at Woolwich must have been surprised at the mildness of its thunder. To avoid the strain resulting from quick combustion, the powder employed is composed of lumps far larger than those of the pebble-powder above referred to. In the long tube of the gun these lumps of solid matter gradually resolve themselves into gas, which on issuing from the muzzle imparts a kind of push to the air, instead of the sharp shock necessary to form the condensation of an intensely sonorous wave.

These are some of the physical reasons why gun-cotton might be regarded as a promising fog-signal. Firing it as we have been taught to do by Mr. Abel, its explosion is more rapid than that of gunpowder. In its case the air particles, alert as they are, will not, it might be presumed, be able to slip from places of condensation to places of rarefaction with a rapidity sufficient to forestall the formation of the wave. On *à priori* grounds then, we are entitled to infer the effectiveness of gun-cotton, while in a great number of comparative experiments, stretching from 1874 to the present time, this inference has been verified in the most conclusive manner.

As regards explosive material, and zealous and accomplished help in the use of it, the resources of Woolwich Arsenal have been freely placed at the disposal of the Elder Brethren. General Campbell, General Younghusband, Colonel Fraser, Colonel Maitland, and other officers, have taken an active personal part in the investigation, and in

most cases have incurred the labour of reducing and reporting on the observations. Guns of various forms and sizes have been invoked for gunpowder, while gun-cotton has been fired in free air, and in the foci of parabolic reflectors.

On the 22nd of February, 1875, a number of small guns, cast specially for the purpose—some with plain, some with conical, and some with parabolic muzzles, firing 4 oz. of fine-grain powder, were pitted against 4 oz. of gun-cotton, detonated both in the open and in the focus of a parabolic reflector.* The sound produced by the gun-cotton, reinforced by the reflector, was unanimously pronounced loudest of all. With equal unanimity, the gun-cotton detonated in free air was placed second in intensity. Though the same charge was used throughout, the guns differed notably among themselves, but none of them came up to the gun-cotton, either with or without the reflector. A second series, observed from a different distance on the same day, confirmed to the letter the foregoing result.

As a practical point, however, the comparative cost of gun-cotton and gunpowder has to be taken into account, though considerations of cost ought not to be stretched too far in cases involving the safety of human life. In the earlier experiments, where quantities of equal price were pitted against each other, the results were somewhat fluctuating. Indeed, the perfect manipulation of the gun-cotton required some preliminary discipline—promptness, certainty, and effectiveness of firing, augmenting as experience increased. As 1lb. of gun-cotton costs as much as 3 lbs. of gunpowder, these quantities were compared together on the 22nd of February. The guns employed to discharge the gunpowder were a 12-lb. brass howitzer, a 24-lb. cast-iron howitzer, and the long 18-pounder used at the South Foreland. The result recorded is, that the 24-lb. howitzer, firing 3 lbs. of gunpowder, had a slight advantage over 1 lb. of gun-cotton detonated in the open; while the 12-lb. howitzer and the 18-pounder were both beaten by the gun-cotton. On the 2nd of May, on the other hand, the gun-cotton is reported as having been beaten by all the guns.

Meanwhile, the parabolic muzzle-gun, expressly intended for fog-signalling, was pushed rapidly forward, and on the 22nd and 23rd of March, 1876, its power was tested at Shoeburyness. Pitted against it were a 16-pounder, a $5\frac{1}{2}$-inch howitzer, $1\frac{1}{2}$ lb. of gun-cotton detonated in the focus of a reflector, and $1\frac{1}{2}$ lb. of gun-cotton detonated in free air. On this occasion, nineteen different series of experiments were made, when the new experimental gun, firing a 3-lb. charge, demonstrated its superiority over all guns previously employed to fire the same charge. As regards the comparative merits of the gun-cotton fired in

* For charges of this weight the reflector is of moderate size, and may be employed without fear of fracture.

the open, and the gunpowder fired from the new gun, the mean values of their sounds were found to be the same. Fired in the focus of the reflector, the gun-cotton clearly dominated over all the other sound-producers.*

The whole of the observations here referred to were embraced by an angle of about 70°, of which 50° lay on the one side and 20° on the other side of the line of fire. The shots were heard by eleven observers on board the "Galatea," which took up positions varying from 2 miles to $13\frac{1}{2}$ miles from the firing-point. In all these observations, the reinforcing action of the reflector, and of the parabolic muzzle of the gun, came into play. But the reinforcement of the sound in one direction implies its withdrawal from some other direction, and accordingly it was found that at a distance of $5\frac{1}{4}$ miles from the firing-point, and on a line including nearly an angle of 90° with the line of fire, the gun-cotton in the open beat the new gun; while behind the station, at distances of $8\frac{1}{2}$ miles and $13\frac{1}{2}$ miles respectively, the gun-cotton in the open beat both the gun and the gun-cotton in the reflector. This result is rendered more important by the fact that the sound reached the Mucking Light, a distance of $13\frac{1}{2}$ miles, against a light wind which was blowing at the time.

Most, if not all, of our ordinary sound-producers send forth waves which are not of uniform intensity throughout. A trumpet is loudest in the direction of its axis. The same is true of a gun. A bell, with its mouth pointed upwards or downwards, sends forth waves far denser in the horizontal plane passing through the bell than at an angular distance of 90° from that plane. The oldest bellhangers must have been aware of the fact that the sides of the bell, and not its mouth, emitted the strongest sound, their practice being determined by this knowledge. Our slabs of gun-cotton also emit waves of different densities in different parts. It has occurred in the experiments at Shoeburyness that when the broad side of a slab was turned towards the suspending wire of a second slab six feet distant, the wire was cut by the explosion, while when the edge of the slab was turned to the wire this never occurred. To the circumstance that the broad-sides of the slabs faced the sea is probably to be ascribed the remarkable fact observed on the 23rd March, that in two directions, not far removed from the line of fire, the gun-cotton detonated in the open had a slight advantage over the new gun.

Theoretic considerations rendered it probable that the shape and size of the exploding mass would affect the constitution of the wave of sound. I did not think large rectangular slabs the most favourable shape, and accordingly proposed cutting a large slab into fragments of different sizes, and pitting them against each other. The differences

* In this case the reflector was fractured by the explosion, but it did good service after fracture.

between the sounds were by no means so great as the differences in the quantities of explosive material might lead one to expect. The mean values of eighteen series of observations made on board the "Galatea," at distances varying from $1\frac{3}{4}$ mile to 4·8 miles, were as follows :—

Weights.....	4-oz.	6-oz.	9-oz.	12-oz.	7-oz. Rocket.
Value of sound.....	3·12	3·34	4·0	4·03	3·35

These charges were cut from a slab of dry gun-cotton about $1\frac{3}{4}$ inch thick : they were squares and rectangles of the following dimensions : —4 oz., 2 inches by 2 inches ; 6 oz., 2 inches by 3 inches ; 9 oz., 3 inches by 3 inches ; 12 oz., 2 inches by 6 inches.

The numbers under the respective weights express the recorded value of the sounds. They must be simply taken as a ready means of expressing the approximate relative intensity of the sounds as estimated by the ear. When we find a 9-oz. charge marked 4, and a 12-oz. charge marked 4·03, the two sounds may be regarded as practically equal in intensity, an addition of 30 per cent. in the larger charges producing no sensible difference in the sound. Were the sounds estimated by some physical means, instead of by the ear, the values of the sounds would not, in my opinion, show a greater advance with the increase of material than that indicated by the foregoing numbers. Subsequent experiments rendered still more certain the effectiveness, as well as the economy, of small charges of gun-cotton.

It is an obvious corollary from the foregoing experiments that on our "nesses" and promontories, where the land is clasped on both sides for a considerable distance by the sea—where, therefore, the sound has to propagate itself rearward as well as forward—the use of the parabolic gun, or of the parabolic reflector might be a disadvantage rather than an advantage. Here gun-cotton, exploded in the open, forms a most appropriate source of sound. This remark is especially applicable to such lightships as are intended to spread the sound all round them as from central foci. As a signal in rock lighthouses, where neither syren, steam-whistle, nor gun could be mounted, and as a handy fleet-signal, dispensing with the lumber of special signal-guns, the gun-cotton will prove invaluable. But in most of these cases we have the drawback that local damage may be done by the explosion. The lantern of the rock-lighthouse might suffer from concussion near at hand, and though mechanical arrangements might be devised, both in the case of the lighthouse and of the ship's deck, to place the firing-point of the gun-cotton at a safe distance, no such arrangement could compete, as regards simplicity and effectiveness, with the expedient of a *gun-cotton rocket.* Had such a means of signalling existed at the Bishop's Rock Lighthouse the ill-fated Schiller might have been warned of her approach to danger ten, or it may be

twenty, miles before she reached the rock which wrecked her. Had the fleet possessed such a signal, instead of the ubiquitous but ineffectual whistle, the "Iron Duke" and "Vanguard" need never have come into collision.

It was the necessity of providing a suitable signal for rock lighthouses, and of clearing obstacles which cast an acoustic shadow, that suggested the idea of the gun-cotton rocket to Sir Richard Collinson, Deputy Master of the Trinity House. That idea was to place a disk or short cylinder of the gun-cotton, in the head of a rocket, the ascensional force of which should be employed to carry the disk to an elevation of 1,000 feet or thereabouts, where by the ignition of a fuse associated with a detonator, the gun-cotton should be fired, sending its sound in all directions vertically and obliquely down upon earth and sea. The first attempt to realize this idea was made on the 18th of July, 1876, at the fire-work manufactory of the Messrs. Brock, at Nunhead. Eight rockets were then fired, four being charged with 5 oz. and four with $7\frac{1}{2}$ oz. of gun-cotton. They ascended to a great height, and exploded with a very loud report in the air. On the 27th of July, the rockets were tried at Shoeburyness, the most noteworthy result on this occasion being the hearing of the rockets at the Mouse Lighthouse, $8\frac{1}{2}$ miles E. by S., and at the Chapman Lighthouse, $8\frac{1}{2}$ miles W. by N.; that is to say, at opposite sides of the firing-point. It is worthy of remark that, in the case of the Chapman Lighthouse, land and trees intervened between the firing-point and the place of observation. "This," as General Younghusband justly remarked at the time, "may prove to be a valuable consideration if it should be found necessary to place a signal station in a position whence the sea could not be freely observed." Indeed, the clearing of such obstacles was one of the objects which the inventor of the rocket had in view.

On the 13th of December, 1876, and again on the 8th of March, 1877, comparative experiments on firing at high and low elevations were executed. The gun-cotton near the ground consisted of $\frac{1}{2}$-lb. disks suspended from a horizontal iron bar about $4\frac{1}{2}$ feet above the ground. The rockets carried the same quantity of gun-cotton in their heads, and the height to which they attained, as determined by a theodolite, was from 800 to 900 feet.

The latter day was cold, with occasional squalls of snow and hail, the direction of the sound being at right angles to that of the wind. Five series of observations were made on board the "Vestal," at distances varying from 3 to 6 miles. The mean value of the explosions in the air exceeded that of the explosions near the ground by a small but sensible quantity. At Windmill Hill, Gravesend, however, which was nearly to leeward, and $5\frac{1}{2}$ miles from the firing-point, in nineteen cases out of twenty-four the disk fired near the ground was loudest; while in the remaining five the rocket had the advantage. Towards the

close of the day the atmosphere became very serene. A few distant cumuli sailed near the horizon, but the zenith and a vast angular space all round it were absolutely free from cloud. From the deck of the "Galatea" a rocket was discharged, which reached a great elevation, and exploded with a loud report. Following this solid nucleus of sound was a continuous train of echoes, which retreated to a continually greater distance, dying gradually off into silence after seven seconds' duration. These echoes were of the same character as those so frequently noticed at the South Foreland in 1872–73, and called by me "aerial echoes."

On the 23rd of March the experiments were resumed, the most noteworthy results of that day's observations being that the sounds were heard at Tillingham, 10 miles to the N.E.; at West Mersea, $15\frac{3}{4}$ miles to the N.E. by E.; at Brightlingsea, $17\frac{1}{2}$ miles to the N.E.; and at Clacton Wash, $20\frac{1}{2}$ miles to the N.E. by $\frac{1}{2}$ E. The wind was blowing at the time from the S.E. Some of these sounds were produced by rockets, some by a 24-lb. howitzer, and some by an 8-inch Maroon.

In December, 1876, Mr. Gardiner, the managing director of the Cotton-powder Company, had proposed a trial of this material against the gun-cotton. The density of the cotton he urged was only 1·03, while that of the powder was 1·70. A greater quantity of explosive material being thus compressed into the same volume, Mr. Gardiner thought that a greater sonorous effect must be produced by the powder. At the instance of Mr. Mackie, who had been in communication previously with the Deputy Master of the Trinity House and myself, a Committee of the Elder Brethren visited the cotton-powder manufactory, on the banks of the Swale, near Faversham, on the 16th of June, 1877. The weights of cotton-powder employed were 2 oz., 8 oz., 1 lb., and 2 lbs., in the form of rockets and of signals fired a few feet above the ground. The experiments throughout were arranged and conducted by Mr. Mackie. Our desire on this occasion was to get as near to windward as possible, but the Swale and other obstacles limited our distance to $1\frac{1}{2}$ mile. We stood here E.S.E. from the firing-point while the wind blew fresh from the N.E.

The cotton-powder yielded a very effective report. The rockets in general had a slight advantage over the same quantities of material fired near the ground. The loudness of the sound was by no means proportional to the quantity of the material exploded, 8 oz. yielding very nearly as loud a report as 1 lb. The "aerial echoes," which invariably followed the explosion of the rockets, were loud and long-continued, shading off, as in all previous cases, by imperceptible gradations into silence.

On the 17th of October, 1877, another series of experiments with howitzers and rockets was carried out at Shoeburyness. The charge of the howitzer was 3 lbs. of L.G. powder. The charges of the

rockets were 12 oz., 8 oz., 4 oz., and 2 oz., of gun-cotton respectively. The gun and the four rockets constituted a series, and eight series were fired during the afternoon of the 17th. The observations were made from the "Vestal" and the "Galatea," positions being assumed which permitted the sound to reach the observers with the wind, against the wind, and across the wind. The distance of the "Galatea" varied from 3 to 7 miles, that of the "Vestal," which was more restricted in her movements, being 2 to 3 miles. Briefly summed up, the result is that the howitzer, firing a 3-lb. charge, which it will be remembered was our best gun at the South Foreland, was beaten by the 12-oz. rocket, by the 8-oz. rocket, and by the 4-oz. rocket. The 2-oz. rocket alone fell behind the howitzer.

It is worth while recording the distances to which some of the sounds were heard on the day now referred to :—

1. Leigh	6½ miles	W.N.W. ..	24	out of	40	sounds heard.
2. Girdler Light-vessel ..	12 „	S.E. by E...	5	„	„	
3. Reculvers	17½ „	S.E. by S. ..	18	„	„	
4. St. Nicholas	20 „	S.E. ..	3	„	„	
5. Epple Bay	22 „	S.E. by E. ..	19	„	„	
6. Westgate.....	23 „	S.E. by E...	9	„	„	
7. Kingsgate	25 „	S.E. by E...	8	„	„	

The day was cloudy, with occasional showers of drizzling rain; the wind about N.W. by N. all day; at times squally, rising to a force of 6 and 7 and sometimes dropping to a force of 2 or 3. The station at Leigh excepted, all these places were to leeward of Shoeburyness. At four other stations to leeward, varying in distance from 15½ to 24½ miles, nothing was heard, while at eleven stations to windward, varying from 8 to 26 miles, the sounds were also inaudible. It was found, indeed, that the sounds proceeding against the wind did not penetrate much beyond 3 miles.

On the following day, viz., the 18th October, we proceeded to Dungeness with the view of making a series of strict comparative experiments with gun-cotton and cotton-powder. Rockets containing 8 oz., 4 oz., and 2 oz. of gun-cotton had been prepared at the Royal Arsenal; while others, containing a similar quantity of cotton-powder, had been supplied by the Cotton-powder Company at Faversham. With these were compared the ordinary 18-pounder gun, which happened to be mounted at Dungeness, firing the usual charge of 3 lbs. of powder.

From these experiments it appeared that the gun-cotton and cotton-powder were practically equal as producers of sound.

The effectiveness of small charges was also illustrated in a very striking manner, only a single unit separating the numerical value of the 8-oz. rocket from that of the 2-oz. rocket. The former was recorded as 6·9 and the latter as 5·9, the value of the 4-oz. charge being intermediate between them. These results were recorded by a number

of very practised observers on board the "Galatea." They were completely borne out by the observations of the Coastguard, who marked the value of the 8-oz. rocket 6·1, and that of the 2-oz. rocket 5·2. The 18-pounder gun fell far behind all the rockets, a result probably to be in part ascribed to the imperfection of the powder. The performance of the syren was, on the whole, less satisfactory than that of the rockets. The instrument was worked, not by steam of 70 lbs. pressure, as at the South Foreland, but by compressed air, beginning with 40 lbs. and ending with 30 lbs. pressure. The trumpet was pointed to windward, and in the axis of the instrument the sound was about as effective as that of the 8-oz. rocket. But in a direction at right angles to the axis, and still more in the rear of this direction, the syren fell very sensibly behind even the 2-oz. rocket.

These are the principal comparative trials made between the gun-cotton rocket and other fog-signals; but they are not the only ones. On the 2nd of August, 1877, for example, experiments were made at Lundy Island with the following results. At 2 miles distant from the firing point, with land intervening, the 18-pounder, firing a 3-lb. charge, was quite unheard. Both the 4-oz. rocket and the 8-oz. rocket, however, reached an elevation which commanded the acoustic shadow, and yielded loud reports. When both were in view, the rockets were still superior to the gun. On the 6th of August, at St. Ann's, the 4-oz. and 8-oz. rockets proved superior to the syren. On the Shambles Light-vessel, when a pressure of 13 lbs. was employed to sound the syren, the rockets proved greatly superior to that instrument. Proceeding along the sea margin at Flamboro' Head, Mr. Edwards states that at a distance of $1\frac{1}{4}$ mile, with the 18-pounder gun hidden behind the cliffs, its report was quite unheard, while the 4-oz. rocket, rising to an elevation which brought it clearly into view, yielded a powerful sound in the face of an opposing wind.

On the evening of February 9th, 1877, a remarkable series of experiments was made by Mr. Prentice, at Stowmarket, with the gun-cotton rocket. From the report with which he has kindly furnished me I extract the following particulars. The first column in the annexed statement contains the name of the place of observation, the second its distance from the firing-point, and the third the result observed:—

Stoke Hill, Ipswich....	10 miles	Rockets clearly seen and sounds distinctly heard 53 seconds after the flash.
Melton	15 „	Signals distinctly heard. Thought at first that sounds were reverberated from the sea.
Framlingham	18 „	Signals very distinctly heard, both in the open air and in a closed room. Wind in favour of sound.
Stratford. St. Andrews	19 „	Reports loud; startled pheasants in a cover close by.

Tuddenham. St. Martin	10 miles	Reports very loud; rolled away like thunder.
Christ Church Park....	11 „	Report arrived a little more than a minute after flash.
Nettlested Hall.....	6 „	Distinct in every part of observer's house. Very loud in the open air.
Bildestone.....	6 „	Explosion very loud, wind against sound.
Nacton	14 „	Reports quite distinct—mistaken by inhabitants for claps of thunder.
Aldboro	25 „	Rockets seen through a very hazy atmosphere; a rumbling detonation heard.
Capel Mills	11 „	Reports heard within and without the observer's house. Wind opposed to sound.
Lawford.....	15½ „	Reports distinct: attributed to distant thunder.

In the great majority of these cases, the direction of the sound enclosed a large angle with the direction of the wind. In some cases, indeed, the two directions were at right angles to each other. It is needless to dwell for a moment on the advantage of a signal commanding ranges such as these.

The explosion of substances in the air, after having been carried to a considerable elevation by rockets, is a familiar performance. In 1873 the Board of Trade actually proposed a light-and-sound rocket as a signal of distress, which proposal was subsequently realised, but in a form too elaborate and expensive for practical use. The idea of the gun-cotton rocket with a view to signalling in fogs is, I believe, wholly due to the Deputy Master of the Trinity House. Thanks to the skilful aid given by the authorities of Woolwich, by Mr. Prentice, and Mr. Brock, that idea is now an accomplished fact; a signal of great power, handiness, and economy, being thus placed at the service of our mariners. Not only may the rocket be applied in association with lighthouses and lightships, but in the Navy also it may be turned to important account. Soon after the loss of the "Vanguard" I ventured to urge upon an eminent naval officer the desirability of having an organised code of fog-signals for the fleet. He shook his head doubtingly, and referred to the difficulty of finding room for signal-guns. The gun-cotton rocket completely surmounts this difficulty. It is manipulated with ease and rapidity, while its discharges may be so grouped and combined as to give a most important extension to the voice of the admiral in command. It is needless to add that at any point upon our coasts, or upon any other coast, where its establishment might be desirable, a fog-signal station might be extemporised without difficulty.

I have referred more than once to the train of echoes which accompanied the explosion of gun-cotton in free air, speaking of them as similar in all respects to those which were described for the first time in my Report on fog-signals, addressed to the Corporation of Trinity

House in 1874.* To these echoes I attached a fundamental significance. There was no visible reflecting surface from which they could come. On some days, with hardly a cloud in the air, and hardly a ripple on the sea, they reached us with magical intensity. They came directly from the body of the air in front of the great trumpet which produced them. The trumpet-blasts were five seconds in duration, but long before the blast had ceased the echoes struck in, adding their strength to the primitive note of the trumpet. After the blast had ended the echoes continued, retreating further and further from the point of observation, and finally dying away at great distances. The echoes were perfectly continuous as long as the sea was clear of ships, "tapering" by imperceptible gradations into absolute silence. But when a ship happened to throw itself athwart the course of the sound, the echo from the broadside of the vessel was returned as a shock which rudely interrupted the continuity of the dying atmospheric music.

These echoes have been ascribed to reflection from the crests of the sea-waves. But this hypothesis is negatived by the fact that the echoes were produced in great intensity and duration when no waves existed —when the sea, in fact, was of glassy smoothness. It has been also shown that the direction of the echoes depended not on that of waves, real or assumed, but on the direction of the axis of the trumpet. Causing that axis to traverse an arc of 210°, and the trumpet to sound at various points of the arc, the echoes were always, at all events in calm weather, returned from that portion of the atmosphere towards which the trumpet was directed. They could not, under the circumstances, come from the glassy sea; while both their variation of direction, and their perfectly continuous fall into silence, are irreconcileable with the notion that they came from fixed objects on the land. They came from that portion of the atmosphere into which the trumpet poured its maximum sound, and fell in intensity as the direct sound penetrated to greater atmospheric distances.

The day on which our latest observations were made was particularly fine. Before reaching Dungeness, the smoothness of the sea and the serenity of the air caused me to test the echoing power of the atmosphere. A single ship lay about half a mile distant between us and the land. The result of the proposed experiment was clearly foreseen. It was this. The rocket being sent up, it exploded at a great height; the echoes retreated in their usual fashion, becoming less and less intense as the distance of the surfaces of reflection from the observers increased. About five seconds after the explosion, a single loud shock was sent back to us from the side of the vessel lying between us and the land. Obliterated for a moment by this

* See also "Philosophical Transactions" for 1874, p. 183.

more intense echo, the aërial reverberation continued its retreat, dying away into silence in two or three seconds afterwards.

I have referred to the firing of an 8-oz. rocket from the deck of the "Galatea," on the 8th of March, 1877, stating the duration of its echoes to be seven seconds. Mr. Prentice, who was present at the time, assured me that, in his experiments with rockets, similar echoes had been frequently heard of more than twice this duration. The ranges of his sounds alone would render this result in the highest degree probable.

To attempt to interpret an experiment which I have not had an opportunity of repeating, is an operation of some risk; and it is not without a consciousness of this that I refer here to a result considered adverse to the notion of aërial echoes. When the trumpet of a syren is pointed towards the zenith, it is alleged that when the syren is sounded no echo is returned. Now the reflecting surfaces which give rise to these echoes are for the most part due to differences of temperature between sea and air. If, through any cause, the air above be chilled, we have descending streams—if the air below be warmed, we have ascending streams as the initial cause of atmospheric flocculence. A sound proceeding vertically does not cross the streams, nor impinge upon the reflecting surfaces, as does a sound proceeding horizontally across them. Aërial echoes, therefore, will not accompany the vertical sound as they accompany the horizontal one. The experiment, as I interpret it, is not opposed to the theory of aërial echoes which I have ventured to enunciate. But, as I have indicated, not only to see, but to vary such an experiment, is a necessary prelude to grasping its full significance.

In a paper published in the "Philosophical Transactions" for 1876, Professor Osborne Reynolds refers to these echoes in the following terms: "Without attempting to explain the reverberations and echoes which have been observed, I will merely call attention to the fact that in no case have I heard any attending the reports of the rockets,* although they seem to have been invariable with the guns and pistols. These facts suggest that the echoes are in some way connected with the direction given to the sound. They are caused by the voice, trumpets, and the syren, all of which give direction to the sound; but I am not aware that they have ever been observed in the case of a sound which has no direction of greatest intensity."

The reference to the voice and other references cause me to think that, in speaking of echoes, Professor Osborne Reynolds and myself are dealing with different phenomena. Be that as it may, the foregoing observations render it perfectly certain that the condition as to direction here laid down is not necessary to the production of the echoes.

* These carried 12 oz. of gunpowder.

There is not a feature connected with the aërial echoes which cannot be brought out by experiments in the laboratory. I have recently made the following experiment:—A rectangle, 22 inches by 12, is crossed by 23 brass tubes, each having a slit along it from which gas can issue. In this way, 23 low, flat flames are obtained. A sounding reed, fixed in a short tube, is placed at one end of the rectangle, and a sensitive flame at some distance beyond the other end. When the reed sounds, the flame in front of it is violently agitated, and roars boisterously. Turning on the gas, and lighting it as it issues from the slits, the air above the flames becomes so heterogeneous that the sensitive flame is instantly stilled by the aërial reflection, rising from a height of 6 inches to a height of 18 inches. Here we have the acoustic opacity of the air in front of the South Foreland strikingly imitated. Turning off the gas, and removing the sensitive flame to some distance behind the reed, it burns there tranquilly, though the reed may be sounding. Again lighting the gas as it issues from the brass tubes, the sound reflected from the heterogeneous air throws the sensitive flame into violent agitation. Here we have imitated the aërial echoes heard when standing behind the syren-trumpets at the South Foreland. The experiment is extremely simple, and in the highest degree impressive.

March 28, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

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

Notice was given that the name of Dr. Radcliffe had been withdrawn from the list of Candidates for the Fellowship.

The following Papers were read:—

I. "On Putrescent Organic Matter in Potable Water. II." By GUSTAV BISCHOF. Received February 18, 1878. Communicated by E. FRANKLAND, F.R.S.

Referring to the paper which I communicated to the Royal Society last session*, I have to add, that after passing water continuously for six weeks through one of the vessels there described, filled with spongy

* "Proceedings," vol. xxvi, p. 152.

iron, and on the bottom of which meat had been placed, the latter was still in a perfectly fresh and hard condition.

This result encouraged me to try in a similar way the action of spongy iron upon hay infusion. The hay, steeped in water, was heated for two days to about 35° C. The infusion then showed, under the microscope, an abundance of organic life. I could not obviously test the action of spongy iron upon this infusion, in the manner described in my last paper, namely, by passing it continuously for weeks through a vessel containing spongy iron and meat. However, I believe the result will be considered equally conclusive as to the absence of any putrefactive agents in hay infusion after filtration through spongy iron, should the meat remain fresh on being immersed in the filtered liquid for several weeks.

The following arrangement was adopted:—

A large vessel was filled with the ordinary filtering materials employed in spongy iron filters, namely, commencing at the bottom, with gravel, sand, pyrolusite, and spongy iron. The three first materials were introduced to prevent the filtered solution containing any iron. The vessel was provided with a neck at the bottom, and a volume of the hay infusion, equal to the bulk of spongy iron in the filter, was passed through it every 45 minutes.

Into the neck of the vessel I fixed some tin tubing by means of an india-rubber stopper. The tubing was connected with six tins, or cylindrical vessels of tin-plate, holding about 500 cub. centims. each. After placing a piece of fresh meat in each tin, a lid was soldered on air-tight. The tubing was so arranged, that the inlet tube passed to the bottom of each tin, an exit tube carrying the water off from the top. By first pressing the tubing together, and then cutting it, all the tins could be readily separated without exposure to air. Each tin was provided on the top with a short piece of tubing, to serve later on for the escape of steam.

In order to wash the apparatus and materials, I passed, in the first instance, water of the New River Company continuously through them for 24 hours. The tins and contents were then immersed for two hours in boiling water to destroy any putrefactive germs adhering to the meat. After closing the tubing, which served during boiling for the escape of steam, the tins were cooled, and then the filtration of New River water continued for two more hours.

The last two tins, which, it will be understood, contained filtered water and meat, were now detached, and into one of them I passed 100 cub. centims. of filtered air, after connecting under alcohol with a glass tube, 6 feet long, containing compressed cotton wool, through which the air was forced. These two tins were kept for comparison with those in which the meat was to remain in contact with filtered hay infusion. The infusion was passed for six hours through the

apparatus, including the remaining four tins. These being then also detached, 100 cub. centims. each of filtered air were passed as before into two of the tins.

On opening the tins, which contained filtered water, the one with, the other without filtered air, the meat was, after four to five weeks' standing, found to be quite fresh. Nevertheless, when, after some time, a drop adhering to the meat was examined under the microscope, moving organisms were detected.

The tins which contained meat and the filtered hay infusion gave similar results. One pair of the tins, the one with, the other without filtered air, was opened after nine weeks, and the other after nine months' standing. In both cases the meat remained fresh.

The hay infusion after filtration had a peculiar smell, reminding me of some kinds of cheese. This obviously imparted to the meat a similar smell, which, however, was quite distinct from that of putrid meat. After the samples of meat had been standing for 24 hours, the smell decreased considerably, and the following day it was hardly perceptible. Between the third and sixth day the several samples exhibited gradually the characteristic smell of putrid meat; those samples which did not contain filtered air resisting putrescence, apparently, longer than the others. The filtered hay infusion, which had not been in contact with meat, had, after several months' standing still, the same peculiar smell.

The conclusions which I drew in my last paper as to the antiseptic properties of spongy iron upon putrefactive agents in ordinary water apply therefore equally to hay infusion. However, those samples to which filtered air had been supplied, prove more conclusively than my experiments last year that the bacteria, or their germs, are not revived when supplied with oxygen after the filtration. This, in my opinion, is a result of some importance, as it demonstrates that, by filtration through spongy iron, putrefaction of organic matter is not only suspended for a time, but that it ceases entirely until reinstated by some putrefactive agent foreign to the water.

Since communicating my last paper, I have also continued the inquiry, how the peculiar action of spongy iron upon organic matter is to be explained. If a rod be inserted into a body of spongy iron, which has been in contact with water for some time, gas bubbles are seen to escape. This gas is sometimes explosive, sometimes not. I collected a quantity of the gas from two different filters, one of which had been in constant operation with ordinary water for ten months. It was free from any carbonic anhydride, but contained carbon and hydrogen. The hydrogen obviously results from the decomposition of water by spongy iron. The carbon might be due to the decomposition of carbonaceous organic matter, or it might be produced similarly to the carbo-hydrogen obtained, when dissolving ordinary iron by an acid.

To decide which of these explanations is correct, a vessel was filled with spongy iron and distilled water, the air being expelled from the former as far as practicable. After three months' standing, I analyzed the gas, collected as before by inserting a rod into the spongy iron. I found that it neither contained hydrogen nor carbon, therefore most probably consisted only of atmospheric nitrogen. This appears to indicate that the carbon, which was obtained in the previous experiment, is actually the result of the decomposition of organic matter.

The connexion between disease and impure water, more especially if it be contaminated by putrescent organic matter, has been strongly urged by various authorities, such as Drs. Buchanan, Frankland, Sanderson, Simon, Tyndall, and others. This has led me to attach such importance to the demonstration that "living ferments" are absent from polluted water after filtration through spongy iron. Analytical figures, in their turn, have proved that even Thames water can by filtration through this material be made, chemically speaking, purer than some of the best deep well waters. As the latter are mostly more or less supplied by polluted surface water, which is purified by filtration in passing downwards, there is no reason why they should be preferred to artificially filtered water, provided the physiological character of both proves to be alike. This inquiry is at the present moment being officially instituted in several countries. Trustworthy evidence in the form of actual experience may thus ere long be expected to settle the final question, whether and how far the artificial purification of impure water by spongy iron can be considered a safeguard against the propagation of disease.

II. "On the Modifications of the Simple and Compound Eyes of Insects." By B. Thompson Lowne, F.R.C.S., Lecturer on Physiology at the Middlesex Hospital Medical School, Arris and Gale Lecturer on Anatomy and Physiology in the Royal College of Surgeons. Communicated by Professor Flower, F.R.S. Received February 27, 1878.

(Abstract.)

The simple eyes of insects have been so accurately described by previous observers, that little need be said on their structure. I have described the simple eye of *Eristalis*, chiefly for comparison with the compound and aggregate eyes. The close relation of the recipient rods to the inner surface of the cornea in this insect is most noteworthy, since, combined with the great convexity of the cornea and the highly refractive nature of the rods themselves, this renders the formation of an optical picture impossible. These facts with the small

number of recipient rods shew that it is most probable that the function of the ocelli is rather the perception of the intensity and the direction of light than of the actual disposition and colours of surrounding objects.

The compound eyes of insects are of two kinds; an intermediate condition between the simple and compound eye, closely resembling the aggregate eyes of other anthropods, which I have at present only found in the Nematocerous Diptera, and in the bees, wasps, and ants, the only examples of the Hymenoptera in which I have examined the eyes; and the true compound eye, found in the majority of insects.

I strongly suspect that further observations will show that the aggregate form of eye is also characteristic of the Hemiptera and of a large number of the Coleoptera; I hold this view, from a casual observation of the eyes of several species of Hemiptera, as well as from the imperfect description of the eyes of these insects in Dr. Grenacher's recent paper.

I have examined and described the eyes of *Tipula*, *Formica*, and *Vespa*, which are examples of very highly modified aggregate eyes. In these insects each facet of the cornea has sixteen recipient rod cells behind it. In *Tipula* these are so deeply pigmented, that in the adult insect it is impossible to get a transparent section; although I have adopted the method described by Dr. Schaefer in the preparation of fine sections of the mammalian ovum,* of imbedding the specimen in cacao butter, and have cut sections of certainly not more than $\frac{1}{10000}$ of an inch in thickness. These have been examined with a sixteenth immersion of Nachet's.

The nearest approach to these deeply pigmented cells is seen in the red cones in the eye of the pigeon, described by Max Schultze,† or perhaps in the deeply coloured rods of the eel (Kühne).‡ But, unlike the latter, the pigmented parts of the eyes of insects, as far as my observations go, are unaffected by light, and the same observation has been made by Kühne with regard to the colour in the so-called rods in the eye of the lobster.§

Behind the corneal facet, and immediately in relation with it, each rod-cell has a minute highly refractive spherule; this is of an intense purple colour in the eye of *Vespa* and *Tipula*, but it is colourless in the eye of *Formica rufa*. It is exceedingly like the globule found between the inner and outer segments of the cones in many birds.

Behind the rod-cells a very remarkable structure is found which has hitherto escaped detection, except by Dr. Grenacher,‖ who describes it under the name retinula. As this term is, however, applied by him to

* "Proc. Roy. Soc." † "Archiv," Bd. iii.

‡ Kühne "Untersuch. aus dem Physiolog. Inst. der Univ. Heidelberg," Bd. i, Hft. i. § *Ibid*, hft. ii.

‖ "Zehender's Monatsblatt. Beigeheft," 1877.

structures in other insects which are perhaps very different, and as it tends at least to give an erroneous idea of its nature, I have preferred to call it the *facellus*.

The facellus consists of a cylindrical or fusiform bundle of seven or more—twelve at least in the ant—fusiform cells, each of which has a fine axial thread of highly refractive material. This thread is prolonged from the outer extremity of the facellar cell into the rod-cell layer above described. These threads pass also into the deeper structure beneath the facellus, which I have named the *stemon*.

The stemon is the large rod-like organ which connects the facellus with the ganglionic retina. The stemon of each central facet remains distinct through its whole course, but those of the peripheral facets unite in bundles of four or more near their inner extremities. In *Tipula* each stemon, whether simple or compound, divides at its inner extremity into several irregular fine branches which pass into stellate highly pigmented cells (ganglion cells ?). The inner processes of these cells connect them with the round cells of the ganglionic retina. In the ant and in the wasp, the stemonata do not divide at their inner extremities. In the latter I have not been at present successful in tracing the connexion of the stemon and the nervous structures beneath; but, in the former, each stemon is connected with the ganglionic retina by what appears at first sight to be a thick nerve fibre. From observations on the eyes of Lepidoptera, where similar thick nerve fibres exist, I conclude that, in both cases, these are compound fibres consisting of a large number of primitive fibrillæ. The ganglionic retina in the ant consists exclusively of small round cells; I have been unable to detect either stellate corpuscles or fusiform cells, which are so constant in this organ in insects.

In the third form of eye, the true compound eye, the facets are each provided with a single complex rod-like structure, which I have called the rhabdion, in accordance with the nomenclature proposed by Dr. Grenacher.

The nature of this structure is best seen in the eyes of the crepuscularian Lepidoptera. In the eyes of the Sphingidæ a single rhabdion rests on a distinct facellus, quite comparable, indeed almost identical, with that of the semi-compound eye. The rhabdion is separated from the corneal facet by a cone, described under the term crystal-cone (*krystal-kegel*). The facelli are continuous with thick nerve fibres, which are undoubted compound. These are gathered together into nerve trunks, the fibres from thirty or forty facelli being united into a single trunk, which branches at its inner extremity, and is connected with numerous ganglion cells.

The cone consists of eight cells—Four are superficial, that is, they are in contact with the corneal facet; these remain soft; they have been spoken of by previous writers as "Sempers' cells," and have been

confounded with sub-corneal nuclei of the eye in the Diptera and some other insects, but they are, I believe, very different structures, morphologically speaking. The other four form the hard cone, or, as I shall call it, the scleral cone. These remain distinct throughout the life of the insect, as the cone always splits most readily into four equal segments corresponding to the four primitive cells.

An examination of the eye in moths has shown that the segments of the scleral cone are prolonged as threads into the rhabdion. The threads are apparently viscous immediately after death, as they are liable to contract and form globular or pear-shaped drops, as the inner segments of the rods and cones of some vertebrates do, according to the observations of Max Schultze.*

The chamber in which the cone lies is usually lined with pigment cells. These are very remarkable in structure, reminding me of the retinal pigment cells of vertebrates; they are generally arranged round the apex of the cone, and give off numerous thread-like processes which surround the cone.

Although the compound eye in insects exhibits very considerable modifications in different families, the researches of Claperede† show that these are all developed from a condition closely resembling the type I have just described. The primitive zone consists of eight cells: four of which Claperede speaks of as the cells of "Semper," and four of which he terms cells of the cone. There is every reason to believe that the scleral structures are formed in the interior of the latter, which, in moths, become entirely converted into scleral tissue; the same occurs, according to Leydig,‡ in some Coleoptera, as *Cantharis melanura* and *Elater noctiluca:* I have observed the same thing in the eye of *Dytiscus*, and, as is well known, this condition appertains in *Hyperia*, amongst the Crustacea.

I think there is little doubt, from the observations of Leydig and Dr. Grenacher, that the eyes of most, if not of all, the pentamerous Coleoptera retain the primitive structure of the cone throughout life, and those of many Crustacea present cones which are either partially or entirely converted into scleral tissue.§

I propose to term these two forms of eye, proto- and sclero-conic.

Starting from the proto-conic eye: There are two extreme forms of deviation; in the one, the cone disappears, and its place is occupied by a slightly coagulable fluid. This is the case in the eyes of the heterocerous Diptera. In these insects four portions of protoplasm remain attached to the outer extremity of the rhabdion; further observations are needed in the development of these structures, and as to the origin of the fluid; but analogy, with the conditions in some of the

* "Archiv.," Bd. iii. † "Kol. Zeitsch.," Bd. viii.

‡ "Müller's Archiv.," 1855.

§ Newton, "Eye of the Lobster," "Quarterly Journ. Mic. Sc.," 1874.

Lepidoptera, renders it highly probable that the four bodies are the nuclei of the primitive cells of the cone, and that the remainder of these cells undergoes liquefaction. The four bodies in question are elipsoids, with their long axes at right angles to the plane of the cornea; they exhibit fine longitudinal striations. I have called the compound organ formed of these four bodies, the *tetrasome*.

In the other type of eye, the chamber contains a fluid formed by the liquefaction of a portion of the cells of the primitive cone; but a remarkable body is developed in the interior of these cells of a very complex nature. It consists of a tetrasome, formed of four minute highly refractive spheres, supported on a *tetraphore*. I think it probable that the tetrasome is formed from the nuclei of the four superficial cells of the cone, whilst the tetraphore appears to be a highly modified scleral cone formed in the interior of the deeper cells; but the whole question of the development of these parts is very difficult, and requires further investigation.

I have used the terms hydroconic and tetraphoric to designate these two forms of eye. The first is characteristic of the heterocerous Diptera and Libellulidæ; the second occurs in *Acridium*, and in the diurnal Lepidoptera, *Vanessa*, *Pieris*, *Colias*, and *Goneptrix*.

The rhabdion is either prismatic or cylindrical. In the eye of the pupa it is seen to be formed of four cells; but in the imago these are so closely united that they can no longer be recognized as separate structures. It contains axial longitudinal striæ, which appear to be the internal prolongations of the highly refractive cone or tetrasome.

In the Lepidoptera the rhabdion rests on a facellus, formed of seven fusiform cells, or, in some cases, of as many cylindrical rods, but in the Diptera, Dragon flies, and some saltatorial Orthoptera it is in immediate relation with the outer stellate ganglion cells of the ganglionic retina. In the Diptera the rhabdia of the two peripheral rows of facets are, however, united into bundles of four or more at their inner extremities; at least, this is the case in the Syrphidæ; and these bundles are surrounded by fusiform pigmented cells in such a manner that they somewhat resemble a facellus.

The Diptera have, however, a very remarkable layer in the ganglionic retina itself, which apparently represents the facellus in function at least; I have termed it the facelloid layer.

The accompanying diagram represents the axial structures connected with a single rod-cell, in the semi-compound eye, and with one segment of the cone or tetrasome in the true compound eye.

The rhabdia in the compound eye are surrounded in most insects by a close network of tracheal tubes, but in the Diptera these are replaced by sac-like trachea which fill the interspaces between the prismatic rhabdia; this arrangement has been described by Leydig.

The nervous structures of the retina and optic ganglia of the eyes of

insects are exceedingly difficult to make out; but I think I have succeeded in working out the retinal structures of *Eristalis* and *Agrion* in considerable detail. In the other insects in which I have examined the eye, the knowledge which I have been able to obtain of this portion of the nervous system, must be considered at present as fragmentary. In *Eristalis* there are from without inwards:—1. A double layer of large stellate ganglion cells; 2. A layer of small round nucleated cells; 3. The facelloid layer already referred to; and 4. A layer of stellate ganglion cells.

These parts are connected with a deep ganglion, which consists of several layers of fusiform cells by a decussating optic nerve, the fibres of which cross each other from above downwards, and from without inwards. The deep ganglion is connected by a distinct peduncle with the supra-œsophageal ganglion. All the structures of the ganglionic retina are supported by a fine neuroglia, which extends from a thick outer to a fine inner limiting membrane.

In *Agrion* the ganglionic retina differs from that of *Eristalis* in the absence of a facelloid layer, which is replaced by a triple layer of prismatic cells: the investigation of the nerve structures of this insect is rendered very difficult by the presence of a large quantity of dark pigment in the stellate connective cells of the neuroglia.

In *Vanessa* the facelloid layer of the retina is also absent, but in its place there are numerous layers of fusiform cells. In noctuid moths, or at least in some species, the nervous structures are obscured by the large quantity of deep black pigment which they contain. In the semi-compound, or, as I have termed it, the *microrhabdic* eye of *Tipula*, the nervous retina consists of, (1,) a layer of stellate ganglion cells; (2,) of several layers of round cells; and (3,) of several layers of fusiform cells. The greatest simplicity exists in the eye of *Formica*, in which all the structures of the nervous retina are absent except numerous layers of small round cells. I have not at present been able to make out any decussation of the nerve fibres connecting the deeper parts with the ganglionic retina in the insects with microrhabdic eyes, but the investigation is very difficult, owing to the great change of the plane in which the nerve tracts lie. I do not think, however, that any decussation exists, or I think I should have found indications of it.

The extent and curvature of the cornea and the size and curvature of the facets afford the most important indications as to the manner in which vision is accomplished. In the true compound eye, I think the structure indicates that J. Müller's theory of vision is the most probable; this is also Dr. Grenacher's view, and it is supported, as I shall now endeavour to show, by the curvature of the cornea and the size of the corneal facets in different insects, as well as in different parts of the same eye.

The semi-compound eye introduces no new difficulty in this theory, it is probable, I think, that more than a single luminous impression is received by the elements which are situated behind each facet, and that these correspond with portions of the field of vision which are remote from each other, the central rod cell of one facet corresponding to one of the peripheral rod cells of some other facet; the extreme complexity of the connexions between the cells of the ganglionic retina renders this view not improbable.

In order to determine the effect of the long fine highly refractive threads of the eyes of insects upon the light, I made some experiments on the transmission of light through fine threads of glass.

I took a capillary tube of glass $\frac{1}{500}$ of an inch in thickness, about $\frac{1}{100}$ of an inch in diameter, and an inch in length, placed it upright in a small trough of water under the microscope and examined it with an inch objective. I found that no light passed through the lumen of the tube, but that the section of the wall of the tube appeared brightly illuminated. I next placed a few fine glass threads, drawn from a glass rod, in the interior of the tube; these were as nearly as possible the same length as the tube and measured $\frac{1}{1000}$ of an inch in diameter. The upper end of each of these rods appeared as a brightly illuminated disk in the dark field; when the focus of the microscope was altered the disk enlarged, showing that the rays left the rod in a divergent direction; in some cases when the ends of the rods lay beyond the focus of the microscope, the disks of light exhibited grey rings, the result of interference.

When the lower ends of these rods were lenticular, or fused into a drop, or drawn into a cone, the phenomena were the same, and in all cases the action of an oblique pencil, even when the obliquity was very slight, was feeble as compared with that of a pencil having the direction of the axis of the rod.

These results are such as would be predicted on the undulatory theory; all the light passing into the rod, except very oblique rays, would be totally reflected, without any change of phase in the undulations, at the surface of the glass, whilst all except the axial rays would be very much enfeebled by numerous reflections and interference from the different lengths of the paths of the rays. I think threads of a highly refractive character immersed in a medium of a less refractive index, when less than $\frac{1}{5000}$ of an inch in diameter, would destroy the effect of rays of only very small obliquity by interference.

In order to determine the effect of the pigment, I covered the exterior of some glass rods of $\frac{1}{500}$ of an inch in diameter with black varnish, and I then found it impossible to transmit any rays of even the slightest obliquity through half an inch of such a rod.

From these facts I think it may be concluded that it is probable that the highly refractive structures may be regarded in the light of

luminous points, which serve as stimuli in exciting the recipient protoplasm in which their ends are imbedded.

The focus of the facet when this is lenticular, in all the insects which I have examined, is situated considerably deeper than the outer end of the rhabdion and below the surface of the rod cells in the microrhabdic eye, so that even for objects as close as $\frac{1}{10}$ of an inch to the cornea, we have to deal with convergent rays and not with a focal point. This indicates some mode of nerve stimulation other than the union of homocentric pencils, in a point beneath the compound cornea in relation with the recipient elements. Considering the small size of the parts, I think it quite possible that we must look to the phenomena of interference for the explanation; at least, they must play an important part in the phenomenon.

Whatever may be the manner in which vision is accomplished, the size of the corneal facets and the general curvature of the cornea renders the theory of J. Muller highly probable. It is true that Claperède has expressed the reverse opinion, but I shall endeavour to show that he has done so on insufficient data. According to his calculation, a bee should be unable to distinguish objects of less than eight inches in diameter at a distance of twenty feet from it. This calculation is based on the idea that the acuity of vision in this insect is the same in all parts of the field of vision, and that the general surface of the common cornea is approximately a segment of a sphere. This is not the case, for the angles subtended by the adjacent facets in the centre of the cornea, which is considerably flattened, is not more than half a degree at the most; so that on J. Muller's theory, supposing each facet to give rise to only a single luminous impression, the bee should be able to distinguish objects of about two inches in diameter at a distance of twenty feet, an acuity of vision quite equal to account for all the phenomena of vision in bees.

I have measured the curvature of the cornea of a number of insects, with a view to determining the angles made by the lines of vision drawn from the centre of adjacent facets. This is done in the following manner:—A magnified image of the cornea is thrown on a sheet of white paper, by means of a microscope and camera lucida, and the curve of its profile drawn; in this way I have found the principal meridians. These curves approach more or less closely to an epicycloid.

It is easy with such curves and the size of the corneal facets to determine the angles made by adjacent facets. The angles vary inversely as the radius of curvature, and, therefore, the acuity of vision varies directly as the radius of curvature when the diameter of the facets remains the same, and inversely as the diameter of the facets when these vary in size. In many insects, as *Tabanus*, the peripheral facets of the cornea are twice or three times the diameter

of those in the centre, and the radius of curvature is very short at the extreme periphery.

In most insects the acuity of vision determined in this manner diminishes very rapidly at the periphery of the field. In the centre of the field it enables them to perceive, as distinct, objects which subtend one degree. In *Æschna grandis* the sharpness of vision is much greater, as the adjacent facets make an angle of only eight minutes with each other. This is the least angle I have measured in any insect, but I have no doubt, from the nature of the curve forming the meridians of the eye in the great dragon flies, that a small part of the centre of the field has a much greater acuity of vision than this; in the wasp the angle subtended by the smallest visual perceptions is twice as great as in *Æschna;* and in the bee it is half a degree.

The direction of the visual line, or the line perpendicular to the compound cornea in the centre of the field of most acute vision, varies in different insects. In the predaceous kinds it is directed forwards in the plane of the body, or forwards and outwards, making an angle of 30° between the visual lines of the two eyes. In the pollen feeders it is directed downwards as well as forwards and outwards.

The size of the corneal facets varies in different insects from $\frac{1}{2000}$ to $\frac{1}{750}$ of an inch in diameter. Their size, except in a few insects, is dependent on the size of the insect, the largest insects having the largest and the smallest the smallest corneal facets. From this it follows that the vision of large insects is more perfect than that of small ones, except where the curvature of the cornea is very flat. This corresponds with the manner in which the insects fly. For instance, the small Diptera fly round in small circles, and seldom leave the place in which they first attain their adult condition, except when borne away by currents of air, whilst the larger species take long flights when disturbed or in search of food. The experiments of Muller and others have shown that the direction and length of the flight of insects depends largely on the visual powers of the insect. The forward flight of *Tabanus* and of many flies corresponds with the direction of their visual line, and the same may be said of the lateral movements of the large dragon flies.

The mimicry of insects, especially that between the Diptera and the Hymenoptera is sufficiently close to be a protection or advantage to the unarmed insect, and is such that it would render the one indistinguishable from the other, or the two insects would be scarcely to be distinguished under conditions of vision equal to those with which the insects appear to be endowed except at very close quarters.

In the extreme periphery of the cornea the adjacent facets make an angle of from 30′ in wasps and some other Hymenoptera, to 12° in many insects. In the microrhabdic eye of *Tipula* the curvature of the common cornea approaches the segment of a hemisphere.

In most insects the field of vision has a small region common to the two eyes in the vicinity of the mouth; it is chiefly developed in the predatory species, and probably serves in determining the distance of their prey from their mandibles.

III. "Measurements of Electrical Constants. No. II. On the Specific Inductive Capacities of Certain Dielectrics." By J. E. H. Gordon, B.A. Camb. First Series. Communicated by Professor J. Clerk Maxwell, F.R.S. Received March 9, 1878.

(Abstract.)

The author has, under Professor Clerk Maxwell's directions, carried out some measurements of specific inductive capacities by a new method. The essential features of it are:—

(1.) It is a zero method.

(2.) The electrified metal plates never touch the dielectrics.

(3.) No permanent strain is produced or charge communicated, as the electrification is reversed some 12,000 times per second.

The potentials of the electrified plates were about equal to that of 2,000 cells.

The following are the results obtained:—The solid dielectrics were plates 7 inches square, and from $\frac{1}{4}$ inch to 1 inch thick.

Dielectric.		Specific Inductive Capacity.		
Ebonite, 4 slabs of thickness, $\frac{3}{4}$, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$ inch, about.	(1.)	1·5593	Mean	1·56215
	(2.)	1·5553		
	(3.)	1·5671		
	(4.)	1·5669		
Best quality gutta percha				1·5939
Chatterton's compound				1·6080
India-rubber { black				1·5502
India-rubber { vulcanised.....				1·5988
Sulphur.....				1·6127
Shellac				1·6362
Solid paraffin, sp. gr. ·9109 at 11° C. Melting point 68° C. 6 slabs, each $\frac{3}{4}$-inch thick, about.	(1.)*	1·4986	Mean.....	1·49753
	(2.)	1·4943		
	(3.)	1·4920		
	(4.)	1·5033		
	(5.)	1·4936		
	(6.)	1·5034		

* These results are corrected for cavities in the plates. The mean of the uncorrected determinations is 1·4864.

Dielectric.				Specific Inductive Capacity.
*Bisulphide of carbon				1·4474
Chance's optical glass. Slabs nearly 1 inch thick.	Double extra dense flint			1·6840
	Extra dense flint			1·6727
	Light flint			1·6677
	Hard crown			1·6872
Common plate glass, 2 slabs, each 1 inch thick, about.	(1.)	1·6933	Mean....	1·6918
	(2.)	1·6903		

The author suggests that the fact that all his results are much lower than those obtained by previous experimenters may perhaps be explained on a supposition that the specific inductive capacity of dielectrics increases from an inferior to a superior limit during the first small fraction of a second after the commencement of the electrification. He discusses this question at some length in his paper.†

An expression of thanks to Professor Maxwell, for his close superintendence of the work, concludes the paper.

IV. "On the Placentation of the Apes, with a Comparison of the Structure of their Placenta with that of the Human Female." By William Turner, M.B. Lond., F.R.S., Professor of Anatomy in the University of Edinburgh. Received March 11, 1878.

(Abstract.)

The introductory chapter of this memoir consists of a summary of the observations of John Hunter, Rudolphi, Breschet, Owen, Huxley, Rolleston, Ercolani, and Kondratowicz, on the form and structure of the placenta in the apes. The author then gives a detailed description of his dissection of the gravid uterus and placenta of a *Macacus cynomolgus* well advanced in pregnancy. He then enters into a detailed comparison between the form and structure of the placenta in the ape and that of the human female, in the course of which he records a number of original observations on the structure of the human placenta. Attention is more especially drawn to the comparative structure of the decidua vera and serotina; to the prolongations of the decidua serotina into the interior of the placenta; to the arrangement, structure, and mode of origin of the intra-chorionic and sub-chorionic cells; to the arrangement of the arteries and veins of the placenta;

* I cannot vouch for the exact accuracy of this determination, as the method of experimenting on liquids is not yet quite perfected.

† Note added April 7th. Compare Ayrton and Perry, on the "Viscosity of Dielectrics," read March 21, 1878.

to the intra-placental maternal blood-spaces; the form and structure of the chorionic villi: and to the relation of the maternal blood to the capillaries of the villi.

The result of this comparison proves that the *Macacus* and human female closely correspond in the form of the uterus, and in the arrangement of the fœtal membranes, and that they both possess a discoid placenta, which in the *Macacus* is divided into two lobes, but is not so divided in the human placenta. In the arrangement and relative position of the constituent parts of the placenta they also correspond; and although some differences of detail in the characters of some of the structures occur, yet, in the main features of construction, makroscopic as well as microscopic, they have a close resemblance to each other.

V. "On the Thermo-Electric Properties of Liquids." By G. Gore, LL.D., F.R.S. Received March 12, 1878.

(Abstract.)

In this communication, the author has described an improved apparatus for examining the thermo-electric properties of liquids, by the use of which, with the precautions stated, all sources of error in such experiments appear to be removed; he has also described a number of experiments he has made with it, and the results obtained.

By employing a sufficient number and variety of electrically-conducting solutions, of acids, salts, and alkalies, in those experiments, he has discovered several exceptions to the usual effect he had formerly obtained, viz., that acid liquids are thermo-electro-positive, and alkaline ones thermo-electro-negative; and has sketched a diagram representing the thermo-electric behaviour of heated platinum in three of the exceptional liquids.

Reasoning upon the satisfactory results obtained, he concludes:—1st. That the electric currents are not produced by chemical action; 2nd. Nor by a temporary disassociation of the constituents of the liquid; 3rd. Nor by the action of gases occluded in the metals; 4th. But that they are produced purely and solely by the heat, and that heat disappears in producing them; 5th. That they are immediate or direct effects of the heat, and that aqueous conducting liquids, therefore, possess true thermo-electric properties: 6th. That the current is a result of a difference of thermic action at the surfaces of the two pieces of metal: 7th. That it is a product of a suitable molecular structure of the liquid, a change of such structure resulting from alteration of temperature, and a direct conversion of heat into electricity; and 8th. That the circumstance which is most influential in

enabling heat to produce the currents, and most determines their direction and amount, is a suitable molecular structure of the *liquid*.

By means of the apparatus and process described, he has discovered irregular molecular changes in several of the liquids examined; and as molecular changes are the bases of various physical and chemical alterations, he suggests the use of this apparatus and method as a new one for discovering anomalous molecular alterations, and other coincident physical and chemical ones, in electrically conducting liquids; and for detecting differences of electric potential between metals and liquids at different temperatures.

By reasoning upon the different results obtained, he concludes also as probable, that when a piece of metal is simply immersed in a suitable liquid, a change of temperature occurs; and this (if correct*) is a parallel fact to that of the production of electricity by simple contact only. The results also support the contact theory of voltaic electricity.

The paper concludes with several suggestions of new lines of research suggested by the experiments, one of which is the construction of a new thermo-electro-motor.

Presents, March 7, 1878.

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* Since writing the paper the Author has proved, by experiment, that when a sheet of platinum is immersed in various saline, alkaline, and acid liquids, a slight rise of temperature takes place; the solutions already employed, in which such a result occurs, are enumerated.

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The Author.

"Researches in Spectrum Analysis in connexion with the Spectrum of the Sun." By J. N. Lockyer, F.R.S. Received November 17, 1877. Read January 24, 1878.*

In a map of the solar spectrum, containing a greatly increased number of lines, such as that upon which I am now engaged, it is possible to seek under favourable conditions the coincidence or non-coincidence of lines due to elements hitherto undetected in the solar atmosphere, on account of their existing in quantities insufficient to give very marked spectral lines.

A search has accordingly been made, on the principles laid down in previous communications, for those elements which contain in their spectra long and well-characterised lines in the photographic region.

Although the evidence cannot be said to be complete until the whole spectrum has been examined, it is not too early to adduce the following evidence as to the existence of several additional metals in the sun's reversing layer, and the probable existence of others.

The results obtained up to the present time are shown in the following tables, premising that in the first paper of this series the metals considered as being solar as the result of the labours of Kirchhoff, Ångström, and Thalèn, together with the considerations there brought forward regarding the length of the lines, were as follows :—

Na	Fe	Ca	Mg	Ni
Ba	Cu	Zn	Cr	Co
H	Mn	Ti	Al	

* See *ante*, p. 49.

† "Phil. Trans.," 1873, p. 253.

Metals the presence of which in the Sun is confirmed.

Name of Metal.	Approximate W.L. of lines reversed in the solar spectrum.	Observations.	By whom previously mapped.	Particulars from Thalèn's Tables.		
				Metal.	Wave length.	Intensity. 1 = brightest.
Strontium	4029·60	Absent from spectre normal near long Mn line ..	New line.			
	4076·77	A line at 4076·9 in spectre normal assigned to Ca	Thalèn	Sr	4078·5	1
				Ca	4077·0	3
	4215·00	In spectre normal assigned to Ca (W.L. 4215·40)	Thalèn	Sr	4215·3	1
				Ca	4215·3	1
	4607·5	A line at 4604·5 in spectre normal assigned to Ca	Thalèn	Sr	4607·5	1
				Ca	4607·5	4
Lead	4019·28	Absent from spectre normal	New line.			
	4056·80	An unassigned line near required position (W.L. 4057·25) in spectre normal	Thalèn	Pb	4058·0	4
	4061·25	Absent from spectre normal	Thalèn	Pb	4062·5	4
Cadmium	4677·00	An unassigned line at W.L. 4676·5 in spectre normal	Thalèn, Kirchhoff	Cd	4676·8	1
	4799·0	An unassigned line at W.L. 4698·9 in spectre normal	Thalèn, Kirchhoff	Cd	4799·0	1
Potassium	4042·75	Absent from spectre normal	New line.			
	4046·28	Absent from spectre normal	New line.			
Cerium	3928·7	Absent from spectre normal	New line.			
	4012·0	Absent from spectre normal	New line.			
Uranium	3931·0	Absent from spectre normal	New line.			
	3943·0	Close to Al line	New line.			
	3965·8	Absent from spectre normal	New line.			

Metals probably present in the Sun.

Name of Metal.	Approximate W.L. of lines reversed in the solar spectrum.	Observations.	By whom previously mapped.	Particulars from Thalèn's Tables.		
				Metal.	Wave length.	Intensity. 1 = brightest.
Vanadium....	3901·3		New line.			
	3909·3		New line.			
	3989·65		New line.			
	3992·5		New line.			
	3997·9		New line.			
	4379·0	Assigned by Ångström to Ca.....	Thalèn	Ca	4379·1	4
				Va	4379·0	1
	4384·0	Absent from Ångström's map	Thalèn	Va	4384·0	1
	4389·0	Assigned by Ångström to Ca.....	Thalèn	Ca	4389·4	4
				Va	4389·0	2
	4407·5	Assigned by Ångström to Ca.....	Thalèn	Ca ..	4407·0	5
				Va ..	4407·5	1
Palladium....	3893·0		New line.			
	3958·0		New line.			
	4787·0	Assigned by Ångström to Fe.....	Thalèn	Pd	4787·0	3
				Fe	4785·8	5
	4817·0	Not allocated by Ångström	Thalèn	Pd	4817·0	3
	4874·0	A line near required position assigned by Ångström to Fe	Thalèn	Pd	4874·0	3
Molybdenum.	3902·0	Very near Fe line	New line.			
	4576·0	Not allocated by Ångström	New line.			
	4706·0	Assigned by Ångström to Fe.....	Thalèn	Mo	4706·5	4
				Fe	4706·5	5
	4730·0	Assigned by Ångström to Fe	Thalèn	Mo	4730·5	4
	4818·0 ?	Absent from Ångström's map	Thalèn	Mo	4818 0	4
	4829·0 ?	Line near assigned by Ångström to Ni	Thalèn	Mo	4829·5	4
				Ni	4828·4	5
				Ni	4830·2	5

Metals probably present in the Sun—*continued.*

Name of Metal.	Approximate W.L. of lines reversed in the solar spectrum.	Observations.	By whom previously mapped.	Particulars from Thalèn's Tables.		
				Metal.	Wave length.	Intensity. 1 = brightest.
Indium	4101·0	Apparently coincident with Fe line in solar spectrum	Thalèn	In	4101·0	1
	4509·0	Absent from Ångström's map	Thalèn	In	4509·5	1
Lithium	4603·0	The line in Ångström's map is placed at W.L. 4601·7 and no metal assigned	Thalèn	Li	4602·7	1
Rubidium	4202·0	The line in Ångström's map is placed at 4201·0 and made winging the adjacent Fe line	Thalèn	Rb	4202·0	2
Cæsium	4554·9	Assigned by Ångström to Fe, no line in Thalèn in this position	New line.			
	4592·0	Absent from Ångström's	New line.			
Bismuth	4722·0	Absent from Ångström's map	Thalèn	Bi	4722·0	1
Tin	4524·0	Not in spectre normal, a neighbouring Bi line at 4524·4 is shown	Thalèn	Sn	4524·0	1
Lanthanum	3948·2	Not in spectre normal	New line.			
	3983·0	Not in spectre normal	New line.			
	3995·04	Not in spectre normal	New line.			
Glucinum	3904·75	Not in spectre normal	New line.			
Yttrium or Erbium	3949·6	Not in spectre normal	New line.			
	3981·8	Not in spectre normal	New line.			

It is important to bear in mind that the lines recorded in the foregoing tables are in most cases the very longest visible in the photographic region of the respective spectra. In some cases they are limited to the region 39–40, which I have more especially studied. So that the fact of their being reversed in the solar spectrum must be considered as the strongest evidence obtainable in favour of the existence in the sun of the metals to which they belong, pending the complete investigation of their spectra.

Where, however, there is only one line, as with Li, Rb, &c., the presence of these metals in the sun's reversing layer can, for the present, only be said to be probable. Neither must it be forgotten that in addition to the long lines which a spectrum may contain in the red, yellow, or orange, long lines may exist in the hitherto unexplored ultra-violet region, so that the necessity for waiting for further evidence before deciding finally upon the presence or absence of such metals in the sun will be rendered obvious.

It will be thought remarkable that if the long lines of such metals as lithium and rubidium are found in the photographic region of the spectrum, the long lines (Li, W.L. 6705; Rb, W.L. 6205 and 6296), should have escaped detection.

To this it may be replied that, although these red lines may be apparently the brightest to the eye, it by no means follows they are the longest, since they are situated in a part of the spectrum which affects the visual organ more strongly than the photographic region does. It is possible also that the reasoning I have lately used in a paper communicated to the Society on the spectrum of calcium may be applied in these cases.

Since a sensitized film is affected by some rays more strongly than by others, in determining the lengths of lines from a photograph, it is not fair to compare together portions of the spectrum separated by too great an interval.

Furthermore the fact of these red lines having been overlooked in the solar spectrum is not conclusive proof of their absence, inasmuch as this portion of the spectrum is both brighter and less refrangible, and a greater degree of dispersion would be necessary when prisms are employed to render visible faint dark lines which are easily detected in the photographic region.* I hope to be able to make special search for these lines on some future occasion.

For metals having long lines in the green a special search was made. The long thallium line (W.L. 5349) was photographed, but no distinct evidence of a corresponding solar line was obtained.

Two long silver lines were found also, about W.L. 4018 and 4212,

* It is significant that there is a dark line near the position of the Li line both in Ångström and Kirchhoff's maps not assigned to any metal.

but these lines, which are reversed, are of such great width that it is at present impossible to say whether they are coincident with lines in the solar spectrum.

April 4, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

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

Notice was given that, with a view to facilitate observation of the Solar Eclipse of July 29, the Pennsylvania Railroad Company will convey observers, being private persons, from Philadelphia or New York to Denver and back at reduced fares.

The following Papers were read:—

I. "On the Development of the Parasitic Isopoda." By J. F. Bullar, B.A., Trinity College, Cambridge. Communicated by Dr. Michael Foster, F.R.S., Prelector of Physiology in Trinity College, Cambridge. Received March 14, 1878.

(Abstract.)

The paper contains an account of some points in the development of the *Cymothoa œstroides* and *C. parallela* of Milne-Edwards.

The work was mainly carried on in the Zoological Station at Naples, and the author takes this opportunity of returning his best thanks to Dr. Dohrn and Dr. Eisig for the kind way in which they forwarded his researches.

The eggs were prepared in the way described by Bobretzky in his paper on the development of *Oniscus murarius* ("Zeit. für Wiss. Zool." Bd. xxiv), namely, by heating them in water, and then hardening them, first in bichromate of potash, and then in alcohol, beginning with 70 cent. and gradually increasing the strength to absolute. The sections were stained with Kleinenberg's hæmatoxylin and mounted in Canada balsam.

The eggs when first laid are surrounded by a single membrane.

The earliest stages of segmentation were not observed; the first described is that in which a circular patch of cells has appeared at one pole of the egg. The cells are of considerable size and contain very large granular nuclei; in the centre of the patch they are polygonal, and more than one layer deep, but at the edges they are flattened and form a single layer.

The blastoderm gradually spreads over the yolk, the cells on the

dorsal surface being very thin and difficult to recognise, while on the opposite side they form a thickened patch (the future ventral wall of the embryo), which elongates, and soon shows traces of segmentation.

The procephalic lobes appear first, the other segments are formed in order from before backwards.

A second membrane is now formed round the egg.

The epiblast of the procephalic lobes soon becomes thickened, to form the cerebral ganglion. At the most anterior end there is a separate mass on each side, but in front of the mouth the thickening is continuous right across; it extends backwards, along the median line, to form the ventral nerve-cords and ganglia.

The mouth and, rather later, the anus appear as involutions of the epiblast.

Beneath the epiblast a second layer of scattered cells appears.

The limbs arise as hollow protuberances of the epiblast, filled with cells from the lower layer.

On the dorsal surface a peculiar organ, homologous with the "micropyle apparatus" of the Amphipods, is developed. At a later stage it is situated in the first thoracic segment. It disappears before the adult form is reached.

Just behind the mouth involution a solid mass of cells appears, from which the liver will be developed.

The nervous system, which at first consisted of a continuous mass of epiblast cells, becomes differentiated into a cellular and a fibrous portion. The external layer of cells or epidermis is separated from it, and it becomes segmented so as to form a chain of separate ganglia.

The epiblast covering the head is thickened at a certain point on each side, and from these thickenings the eyes are developed.

The solid masses of cells representing the liver become converted into three cæcal tubes, on each side of the body, opening to the yolk, and quite distinct from both the fore and hind guts.

The heart appears above the hind gut in the abdomen.

The hind gut grows forwards very rapidly, and comes nearly into contact with the fore gut. The liver cæca increase in size and become filled with oil-drops derived from the yolk.

It is now possible to demonstrate, by dissection, that the yolk is surrounded by an exceedingly thin membrane, continuous with the walls of the liver cæca, and opening into the point of junction of the fore and hind guts. This membrane appears to contain nuclei.

As development proceeds the yolk gradually disappears, and the membrane surrounding it disappears also.

The whole of the alimentary canal of the adult is formed from the fore and hind guts.

In the paper an attempt is made to prove the truth of the suggestion, originally put forward by Dr. Dohrn, that the yolk membrane is

the morphological representative of the mid gut, and that the liver really arises as a diverticulum from it.

Four varieties of embryos, taken from animals answering the description of *C. œstroides*, are described; but as it was found impossible to make out any differences in the adults, the question whether these varieties represent distinct species or polymorphic forms is left undecided.

II. "On the Determination of the Constants of the Cup Anemometer by Experiments with a Whirling Machine." By the Rev. T. R. Robinson, D.D., F.R.S., &c. Received March 14, 1878.

(Abstract.)

In his description of the cup anemometer (Transactions Royal Irish Academy, Vol. XXII), Dr. Robinson inferred from experiments on a very limited scale with Robins' whirling machine, that the ultimate ratio of the wind's velocity to that of the centre of the cups =3. Some recent experiments by M. Dohrandt show that this number is too great; but as some of the details appeared objectionable, and as they did not include all the necessary data for determining the constants, the author was desirous of repeating them. He was enabled to do this by a liberal grant from the Royal Society, and the results are given in this paper.

After describing the apparatus and the locality in which it was established, he proceeds to explain the conditions of an anemometer's action. Considering only two opposite cups, and supposing them in motion, the pressure on the concave surface is as that surface and the square of the resultant of the wind's velocity V and v, that of the anemometer, and as a, the pressure of an unit V on the cup normal to the arm.

This is opposed, 1. By the pressure of a similar resultant on the convex surfaces, and $\acute{a}$, another coefficient, also normal to the arm, but quite different from a; 2. By various resistances depending on v^2; and 3. By the friction of the machine estimated at the centres of the cups.

a and $\acute{a}$ are functions of V, v, and θ, the angle which the wind makes with the arm, but it is impossible to determine them *à priori* in the present state of hydrodynamics. It is, however, obvious that if V be constant, the mean values of v, a, and $\acute{a}$ through one revolution will soon also become constant, and as the mean impelling and resisting forces balance each other, the condition of permanent motion is expressed by an equation of the form $a\mathrm{V}^2-2\beta\mathrm{V}v-\gamma v^2-\mathrm{F}=0$; or $\mathrm{V}^2-2x\mathrm{V}v-yv^2-\frac{\mathrm{F}}{a}=0$ (I), which, if the constants are known, gives

V in terms of v and F. Conversely, if we have a sufficient number of values of V, v, and F, we can determine α, β, and γ.

Unfortunately, there is much difficulty in obtaining some of these values. We cannot produce wind of known velocity, and must substitute for it the translation of the anemometer through the air with a known speed.*

The most convenient mode of doing this which occurred to him was the attaching the anemometer to a whirling machine. In this case, however, the rotation of the apparatus causes an air-vortex, whose motion must be subtracted from that of the arm to give what is assumed as the effective V. The means by which this vortex current was measured are described; they show that it is exceedingly irregular, and that it is accompanied by a radial current still more disorderly. We get v with sufficient accuracy, but it is otherwise with F. Of this there are four kinds acting in these experiments. 1. That due to the weight of the moving parts of the anemometer; 2. That caused by the action of a brake, intended to diminish v in respect of V; 3. That produced by the lateral pressure of its axle on its supports, produced by the wind, &c.; and, 4. That due to the centrifugal force arising from the circular track of the anemometer's centre, which in the actual arrangement pressed the axle outwards. The modes of measuring these are described; but this part of the work proved far from satisfactory. The ground where the apparatus was established was affected by tremors from the action of machinery, which made all these frictions variable; and as he had the use of it for a very limited time, it was impossible to repeat the work with the precautions taught by experience.

The constant a was determined by connecting a strong clock-spring with an arm of the anemometer and that of the whirling machine. When the whirl was rotated the anemometer tended the spring till its elastic force equalled the air's pressure on the cups. Then a brake-friction was applied much beyond the power of the spring, which kept the anemometer in its place; V was determined, and the tension given by a graduated circle fixed on the axle. Then $a=\frac{T-F}{V^2}$.

These measures showed that equation (I) contained no notable term

* In 1845 it occurred to Dr. Robinson to carry an anemometer on a railway, for which full opportunity was offered by a valued friend, Mr. Bergin, then Secretary of the Dublin and Kingstown Railway. He gave it up after carefully considering the disturbing influences and the precautions that were required. The space to be traversed should have no curves; should be perfectly unenclosed; should not be very long; and at each end of it an anemometer should be established to keep record of any wind. The experimental instrument should be 20 or 30 feet before the engine, so as to be clear of the air which it drives before it, and should be carried by a platform so formed as to present little resistance, and 10 or 12 feet above it.

of V except the square; secondly, that with cups of a given size a is not changed by varying the arms from 24 to 12; and, thirdly, that it is as the area of the cups.

Five instruments were used. No. I, cups 9 inches, arms 24, like the Kew ones; No. II, cups 4 inches, arms 24; No. III, cups 9 inches, arms 12; No. IV, cups 4 inches, arms 12; No. V, cups semi-cylinders, 9 inches by 9. The results with these are given in tables which show along with V, v, and W the vortex current, the frictions 1, 2, and 4, the air's density, and $\frac{V}{v}=m$. This last is seen to differ in each anemometer, and to be variable in each, ranging from 21·58 to 2·32. It increases with F and decreases as v increases in such a manner as shows that it will remain finite even when v is quasi-infinite.

Putting (I) in the form $\frac{F}{V^2}=a-\frac{2\beta v}{V}-\frac{\gamma v^2}{V^2}$, or (II) $\eta=a-2\beta\xi-\gamma\xi^2$, and treating those for each instrument by minimum squares, he got values for a, β, and γ, which, however, were unsatisfactory.

Dividing the 40 belonging to No. III into three groups, in the first of which are all whose $v<5$, in the second those from 5 to 9, in the third those >9, each gave discordant values for the constants. Those of a least so, those of γ most; the latter, indeed, rambled so much that no reliance could be placed on them. The matter was not mended by combining the entire. Thinking this discordancy might arise from equ. (I) containing a term δv, he tried this, but with a result so much worse that such a term, if it exist, can have no sensible influence. The results for the other instruments were similar. In fact, the method of minimum squares applies very imperfectly to a case like this, where the coefficients of the unknown quantities and the absolute terms are themselves affected with errors. Besides this, in the final equations of this process the coefficients of γ and β are so much less than those of a that they, especially γ, must be less accurately determined. It is also to be noted that these constants may be changed within certain limits, and still satisfy the equations approximately.

It was, however, suggested to him by Professor Stokes that, as equ. (II) has only two variables, η and ξ, it could be plotted on a plane surface, and this gave valuable information. The plottings for the five instruments are given, and show distinctly both the general agreement of (I) with the observations and the cause of the discordances.

Though in all the dots are much scattered, yet through a large portion of each the general direction is a right line with (in some cases) a barely perceptible downward curvature.

Since the curvature is nearly as γ this last must be very small, and assuming it=0, the equation $aV^2-2\beta Vv-F=0$ will be sufficiently accurate. Towards the vertex of the curves (where v is small) the

dots are so straggling that nothing can be made of them. In No. II, and still more in No. IV, they show that the frictions were considerably astray. Guided by these indications, and assuming for a nine-tenths of his measures of that constant, he deduced for Nos. I and III values of β and γ so nearly equal as to make it probable that their means would satisfy both. This would give $x=1{\cdot}1282$; $x^2+y=z=1{\cdot}340$. The positive root of (I) gives $V=v\left\{x+\sqrt{z+\frac{F}{av^2}}\right\}$. (III).

Computing $\acute{V}$ from this, we find $\Delta\acute{V}=$obs.$-$calc., of which tables are given for the five anemometers. As might be expected from the plottings, they are not very close, but show no systematic deviation from the law denoted by (I). So it may be assumed exact for all practical purposes through a range of V from 5 to 42 miles, and of F from 113 to 3277 grains. For No. III the probable error$=\pm 0{\cdot}45$. In both the errors are less on the hypothesis $\gamma=0$. In No. II these mean constants fail, but others deduced for it represent the series, though not so well as in the preceding; here also $\gamma=0$ is not inferior. In No. IV the frictions seem to have been deranged so much that the entire series cannot be well represented by any constants. Circumstances detailed in the paper account for this. No. V, cylinder cups, is the best of all. If, as seems probable, x and z are the same for all hemispherical anemometers, the difference between their indications will depend solely on $\frac{F}{a}$, and using the values given above, the limiting value of $m=2{\cdot}286$, instead of 3. Though if these experiments were repeated with Dr. Robinson's present experience, and in an undisturbed locality, better results might be obtained, yet the errors of the vortex current would still cause uncertainty; and he intends to try another plan.

The anemometer No. I, with its apparatus duly altered, is now erected on the roof of the dwelling-house 22 feet from the Kew one also there, to which it is exactly similar. Denoting the latter as S (the standard one), the other, E, is to be loaded with a brake friction, which will make its v less than that of S; when this has gone on long enough to ensure that an equal amount of wind has passed each instrument, a larger brake friction is applied to E. We shall thus have three equations (1), but four unknown quantities, a, x, V, y. a, however, is certainly known nearly by the measures already made. F also can now be measured with far greater precision. The chief difficulty to be feared is the unsteadiness of the wind during each experiment; but as the time of each revolution of the two anemometers is recorded on the chronograph, it will be possible to eliminate this element of doubt by selecting those times which have a given ratio.

III. "On the Action of Ozone on Nuclei." By CHARLES TOMLINSON, F.R.S. Received March 14, 1878.

After the reading of my paper on the 21st ultimo, Professor Stokes was so good as to suggest that some of my experiments should be repeated with ozone, prepared by the action of a coil, instead of that of phosphorus.

Professor Guthrie was so kind as to furnish me with a couple of bottles of ozone, prepared by sending oxygen slowly through Wills's generator in connexion with an induction coil.

The ozone was used soon after it was prepared, and in the following manner:—

Oil of cajuput was distilled, and the fresh distillate was found to be inactive on a supersaturated solution of sodic sulphate (3 to 1), although it was repeatedly shaken up with the solution. The newly-distilled oil was poured into one of the bottles of ozone, and shaken up with it, and then left for about fifteen minutes. It was dropped into nine flasks of the solution just named, and was active in all. In some of the flasks the solution became solid as soon as the ozonised oil reached the surface; in others, immediately on shaking the flask, or after a short interval of repose; while, in a third set, after adding the oil, the axis of the flask being brought into a nearly horizontal position, the flask was made to revolve slowly, when the solution solidified against the side, so as to form a kind of lining to it.

On the 12th March the wind was N.E., and the ozone in the air was very active on test paper. A paraffin oil of high boiling point was distilled, and specimens of the fresh distillate were powerfully active on a solution of sodic sulphate (2 to 1). A similar oil distilled during a S. or W. wind, as noticed in a former note, was inactive.

To a solution of sodic sulphate (3 to 1), containing oil of cajuput in an inactive condition, a solution of hydric peroxide was added, but it had no effect in rendering the oil active. The flask was shaken every day during a week, and the only effect was to liberate bubbles of gas. On adding to this flask a drop or two of the ozonised oil, the solution immediately became solid.

(23rd March.) The inactive distillates of cajuput and paraffin oils, shaken up several times during about half-an-hour with pure oxygen gas, became active. Test papers, suspended in the bottles, showed the presence of ozone. Washed castor oil, similarly treated, remained inactive; but shaken up with ozone, and left in contact with it for some hours, became active.

I repeated the experiment, described in a former note, on the activity of charcoal, on which Pellogio founds his theory of absorption. Pieces of box-wood, buried in sand, were heated in a crucible during

some hours. When cold, and just taken out of the sand, they were found to be inactive (except that they liberated a portion of the modified salt after the manner of the inactive essential oils); but by exposure to the outer air (wind N.E.) they became active in about ten minutes. Cocoa-nut shell charcoal requires a longer exposure to become active.

Hence the activity of charcoal does not depend on absorption, but on the condition of the air contained within its pores.

IV. "Notes on Physical Geology." By the Rev. Samuel Haughton, M.D. Dublin, D.C.L., Oxon, F.R.S., Professor of Geology in the University of Dublin.

April 11, 1878.

Dr. W. FARR, Vice-President, in the Chair.

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

Dr. William Carmichael McIntosh was admitted into the Society.

The following Papers were read:—

I. "The Acceleration of Oxidation caused by the Least Refrangible End of the Spectrum." By Captain Abney, R.E., F.R.S. Received March 16, 1878.

Preliminary Note.

In a paper contributed to the Philosophical Magazine in January last, I expressed an opinion that Chastaing's idea regarding an acceleration of oxidation being caused by red light might prove true in regard to the oxidation of the photographic image, and elsewhere* that Becquerel's coloured spectra and Draper's reversed spectra might be explained on the same principles. Owing to want of time until this last week, I have been unable to investigate the matter, but can now affirm that such is the case as regards oxidation of the photographic image.

Silver bromide, free from contamination, emulsified in collodion, which had been previously carefully purified, was spread on a plate,

* "Treatise on Photography." Longmans. Page 225.

and exposed to diffused light. It was then submitted to the action of the solar spectrum, while immersed in a solution of potassium permanganate, or hydroxyl. When the strength of the permanganate, or hydroxyl, was correct a reversed image of the least refrangible end of the spectrum was obtained, an increase in oxidation taking place when the red rays acted, the reversal taking place somewhere near D, extending into the ultra-red.

The accelerating effect of the red rays is most marked when the solution of permanganate or hydroxyl is weak; but there is a limit to the dilution, caused by the fact that silver bromide is sensitive as far as the line *a*, and there must be sufficient strength to oxidise the invisible image as it is formed, besides gradually destroying the effect of the preliminary exposure. With silver iodide this is not the case, the reversed action is much more readily obtained. The strength of hydroxyl solution I employed I cannot state. That of permanganate was as follows:—

1 grain of potassium permanganate was dissolved in 200 oz. of water.

When the dilution was doubled the reducing action of the red rays on the bromide film was greater than its power of oxidation. A collimator 18 inches long, a prism of 2-inch side, a slit $\frac{1}{100}$ inch wide, and a camera with lens of 2-feet focus, were employed for throwing the spectrum on the plate. The exposure was five minutes, sunlight feeble; preliminary exposure four seconds in the diffused light of my laboratory. A specimen accompanies this paper. Some interesting experiments in an atmosphere free from oxygen are being undertaken.

(Addendum.) Received April 5, 1878.

Since making my communication, which was received by the Royal Society on the 16th March last, I have employed films which were sensitive to the reducing action of the red rays to a wave-length of about 10,000 tenth-metres, and by modifying the strength of the oxidizing solution, the acceleration of oxidation by the red rays and ultra-red rays has been more strongly confirmed. The oxidation on all the films is effected by potassium bichromate, nitric acid, and ozone, in addition to the agents I have already given.

Had not the sensitive film employed been capable of being reduced by the action of the red rays and ultra-red rays, the proof of the acceleration of oxidation by the same rays might perhaps have been open to dispute.

II. "Summary of an Experimental Inquiry into the Function of Respiration at Various Altitudes." By William Marcet, M.D., F.R.S. Received March 19, 1878.

These experiments were mainly undertaken with the view of inquiring into the state of the respiration of tourists at various altitudes, and under the different circumstances met with on Alpine excursions. I was engaged with the present work in the last three summers; the stations selected being:—

1. A village named Yvoire near the Lake of Geneva, altitude 1,230 feet.
2. The Hospice of the Great St. Bernard, 8,115 feet.
3. The Riffel Hotel, Zermatt, 8,428 feet.
4. The St. Theodule Hut, 10,899 feet.
5. The summit of the Breithorn, 13,685 feet.

An interesting paper on the physiological influence of the fall of atmospheric pressure by A. Mermod appeared in "The Bulletin de la Société Vaudoise des Sciences Naturelles" for September, 1877. After making a series of observations on the pulse, rate of breathing and temperature of the body at several, though but moderate altitudes, he selects Strasburg, 124 metres, and a place called Ste. Croix, 1,100 metres, for his experiments on respiration. He made 35 experiments at Strasburg, and 32 at Ste. Croix, the temperature of the air being much the same at both places while occupied with the inquiry. He states, in conclusion, that the absolute and relative quantity of carbonic acid exhaled from the lungs increases by a change of residence to higher altitudes, while the rate of respiration (*frequence respiratoire*) remains always the same, and there is a diminution in the weight of the air breathed.

My results are similar to those obtained by Mr. Mermod, the only point on which we may differ referring to the rate of breathing. My case, however, is slightly different from that of the former experimentalist; he resided two or three months at his stations, while, with the exception of Yvoire, where I spend two or three months every summer, I merely remained at mine long enough to make my experiments. I ascended the Breithorn with my instruments on three different occasions; the second time my stay on the summit lasted five hours fifty-one minutes, and the third time also between five and six hours. I was engaged with the experiments at the St. Theodule Pass during eight days in succession in 1875, and returned there last summer for three days to complete the inquiry, one of those days being taken up with the ascent of the Breithorn with my instruments. The inquiry was carried on for three days at the St. Bernard, and for the same length of time at the Riffel.

My experiments amount to one hundred and eleven in number, they relate to the quantity of carbonic acid expired, the volume of the air exhaled, and the number of expirations observed within a given time.

With respect to the estimation of carbonic acid, the principle of Pettenkofer's method was adopted; but the mode of applying it was somewhat altered in order to suit my requirements. The air to be analysed being first expired into a bag made of strong india-rubber material, was afterwards drawn into a long glass tube of a known capacity. The tubes used in these experiments held about one and a half litre. The tube, fastened upright to a tripod and closed with india-rubber stoppers, was first filled with a nearly saturated solution of salt water;* the bag being then connected with the tube through the stopper at the upper end, the salt water was let out at the lower part, thus aspiring the air from the bag into the tube. The capacity of the tube being known, it became filled with a measured bulk of air to be analysed. The air in the bag had been allowed to cool till its temperature had become the same as that of the salt water in the tube, so that the temperature of the air in the tube and in the bag was alike. A small pear-shaped vulcanised india-rubber bag, of a capacity of about 100 cub. centims. and emptied of its air, was now substituted for the large india-rubber bag, and a pipette holding 100 cub. centims. of the normal solution of barium, and which had been run through the upper india-rubber stopper, was now opened by turning a stopcock. The fluid then fell into the tube, the air thereby displaced being collected in the small india-rubber bag which acted as a diverticulum. A stopcock in the tube connected with this diverticulum being next closed, the large analysis-tube or cylinder was removed from the tripod and its contents driven from one end to the other repeatedly in order to effect the combination of the carbonic acid; the tube was then returned to the tripod, and the air in the diverticulum forced into it by pressure with the fingers, when the stopcock was again closed to be followed by fresh agitation. The milky fluid was now drawn into a small bottle holding about 100 cub. centims., which was well corked, and the solution was subsequently analysed with one of oxalic acid of known strength. With but few exceptions the clear fluid was drawn out of the bottle for analysis with a pipette, as I was led, in the course of this inquiry, to object to filtration. The above description is necessarily much condensed and shortened, but the drawing which accompanies the present summary will explain at one glance the form of apparatus I employed.

The air was breathed into the bag through a well-fitting ori-nazal mask from which two tubes projected, each tube being supplied with a valve carefully made by Mr. Coxeter, of Grafton Street. The valves

* The object of salt water was to avoid the absorption of carbonic acid. I found, however, in the course of the inquiry, that distilled water could safely be used.

were so constructed as to let in the external air for the inspiration, and transmit the inspired air to the bag.

There was much more difficulty than I had anticipated in determining the capacity of the bags used for collecting the air expired. This was done by filling the bag with a bellows under a certain pressure, and then measuring the bulk of air by means of a water aspirator, the amount of water necessary for aspiring out the whole of the air being carefully measured; the capacity of each bag was thus ascertained a number of times, the probable approximate error being finally estimated at half a litre. Two of the bags mostly used in last summer's experiments had been found to contain 39·3 litres of air each under a pressure of one inch of water, another held 46·5 litres of air under 1·5 inch of water. Other bags about the same size were employed, and one still larger was used on a few occasions.

The use of india-rubber bags, although made of strong material, appeared to me at first open to a great objection from the known diffusibility of carbonic acid through caoutchouc, as shown by Thomas Graham;* but in the course of the present inquiry I satisfied myself that no appreciable loss, at all events, no loss serious enough to impair my results, occurred through the bag in the course of an hour, and as much as three-quarters of an hour seldom elapsed between the filling of the bag and the introduction of the air into the analysis-tube. The experiments were made either sitting, or walking on level ground, or walking up hill; most of them were made sitting. They were always conducted in the open air, and no smoking was allowed near the spot while the bag was being filled. The posture selected was assumed a few moments before the experiment was commenced, so as to bring the body under the proper physiological condition. The experiments in the sitting posture were conducted in the following way:—I first placed the empty bag in a woollen jacket to preserve the temperature of the air expired and allow of its being taken, then the bag was connected through a neck and india-rubber tube with a water gauge fastened to a post, and a thermometer was slipped into the bag through another neck. An assistant, and my guide while in the mountains acted as such in a most commendable and intelligent way, stood ready by me with a stop-watch registering the minutes and seconds, and at a preconcerted signal set the watch in motion.† At the same time I began expiring into the bag, holding the mask to the face with my hand. When the bag was full, and the water in the gauge up to a certain mark, the timepiece was stopped. Thus, the time to expire a certain volume of air was determined. The temperature of the air inside the bag was then read off, and the jacket removed from the bag, the latter being next agitated in the air or

* "Phil. Trans.," 1866.

† I frequently attended myself to the watch.

sprinkled over with water or snow till the air it contained had fallen to that of the salt water in the analysis-tube. An assistant holding up the bag I connected it with the tube, into which the air was drawn by letting out the water from the tube into a bottle.

When the experiments were made walking the jacket was usually dispensed with; no account was taken of the carbonic acid in the atmosphere, as I invariably experimented in the open air with the view of obtaining results to be considered comparatively with each other. It is true that M. Truchot* has shown that the proportion of carbonic acid in the atmosphere diminishes as the altitude increases; the difference, however, with respect to the volume of air breathed is so small as to introduce no substantial error in my inquiry. The temperature of the air in the bag immediately after it had been filled, and having its jacket on, was higher than that same air when submitted to analysis. When the difference amounted to more than one or two degrees Fahrenheit, which was nearly always the case, a correction was introduced into the calculation.

Results.

The influence of food in increasing the amount of carbonic acid expired is well known. This is clearly shown on the present occasion by placing in a tabular form the quantity of carbonic acid expired (in grammes and grains) at successive hours after a meal. The table in full being too voluminous for the present abstract, I append the means only:—

Mean Weight of Carbonic Acid expired per Minute.

Time after a meal.	Mean of high altitudes, 10,292 feet.	Number of experiment.	Mean temperature of air.	Low altitude 1,230 feet.	Number of experiment.	Mean temperature of air.
	CO_2 grms. grs.			CO_2 grms. grs.		
From 0 to 1 hr.	0·478 — 7·376	5	45°·7 F.	0·434 — 6·697	6	56°·0 F.
„ 1 to 2 hrs.	0·455 — 7·021	10	48°·1	0·447 — 6·898	4	57°·0
„ 2 to 3 hrs.	0·436 — 6·728	11	46°·9	0·413 — 6·373	11	57°·0
„ 3 to 4 hrs.	0·440 — 6·790	5	46°·7	0·392 — 6·049	6	58°·6
„ 4 to 6 hrs.	0·431 — 6·651 †	8	46°·2	0·396 — 6·111	4	60°·3

In the experiments made on the mountains at various elevations,

* Compte-rendus de l'Académie, vol. lxxvii, 1873.

† It is interesting to observe that the relation between the highest and lowest means of the high altitudes and low altitude respectively is nearly the same, viz., 7·376 : 6·651=6·697 : 6·039; the lowest means found for the low altitude was 6·049.

the mean altitude of which was 10,292 feet, the maximum carbonic acid expired is during the first hour after a meal, and the minimum from four to six hours after eating. There is a slight rise during the fourth hour after a meal, by 4 mgms. only, probably due to a cause independent of food.

At the low station, 1,230 feet above the sea, the maximum carbonic acid is expired the second hour after a meal. There is a slight rise also by 4 mgms. between the fourth and sixth hour after a meal, probably owing likewise to some cause independent of food.

The whole of my experiments are not included in the means of this table, as many of them bore no record of the time they were made at, with reference to the last meal taken. I must also add, that the temperature was only noted occasionally during the day while the experiments were being made, and not at each experiment, as there was frequently no marked change of temperature between one experiment and another.

I only wish to draw general conclusions from this table, which certainly shows that the ingestion of food increases temporarily the amount of carbonic acid expired, a fact which has been fully proved by the experiments of Edward Smith;* I find that the greatest quantity of carbonic acid is expired during the first or second hour after a meal, according to E. Smith this occurs in from one and a-half to two and a-half hours after a meal.

In order to neutralize the influence of food, my experiments were made at all times of the day between breakfast and a late dinner, with but two exceptions which only reduce the means by 2 mgms. for the carbonic acid expired at one station, and 4 mgms. at the other. Hence it cannot be considered, in my estimation of the influence of altitude on respiration, that the food taken exerted any marked influence. I must add, however, that I took, on the whole, less food at the two highest stations—the St. Theodule Pass and Breithorn—than at the other places.

The following table shows the mean results obtained from the experiments made at the five different stations in the sitting posture:—

* "Phil. Trans.," 1859.

Experiments made in the Sitting Posture.

Stations.	Atmospheric pressure.	Altitude.	Mean temperature during experiment.	No. of experiments.	Weight of carbonic acid expired per minute.	Vol. CO_2 expired per minute reduced to 760 mgm. and 0°·C.	Vol. air expired per minute reduced to 760 mgm. and 0°·C.	Per cent. CO_2 in air expired by vol.	Frequency of the respiration per minute.	No. of experiments.	Vol. air expired per expiration reduced.
		feet.			grms.	litres.	litres.				
Breithorn	468 millims. / 18·425 inches.	13,685	34°·9 F.	7	0·487	0·248	4·86	5·1	12·6	6	0·67 lit.
St. Theodule ..	517 millims. / 20·355 inches.	10,899	39°·2	15	0·446	0·227	4·67	4·9	11·8	13	0·61
Riffel	565 millims. / 22·250 inches.	8,428	52°·4	23	0·450	0·229	4·64	4·9	10·3	21	0·64
St. Bernard ..	568 millims. / 22·634 inches.	8,115	43°·7	6	0·460	0·234	4·42	5·1	8·2	6	0·76
Lake of Geneva	726 millims. / 28·592 inches.	1,230	57°·8	37	0·414	0·210	5·14	4·1	8·2	33	0·70

Influence of Temperature at the various Stations on the amount of Carbonic Acid expired.

The stations at which a sufficient number of experiments were made to allow of an approximate estimation of the influence of the temperature of the air on the amount of carbonic acid expired, were Yvoire, the Riffel, and St. Theodule. I have grouped the temperatures and amounts of carbonic acid expired, as will be seen in the following table; the experiments they refer to are quite irrespective of the time at which they have been made after a meal.

Stations.	Mean temperature.	CO_2 expired.	Number of experiments.
Yvoire	51°·5 F.	0·416 grm.	15
	65°·2	0·406 „	15
The Riffel	48°·7	0·491 „	10
	57°·2	0·418 „	10
St. Theodule	36°·1	0·458 „	7
	42°·5	0·439 „	7

In this table, it will be observed that at every altitude there is an increase of carbonic acid expired as the temperature falls, which is especially great at the Riffel. This circumstance observed at the Riffel may be clearly traced to the weather, which was bad during one of the days I was engaged with my experiments there last summer. While sitting out of doors breathing quietly into my bag the rain was falling heavily, accompanied by a cold, piercing wind.

United influence of Temperature and Altitude on the Carbonic Acid expired.

If we now turn our attention to the united influence of temperature and altitude on the amount of carbonic acid expired, the following table will throw some light on the subject by showing the difference of mean temperature, carbonic acid expired, and altitude, between Yvoire and the four other stations:—

Yvoire, 1,230 feet.

Difference in temperature.	Difference in CO_2 expired.	Difference in altitude.
St. Bernard .. − 14°·1	+ 0·046 grm.	6,885 feet
Riffel..... − 5°·4	+ 0·036 „	7,198 „
St. Theodule.. − 18°·6	+ 0.032 „ *	9,669 „
Breithorn.. .. − 22°·9	+ 0·073 „	12,455 „

* At St. Theodule, the temperature was comparatively low in the shade where my thermometer was placed; but many experiments were made in the sun, and, more-

The sign — before the figures, showing differences of temperature, means that those temperatures are by so much lower than the mean temperature at Yvoire; while the sign + in the next column means that the corresponding figures are by so much in excess of those showing the amount of carbonic acid expired at Yvoire. An inspection of this table shows that there is a marked excess of carbonic acid expired at every one of the high stations over the amount expired at Yvoire; and, moreover, that this excess for the summit of the Breithorn is more than twice that registered for the St. Theodule, just double that for the Riffel, and not quite twice that for the St. Bernard.

It is difficult to conclude that this very great proportional excess of carbonic acid exhaled at the Breithorn is due entirely to the cold air met with at that station; and there is, to say the least, a strong presumption of the existence of another cause to account for the formation of more carbonic acid in the body at increasing altitudes. A further consideration of this point will lead to the conclusion that the evaporation from the skin and lungs increasing as the atmospheric pressure falls on rising above the sea, a certain degree of cold is thereby produced. Its effect becomes added to the influence of the fall of atmospheric temperature met with as a rule during the progress of mountain ascents; to be only resisted by the body by additional combustion, attended with an increased production of carbonic acid. In Mr. Mermod's experiments, the temperature of the air was the same at his two stations, and the cold from increased evaporation accounts apparently for the excess of carbonic acid expired at his highest station.

I have therefore come to the conclusion that there is an increase of carbonic acid expired as a person rises above the sea on a mountain excursion, and that this phenomenon is due to two causes—first, the fall of the atmospheric temperature (this is shown as a fact), and secondly, the cold produced by increased evaporation from the body, due to the diminished pressure of the atmosphere, this last conclusion being drawn as an inference. In short, more carbonic acid is formed in the body to counterbalance the influence of cold from the two circumstances just stated. If on ascending to a higher level we should find the same atmospheric temperature as we left at the lower station, still an increased amount of carbonic acid will be expected on account of the cold due to the greater cutaneous and pulmonary evaporation.

As to the volume of air breathed at different altitudes, it is remarkable that, although under barometric pressures very different from each other, and consequently with ever-changing bulks of air actually inhaled, still when these volumes of air are reduced to that they would occupy at the sea-side and at the freezing temperature, we find

over, I took less food there than at the lower stations. These circumstances probably account for the unexpectedly low excess of carbonic acid exhaled at that place.

them nearly but not quite the same. An increase in the amount of carbonic acid evolved at places increasing in altitude was attended with an actual decrease of the volume of air expired also reduced. This result again agrees with that obtained by Mr. Mermod. It applies in my case to greater altitudes, attended with differences of temperatures at the various stations; while, as already stated, the temperature of the air was the same at Mr. Mermod's two stations. As the amount of carbonic acid expired is more at the higher stations than at the lowest, and the volume of air breathed (reduced) less at the higher stations, it must follow that the proportion of carbonic acid expired will be greater at the higher than at the lowest station; we find accordingly that the mean for the high stations yields 4·9 per cent. of carbonic acid in the air expired, while the experiments at the lowest station give 4·1 per cent.

From observations made with and without the face-piece, it follows that I breathed into the open air slower, and apparently deeper, through the mask than without it. This accounts for the small number of expirations I noted per minute in all my experiments. Relatively, however, the results obtained on this point may be accepted, as the breathing was carried on always in the same way while sitting and walking respectively. We find an increase in the frequency of respiration from an altitude of 8,115 feet to that of 13,685 feet, but hardly any such increase between the altitudes of 1,230 feet and 8,115 feet. This greater number of respirations per minute between the St. Bernard and Breithorn was not observed to progress proportionally with rising altitudes from 8,115 feet, as will be seen in the table, and there must have been some other cause besides increased elevation to account for it. Mr. Mermod found no difference in the frequency of respiration between 466 feet and 3,609 feet; and here, again, for low stations I agree with him; but I cannot follow him in concluding that, therefore, the phenomenon is not altered at greater altitudes.

The volume of air reduced, exhaled per respiration, varied at the several stations, but followed no regular change relatively to altitude; it ranged from 510 cub. centims. at St. Theodule, to 760 cub. centims. at the St. Bernard.

Experiments made while walking on Level Ground, or ascending.

In every one of these experiments I walked for a short time, say two or three minutes, or longer, before commencing to collect the air expired. They are fewer in number than those made sitting, and less satisfactory, as it is impossible to depend upon the degree of muscular exertion being the same in comparative experiments while in the act of walking or climbing.

Station.	Occupation.	Weight of CO_2 expired per minute.	Volume of air expired per minute.	Relation of CO_2 to CO_2 expired sitting at station.	Per cent. CO_2 in air expired.	No. of experiment.
		Grms.	Litres.			
Yvoire	Walking level	2·249	25·84	5·42 to 1	4·4	7
St. Bernard	Walking level	2·457	24·77	5·35 to 1	5·0	5
	Walking up rapidly over rocks and grass patches	3·156	32·45	6·86 to 1	4·9	3
	Walking up leisurely	2·120	19·72	4·82 to 1	5·4	2
St. Theodule	Walking level	1·919	22·06	4·32 to 1	4·4	2
	Ascending rapidly steep slope	2·972	24·97	6·69 to 1	6·1*	1
Breithorn	Walking level	1·886	19·48	3·87 to 1	5·0	3

The results have been disposed in a tabular form, and appear to show that while walking on level ground, when a certain altitude is reached, there is a decided fall in the amount of both the carbonic acid and air expired. Yvoire and the St. Bernard gave nearly the same results; but when the height of the St. Theodule Pass was attained (10,899 feet), there was a reduction in the expiration of carbonic acid while walking on level ground from 2·249 grms. at Yvoire, and 2·457 at the St. Bernard, to 1·919 grms.; on the summit of the Breithorn there was a further fall of carbonic acid expired to 1·886 grms., while the volume of air expired per minute was reduced from 24·77 litres to 19·3 litres; but the experiments are not numerous enough to allow of any but very general results. The same remark applies to the experiments made walking up hill. They certainly show, however, that walking up rapidly over rocks and grass patches at or below the elevation of the St. Bernard, yield most carbonic acid, the amount being as much as 3·156 grms. per minute at the St. Bernard, which was attended with the inhalation of the largest volume of air breathed. Ascending quickly at the height of St. Theodule caused a considerable elimination of carbonic acid through the lungs, amounting to 2·972 grms. On the other hand, walking leisurely up hill at the St. Bernard gave rise to the production of no more carbonic acid than quick walking on the level ground at that same station; indeed, the amount was a trifle less.

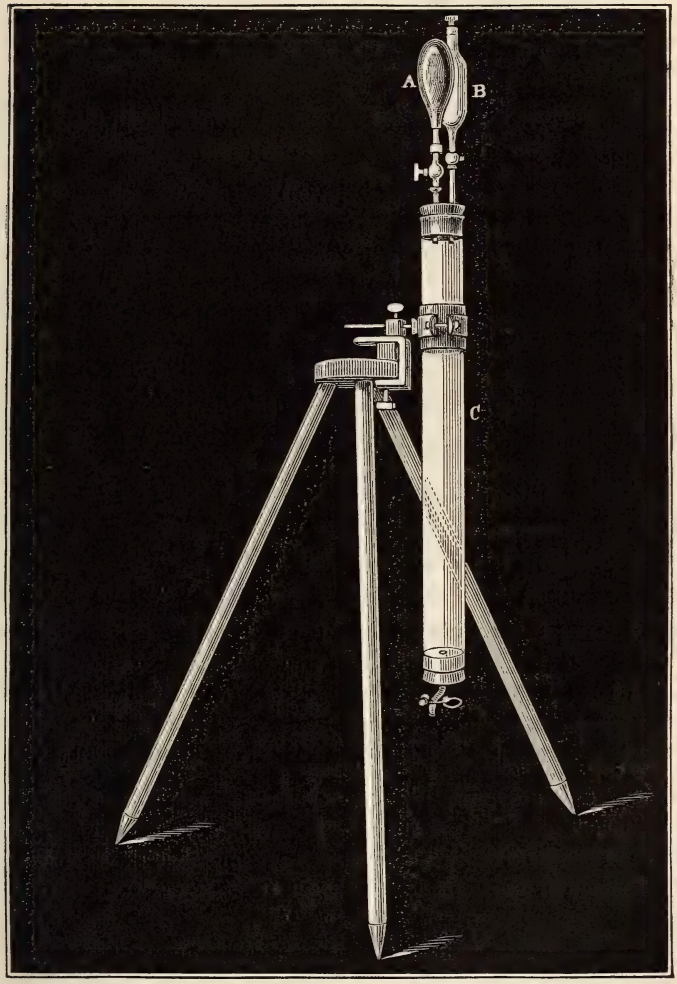
These experiments, therefore, give an idea of the extreme quantities of carbonic acid expired at various altitudes under moderate and great muscular exertion, and appear to show that at great elevations, such as that of the summit of the Breithorn, and perhaps lower, the body is less able to take in a sufficient amount of air for the supply of carbonic

* Appears rather high.

acid necessary to long-continued exertion, which supply becomes thereby reduced in quantity.

EXPLANATION OF THE FIGURE.

A. India-rubber bag acting as a diverticulum.
B. Pipette delivering 100 c.c. of the barium solution.
C. Tube for the analysis of the air expired, of a capacity of about 1·5 litre.



Appendix. Received April 2.

If the relative humidity of the atmosphere should fall at increasing altitudes this might be considered as an additional cause of loss of heat

the body must experience during Alpine ascensions. Professor Plantamour, of Geneva,* from observations made at Geneva and the Great St. Bernard, concludes that there is no marked difference between the hygrometric states at various altitudes. According, however, to Dr. Lombard, who has considerable knowledge and experience of climate, the air appears to be, as a rule, much drier above 1,500 metres than below that altitude.

III. "On Stresses in Rarefied Gases arising from Inequalities of Temperature." By J. Clerk Maxwell, F.R.S., Professor of Experimental Physics in the University of Cambridge. Received March 19, 1878.

(Abstract.)

1. In this paper I have followed the method given in my paper "On the Dynamical Theory of Gases" (Phil. Trans., 1867, p. 49). I have shown that when inequalities of temperature exist in a gas, the pressure at a given point is not the same in all directions, and that the difference between the maximum and the minimum pressure at a point may be of considerable magnitude when the density of the gas is small enough, and when the inequalities of temperature are produced by small solid bodies at a higher or lower temperature than the vessel containing the gas.

2. The nature of this stress may be thus defined: let the distance from the given point, measured in a given direction, be denoted by h, and the absolute temperature by θ; then the space-variation of the temperature for a point moving along this line will be denoted by $\frac{d\theta}{dh}$, and the space-variation of this quantity along the same line by $\frac{d^2\theta}{dh^2}$. There is in general a particular direction of the line h, for which $\frac{d^2\theta}{dh^2}$ is a maximum, another for which it is a minimum, and a third for which it is a maximum-minimum. These three directions are at right angles to each other, and are the axes of principal stress at the given point; and the part of the stress arising from inequalities of temperature is in each of these principal axes a pressure equal to—

$$3\frac{\mu^2}{\rho\theta}\frac{d^2\theta}{dh^2},$$

where μ is the coefficient of viscosity, ρ the density, and θ the absolute temperature.

3. Now, for dry air at 15° C., $\mu = 1{\cdot}9 \times 10^{-4}$ in centimetre-gramme-

* Lombard. "Climat des Montagnes," 1873.

second measure, and $\frac{3\mu^2}{\rho\theta}=\frac{1}{p}$ 0·315, where p is the pressure, the unit of pressure being one dyne per square centimetre, or nearly one-millionth part of an atmosphere.

If a sphere of one centimetre in diameter is T degrees centigrade hotter than the air at a distance from it, then, when the flow of heat has become steady, the temperature at a distance of r centimetres will be

$$\theta=T_0+\frac{T}{2r}, \text{ and } \frac{d^2\theta}{dr^2}=\frac{T}{r^3}.$$

Hence, at a distance of one centimetre from the centre of the sphere, the pressure in the direction of the radius arising from inequality of temperature will be

$$\frac{T}{p}\,0{\cdot}315 \text{ dynes per square centimetre.}$$

4. In Mr. Crookes' experiments the pressure, p, was often so small that this stress would be capable, if it existed alone, of producing rapid motion in small masses.

Indeed, if we were to consider only the normal part of the stress exerted on solid bodies immersed in the gas, most of the phenomena observed by Mr. Crookes could be readily explained.

5. Let us take the case of two small bodies symmetrical with respect to the axis joining their centres of figure. If both bodies are warmer than the air at a distance from them, then in any section perpendicular to the axis joining their centres, the point where it cuts this line will have the highest temperature, and there will be a flow of heat outwards from this axis in all directions.

Hence $\frac{d^2\theta}{dh^2}$ will be positive for the axis, and it will be a line of maximum pressure, so that the bodies will repel each other.

If both bodies are colder than the air at a distance, everything will be reversed; the axis will be a line of minimum pressure, and the bodies will attract each other.

If one body is hotter, and the other colder, than the air at a distance, the effect will be smaller; and it will depend on the relative sizes of the bodies, and on their exact temperatures, whether the action is attractive or repulsive.

6. If the bodies are two parallel disks, very near to each other, the central parts will produce very little effect, because between the disks the temperature varies uniformly and $\frac{d^2\theta}{dh^2}=0$. Only near the edges will there be any stress arising from inequality of temperature in the gas.

7. If the bodies are encircled by a ring having its axis in the line joining the bodies, then the repulsion between the two bodies, when

they are warmer than the air in general, may be converted into attraction by heating the ring, so as to produce a flow of heat inwards towards the axis.

8. If a body in the form of a cup or bowl is warmer than the air, the distribution of temperature in the surrounding gas is similar to the distribution of electric potential near a body of the same form, which has been investigated by Sir W. Thomson.* Near the convex surface the value of $\frac{d^2\theta}{dh^2}$ is nearly the same as if the body had been a complete sphere, namely $2T\frac{1}{a^2}$, where T is the excess of temperature, and a is the radius of the sphere. Near the concave surface the variation of temperature is exceedingly small. Hence the normal pressure on the convex surface will be greater than on the concave surface, as Mr. Crookes has shown by the motion of his radiometers.

Since the expressions for the stress are linear as regards the temperature, everything will be reversed when the cup is colder than the surrounding air.

9. In a spherical vessel, if the two polar regions are made hotter than the equatorial zone, the pressure in the direction of the axis will be greater than that parallel to the equatorial plane, and the reverse will be the case if the polar regions are made colder than the equatorial zone.

10. All such explanations of the observed phenomena must be subjected to careful criticism. They have been obtained by considering the normal stresses alone, to the exclusion of the tangential stresses; and it is much easier to give an elementary exposition of the former than of the latter.

If, however, we go on to calculate the forces acting on any portion of the gas in virtue of the stresses on its surface, we find that when the flow of heat is steady, these forces are in equilibrium. Mr. Crookes tells us that there is no molar current, or wind, in his radiometer vessels. It may not be easy to prove this by experiment, but it is satisfactory to find that the system of stresses here described as arising from inequalities of temperature will not, when the flow of heat is steady, generate currents.

11. Consider, then, the case in which there are no currents of gas, but a steady flow of heat, the condition of which is

$$\frac{d^2\theta}{dx^2}+\frac{d^2\theta}{dy^2}+\frac{d^2\theta}{dr^2}\,(=-\nabla^2\theta)=0.$$

(In the absence of external forces, such as gravity, and if the gas in contact with solid bodies does not slide over them, this is always a solution of the equations, and it is the only permanent solution.) In

* Reprint of Papers on Electrostatics, p. 178.

this case the equations of motion show that every particle of the gas is in equilibrium under the stresses acting on it.

Hence any finite portion of the gas is also in equilibrium; also, since the stresses are linear functions of the temperature, if we superpose one system of temperatures on another, we also superpose the corresponding systems of forces. Now the system of temperatures due to a solid sphere of uniform temperature, immersed in the gas, cannot of itself give rise to any force tending to move the sphere in one direction rather than in another. Let the sphere be placed within the finite portion of gas which, as we have said, is already in equilibrium. The equilibrium will not be disturbed. We may introduce any number of spheres at different temperatures into the portion of gas, and when the flow of heat has become steady the whole system will be in equilibrium.

12. How, then, are we to account for the observed fact that forces act between solid bodies immersed in rarefied gases, and this, apparently, as long as inequalities of temperature are maintained?

I think we must look for an explanation in the fact discovered in the case of liquids by Helmholtz and Piotrowski,* and for gases by Kundt and Warburg,† that the fluid in contact with the surface of a solid must slide over it with a finite velocity in order to produce a finite tangential stress.

The theoretical treatment of the boundary conditions between a gas and a solid is difficult, and it becomes more difficult if we consider that the gas close to the surface is probably in an unknown state of condensation. We shall, therefore, accept the results obtained by Kundt and Warburg on their experimental evidence.

They have found that the velocity of sliding of the gas over the surface due to a given tangential stress varies inversely as the pressure

The coefficient of sliding for air on glass was found to be $\lambda=\frac{10}{p}$ centimetres, where p is the pressure in millionths of an atmosphere. Hence at ordinary pressures λ is insensible, but in the vessels exhausted by Mr. Crookes it may be considerable.

Hence if close to the surface of a solid there is a tangential stress, S, acting on a surface parallel to that of the body, in a direction, h, parallel to that surface, there will also be a sliding of the gas in contact with the solid over its surface in the direction h, with a finite velocity $=S\frac{\lambda}{\mu}$.

13. I have not attempted to enter on the calculation of the effect of this sliding motion, but it is easy to see that if we begin with the case in which there is no sliding, the effect of permission being given to the gas to slide must be in the first place to diminish the action of

* Wiener Sitzb., xl (1860), p. 607.

† Pogg. Ann., clv (1875), p. 337.

all tangential stresses on the surface without affecting the normal stresses; and, in the second place to set up currents sweeping over the surfaces of solid bodies, thus completely destroying the simplicity of our first solution of the problem.

14. When external forces, such as gravity, act on the gas, and when the thermal phenomena produce differences of density in different parts of the vessel, then the well-known convection currents are set up. These also interfere with the simplicity of the problem and introduce very complicated effects. All that we know is that the rarer the gas and the smaller the vessel, the less is the velocity of the convection currents; so that in Mr. Crookes' experiments they play a very small part.

IV. "Note on the Existence of Carbon in the Coronal Atmosphere of the Sun." By J. N. Lockyer, F.R.S. Received March 20, 1878.

It is now four years since I obtained evidence, which seemed to me conclusive, as to the existence of carbon in the sun's atmosphere. There were two points, however, which remained to be settled before the matter could be considered to be placed beyond all doubt.

The first was to establish that the fluted bands generally present in the spectrum of the arc, as photographed, which bands vary very considerably in strength according to the volatility of the metal under experiment, were really bands of carbon—a point denied by Ångström and Thalèn.

This point is settled by the photographs submitted to the Society with this communication. In these the carbon bands remain the same, though one spectrum is that of carbon in air, the other of carbon in chlorine dried with great care, and the proof that it cannot be the spectrum of a combination of carbon with oxygen lies in the fact that in the chlorine it is more brilliant than in the oxygen. Now, assuming the chlorine to have been but imperfectly dried, this would not have happened if a compound of oxygen had been in question.

The next point was to obtain evidence that there was absolutely no shift in the carbon bands, which sometimes happens when the part of the arc photographed is not perfectly in the prolongation of the axis of the collimator.

A photograph has been obtained which supplies such evidence. There are metallic lines close to the carbon bands, which are prolongations of Fraunhofer lines, while the lines which I have already mapped at W. L., 39·27 and 39·295, in the spectrum of iron, are also absolute prolongations. Therefore there is no shift in the carbon flutings. Now the individual lines in the brightest portion of the

fluting in the ultra-violet are absolute prolongations of a fine series of Fraunhofer lines in which the wonderful rhythm of the elements of the flutings is fully retained.

Ångström has already shown that the true carbon *lines* which we get when a coil and jar are employed are not reversed in the spectrum of the sun. I have already shown that the calcium spectrum in the sun is the spectrum of the spark, and not of the arc.

The carbon vapour then exists not only in a more complicated molecular condition (as is evinced by the flutings) than the metallic vapours in the sun's atmosphere, but at a lower temperature.

It must, therefore, exist *above the chromosphere*, that is, in a region of lower temperature. Lower pressure is also indicated by the feeble reversal.

The idea I put forward some time ago that the outer atmosphere of the sun (and possibly the composition of the outer planets) is more metalloidal than metallic is strengthened by this observation.

V. "On the Physiological Action of the Poisonous Principle of *Urechites Suberecta*." By James John Bowrey, F.C.S., M.I.C., Analytical Chemist to the Government of Jamaica. Communicated by Professor A. W. Williamson, Foreign Secretary of the Royal Society. Received March 22, 1878.

Several species of *Echites* are common in Jamaica, and certain of them, known as "nightshades," or "Savana flowers," are reputed to be very poisonous. In this paper I shall confine myself to a species distinguished as the yellow flowered nightshade, and which Professor Daniel Oliver has identified as *Urechites suberecta, Muell. Arg.* (*Echites Neriandra, Griseb.*). This plant is well known to be exceedingly poisonous; frequently when I have been examining a sprig plucked at the road side a passing negro has warned me to be careful as it was very dangerous.

My attention was specially directed to it by the fact that it is supposed to have been the chief poison used by Obeah men in the time of slavery. The most wonderful tales are current of the power of these men, and it is even stated that they could so administer their poisons as to kill immediately or after the lapse of days or weeks.

Sloane states, "No animal will meddle with this herb, and . . . two drams of the expressed juice killed a dog in eight minutes, but it may be so ordered as to kill a person in many days, months, or years." This statement I certainly think exaggerated, but my experiments show that it has some considerable foundation in fact. Sloane further states, "Two or three spoonfuls of the juice purge young persons upwards and downwards, bringing away worms. The milk, outwardly

applied, takes away warts, and cures ringworms and freckles." He also gives the following as the symptoms occurring in the case of a medical man to whom a negress was believed to have given portions of this plant, violent griping, inclination to vomit, loss of appetite, small convulsions in various parts of the body, hectic fever, and continued wasting of flesh. Under treatment a fatal result was prevented, but some considerable time passed before the convulsive symptoms left the patient.

The flowers, and all the green parts of the plant, are very bitter and acrid, so strong and disagreeable is the taste that I think it almost impossible that an adult could ignorantly take a fatal dose of the fresh plant; both Sloane and Gosse, however, give instances in which there was good reason to think death was caused by rum impregnated with the poisonous principle, a plug of the leaves having been used as a stopper. The *dry* leaves may be chewed for some little time before their bitterness is observed. Cattle are sometimes poisoned by eating the dry leaves when grazing, the plant having been cut down and carelessly left in the pasture when it was cleaned. The symptoms described to me as occurring in cattle poisoned by *Urechites suberecta* agree with those I have observed in animals to which the poisonous principles have been given in the laboratory.

Four substances having toxic properties have been obtained, viz.:—

Crude principle, a resinous body extracted from the leaves by Stas's process, under the supposition that they contained an alkaloid. This substance may be considered to be a mixture of all the active principles of the plant with a small proportion of inactive matter.

Urechitoxin, a crystallizable body of definite composition.

Amorphous urechitoxin, an uncrystallizable form of urechitoxin, which has not been obtained perfectly pure, but which resembles the crystallizable substance in many respects.

Urechitin, a crystallizable substance of definite composition, which constitutes the great proportion of the poison present in unchanged *Urechites suberecta* leaves.

The substances enumerated above are glucosides; for further information respecting their preparation and chemical properties the reader is referred to a paper read before the Chemical Society (Journ. Chem. Soc., xxxiii, p. 252).

The effect of the crude principle was tried on mice and cats with the results now to be detailed.

Effect on Mice, given by Mouth.

Experiment I.

A little of the emulsion of the crude poison was given to a mouse; immediately on swallowing it the animal lost all power of locomotion, and convulsive movements of the muscles came on, the whole hind-

quarters moving together. This was over in about a minute, but was succeeded by twitching of the limbs, continuing for about two minutes, after which all motion ceased, and the mouse was dead three minutes after the poison was placed in its throat.

Experiment II.

Gave a smaller dose of the emulsion than that employed in Experiment I to two mice; for some time no effect was visible. In from fifteen to thirty minutes there appeared convulsive movements of the abdomen, as if the animal would vomit; it then jumped about for a few seconds, fell over on its side deprived of all power of locomotion, its muscles twitched for a minute or two, and death followed.

Experiment III.

A little of the crude poison, in a form like very thick treacle, was put in the mouth of a mouse; from its treacly condition the poison could not be spat out by the animal. For half an hour no effect was noticed; then there appeared to be disinclination to move, which increased until motion was plainly impossible, the mouse falling over on its side and dying ninety minutes after the poison was put in its mouth. The other symptoms noted were apparent attempts to vomit, seeming difficulty in breathing, and shortly before death convulsive gaspings for air.

Experiment IV.

A dose of the poison in the same state as that used in Experiment III, but in quantity much less, was given to a mouse; for an hour or two the animal appeared to be unaffected; it then became less active. Next day it would hardly move even when touched, but it ate well. It was not seen on the third and fourth days, but was well provided with food and water. On the morning of the fifth day it seemed better than on the evening of the second day, but as the day passed on it got worse, being very indisposed to move, and when it did clinging to the ground as if afraid of falling over. When first seen on the morning of the sixth day it could hardly move, and as the morning passed on it completely lost all power of locomotion, and died quietly just five full days after it had received the poison. Immediately before death the penis became erected, and semen was emitted.

The most marked symptom in these four experiments was loss of power of locomotion, the paralysis coming on very gradually when the dose was smallest; when the poison quickly proved fatal, convulsions preceded death.

Effect on a Kitten, given by Mouth.

Experiment V.

9.7 A.M. Attempted to give $\frac{1}{10}$ grain of the crude poison in the form

of an emulsion to a fine healthy kitten weighing about 2 lbs. The effect on the mouth and fauces was so immediate that but a small fraction of the dose was swallowed, the instantaneous choking causing the discharge of most of the emulsion, while the large quantity of frothy saliva which directly began to flow must have washed the mouth out completely.

In two or three minutes the animal appeared to be trying to vomit, but possibly it was merely choking up the thick saliva.

9.26 A.M. Passed ordinary fæces.

9.28 A.M. Vomited violently; this repeated at 9.37 and 9.43. Up to this time it kept on its legs, now and then moving about slowly; it appeared very disinclined to move, yet impelled to do so.

9.45 A.M. Lay down and, except when vomiting or roused, remained lying down as if sleeping till three o'clock. During the latter part of the time, when disturbed, it would walk a few steps, but was in great danger of falling over on its side.

10.8 A.M. Convulsive vomiting of a thick greenish-yellow liquid, repeated at 10.33; 11.5; 11.50 A.M., and 12.20, and 1.20 P.M. Each time the vomiting occurred after the kitten had been disturbed, or had itself moved a few steps; it screamed during each vomiting, and even during an attempt to vomit.

At 3 o'clock in the afternoon it walked out of the laboratory into the office, and there went to sleep until removed to my lodgings; it slept all the evening and night, and was still sleepy next morning.

All food and drink were refused on the day of administration; next morning it tried to eat a morsel of meat, but did not swallow any of it; later in the day it drank some milk; after this it recovered rapidly.

The poison was given on an empty stomach. There were altogether ten actual or attempted vomitings, nine of which occurred within the first four and a half hours. Two hours after the administration it seemed to be in a dying state, after this time it gradually improved; very likely had it been disturbed when in its weakest condition, convulsions would have come on, and death would have been the result.

Effect on a Kitten, given subcutaneously.

Experiment VI.

12.45 P.M. Injected $\frac{1}{2}$ cub. centim. of solution of the crude poison in 40 per cent. spirit under the skin of neck of a kitten weighing $1\frac{1}{4}$ lbs. The $\frac{1}{2}$ cub. centim. of solution contained $\frac{1}{25}$ gr., *i.e.*, 2·6 mgms. of the poison.

12.53 P.M. Vomited.

12.56 ,, Vomited violently. Unsteady on its legs; afraid to

move, and when it did so was in danger of falling over on its side. It walked as if drunk.

1.0 P.M. Passed urine and fæces. Vomited. It was now very restless, screamed continually, and had general convulsive tremors. Wagging its tail. Soon it became quiet, and appeared very sleepy, but in a minute or two convulsions of a violent character came on, the pupils dilating in the fit, but contracting as it passed off.

1.15 P.M. A second attack of convulsions came on in which the animal died. The pupil of each eye was so enlarged that no iris could be seen, nor was any visible thirty minutes after death.

Rigor mortis not perceptible till an hour after death; in two hours it was marked. The body was opened in the afternoon on which the animal died; the only special appearances noted were the very distended state of the stomach, and the tenseness of its walls, the tense wiry condition of the intestines, which were so contracted that a transverse section appeared solid, and the gorged state of the auricles and right ventricle, the left ventricle being nearly empty.

Effect of Urechitoxin.

This substance is very bitter and acrid, and produces a singular sensation on the tongue and lips, as if they were swollen and blistered; no visible change is really produced, but so vivid is the sensation that it is almost impossible to persuade oneself that the tongue is of its usual size, and the lips unblistered. The tingling lasts for many hours. Urechitoxin is powerfully sternutatory, the least particle introduced into the nostrils causing violent fits of sneezing, accompanied with a choky sensation at the back of the throat. Experiments have been made on mice, cats, and one dog, with this poison, and as I believe it to be the typical active principle of the plant—if, indeed, not *the* active principle, urechitin being in all probability a compound containing it—more experiments have been made with it than with the other forms of the poison.

Effect on Mice, given by Mouth.

Experiment VII.

Administered 2·5 mgms., *i.e.*, nearly $\frac{1}{25}$ gr. of pure hydrated urechitoxin, dissolved in $\frac{1}{20}$ cub. centim. of spirit (50 per cent.), to a mouse, passing the solution well down its throat. It was dead before I could put it out of my hand.

Administered 1 mgm., *i.e.*, less than $\frac{1}{60}$ gr. of the same urechitoxin in $\frac{1}{10}$ cub. centim. of spirit (30 per cent.), to a mouse in a similar manner, it gave but one or two twitches after being put out of the hand and was dead. These mice were opened, their stomachs found empty, about a quarter drop of liquid squeezed from each and separately tested gave the urechitoxin colour reaction distinctly.

Experiment VIII.

Tried to give a mouse $\frac{1}{2}$ mgm., *i.e.*, nearly $\frac{1}{125}$ gr. pure hydrated urechitoxin, dissolved in $\frac{1}{20}$ cub. centim. of spirit (30 per cent.), but part was swallowed, certainly not more than $\frac{1}{300}$ gr. In two hours the animal was sluggish, clinging to the ground as if afraid of falling over on its side. In six hours so paralysed as to be unable to govern its movements, tumbling over if it tried to move from one spot to another, its limbs trembling much, and its breathing difficult and convulsive. Next morning, nineteen hours after the poison had been given, the mouse was found dead and stiff.

These mice were affected exactly as were those to which the crude poison was administered. The effects, however, appeared much more quickly, no doubt because the urechitoxin was given as a perfect solution.

Effects of small repeated doses of Urechitoxin (given by the Mouth) on Cats.

Experiment IX.

To a healthy kitten, weighing about 2 lbs., $\frac{1}{50}$ gr., *i.e.*, nearly $1\frac{1}{3}$ mgms. of pure hydrated urechitoxin was given. The poison was dissolved in a drop of spirit which was allowed to fall into the back of the kitten's mouth. Frothy saliva began to flow directly, the discharge continuing for nearly forty minutes. I think some of the poison must have been got rid of in the saliva. No other effect followed. Next day the dose was repeated with the same effect; after the flow of saliva had ceased it ate, but about three hours after the administration of the second dose the animal was seized with strong convulsions, crying out spasmodically and gasping for breath. It repeatedly passed urine, also a little fæces, and died in the convulsions.

Experiment X.

Administered $\frac{1}{250}$ gr., *i.e.*, nearly 0·26 mgms. pure hydrated urechitoxin to a kitten weighing about 2 lbs. The poison was given in one drop of spirit (50 per cent.), and was allowed to fall into the throat of the animal. The dose was repeated daily for the four succeeding days, then a day missed; after which the dose was given twice more. In all, seven doses were administered in eight days; during this time the kitten was dull, and had but little appetite. It was found dead within an hour of the exhibition of the last dose, and unfortunately was not seen alive after the poison was given. The total quantity of poison given was $\frac{1}{36}$ gr., rather less than 2 mgms.

On opening the kitten a remarkable absence of fat was observed, the tissues were pale, the lungs almost colourless, and the tongue very white. The large arteries were empty, the large veins gorged with

blood, the left ventricle quite empty, the right ventricle and the auricles were very full, in each auricle was a clot, that in the right very fibrinous and apparently growing from its walls. The liver was normal, the gall-bladder very full, the bile tinging the surrounding tissues. The stomach distended with gas, otherwise empty, its walls thick and stiff, the intestines down to the appendix thick and stiff, indeed almost wiry, empty, and contracted, so as to have the appearance of a solid cylinder more than that of a tube on being cut through transversely. This condition was not observable below the appendix, the lower part of the large intestines contained much fæcal matter. The bladder was empty, the kidneys apparently normal, their external blood-vessels beautifully injected. The body was opened three and a half hours after death.

Experiment XI.

In this experiment $\frac{1}{500}$ gr., *i.e.*, 0·13 mgm. of pure hydrated urechitoxin, in one drop of spirit (40 per cent.), was administered to a kitten on each of the dates noted below.

February 3rd.—First dose given. No effect noticed. Appetite good. Weight 2 lbs. 10 ozs.

February 4th.—No effect. Appetite good.

February 5th, 6th, 7th.—Saliva flowed for a short time; vomited after dose. Appetite good.

February 8th, 10th, 11th.—The administration of each dose was followed by retching. Appetite good. Weight now 2 lbs. 8 ozs.

February 12th and 13th.—No vomiting.

February 14th, 15th, 17th.—Violent vomiting in a few minutes. Appetite still fairly good. Weight from 2 lbs. 7 ozs. to 2 lbs. 9 ozs.

February 18th and 19th.—Did not vomit nor care to eat. Weight fell on the 19th to 2 lbs. 6 ozs.

February 20th.—The last dose given. No vomiting. At first refused to eat, but eight hours after receiving the poison it ate a little. Weight 2 lbs. $5\frac{1}{2}$ ozs.

Next morning it was found dead; it had evidently died in convulsions.

Its dead weight was 2 lbs. $4\frac{1}{2}$ ozs. The pupils of the eyes were so enlarged as to reduce the irises to mere circular lines; this appearance I have noticed in every cat killed by urechitoxin. The kitten was in good condition; on opening it a large quantity of fat was found surrounding the internal organs, otherwise the appearances were similar to those noted in Experiment X. The weight was taken each day before food was given to the animal, and the poison was administered by allowing the drop of spirit containing it to fall into the throat of the kitten immediately after it had been weighed. It was then returned to its cage, and food speedily supplied to it.

Experiment XII.

This was an experiment similar to Experiment XI, excepting that but one half the quantity of hydrated urechitoxin was given daily, viz., $\frac{1}{1000}$ gr., *i.e.*, 0·065 mgm. The kitten weighed 2 lbs. 2 ozs.

February 3rd.—First dose given, repeated on the 4th, 5th, 6th, 7th, 8th, 10th, 11th, 12th, 13th, 14th, without any visible effect. Weight varying from 2 lbs. to 2 lbs. 2 ozs. Appetite good.

February 15th, 17th, 18th, 19th, 20th.—The kitten during this time was dull but still ate fairly. Its weight declined to 1 lb. 15 ozs., one morning being 1 lb. 13 ozs. Though it ate, its appetite was fast failing.

February 21st.—No appetite. Very dull. Weight 1 lb. $14\frac{1}{2}$ ozs.

February 22nd.—Refused food entirely. Weight 1 lb. 13 ozs. Died in convulsions in the evening; it had become very thin. It was not opened.

Exp. XI.—Each dose $\frac{1}{800}$ gr. Given 16 in 18 days. Total weight given $\frac{1}{31}$ gr. = 2·1 mgm. Death on 18th day. Weight on 1st day 2 lbs. 10 ozs., on 18th day 2 lbs. $5\frac{1}{2}$ ozs.

Exp. XII.—Each dose $\frac{1}{1000}$ gr. Given 18 in 20 days. Total weight given nearly $\frac{1}{55}$ gr. = 1·2 mgm. Death on the 20th day. Weight on 1st day 2 lbs. 2 ozs., on 20th day 1 lb. 13 ozs.

Experiment XIII.

In this experiment the poison was given in a manner more likely to ensure the swallowing of each dose completely, and also at the same time to prevent its action on the fauces. A solution of pure hydrated urechitoxin was prepared containing $\frac{1}{100}$ gr. of the poison in each cub. centim., strong spirit being used as the solvent. A number of bits of filtering paper—each about $\frac{3}{4}$-inch square—were suspended on pins. Each of these pieces of paper received $\frac{1}{10}$ cub. centim. of the spirit solution delivered from an accurately graduated pipette; when the spirit had completely evaporated, and the squares were dry, they were placed in a stoppered bottle. To give the dose, a square of paper was cut into shreds and mixed with a little butter—the grease appeared to prevent the cat perceiving the bitter taste of the poison—and placed in a bit of meat, which was then given to the animal. The cat was liberally fed once every day about 11 A.M., its weight being taken first, and the poison given immediately before the day's food was put in its cage. I may remark that plenty of exercise was allowed to all the animals experimented on, the cats having the run of large premises during the day, their cages being closed at night only.

Red cat weighing 5 lbs. 4 ozs. Received first dose July 3rd. Repeated on July 5th, 6th, 7th, 8th, 9th and 10th, during which time its weight was constant.

July 12th, 14th, 15th, 16th, during which time its weight fell to 5 lbs.

For the last few days it was rather dull and inactive, and lost its appetite.

At 10 A.M., on July 17th, there was nothing to attract notice in its appearance; but at 11 o'clock it was seized with convulsions, passed fæces involuntarily; and after the convulsions passed off it lay on its side breathing heavily from the abdomen till about 1 P.M., when it died in a second attack of convulsions.

Its bladder was found distended with urine, the stomach and intestines empty and tense, the latter almost wiry. Gall-bladder full, liver very dark and full of blood. All the veins full of dark blood, which coagulated very quickly. Lungs pale. Heart, right side gorged with dark blood, left side contained a little redder blood, but not fully aërated. Arteries collapsed, and nearly empty. Pupils distended so as almost to obliterate iris. Tongue bluish immediately after death.

Experiment XIV.

A white cat, weighing 6 lbs. at the beginning of the experiment, was treated exactly as the one in Experiment XIII, a square of paper containing $\frac{1}{1000}$ gr. of urechitoxin being given to it every *week* day (with one exception) for eleven weeks. Its weight gradually increased, so that it weighed $6\frac{1}{2}$ lbs. at the end of that time. No effect was apparent up to the day on which it received the last dose. In the morning it was well and cheerful, at 1 P.M. it was discovered very ill, possibly it had been attacked by convulsions. However, it took the dose of poison, and ate a little food. Next day it seemed well, no poison was given to it. The day after it died about 10 A.M.; it was not seen to die, but it presented the appearances seen in other cats poisoned with urechitoxin. There had been a little purging shortly before death.

Experiment XV.

A kitten, weighing $1\frac{3}{4}$ lbs., was treated in the same manner as the cats in Experiments XIII and XIV, a square of poisoned paper being given to it every week day for nearly four weeks, a total of 22 doses being administered. For the first three weeks no effect was apparent and its weight increased to 2 lbs.; during the last week it appeared dull and did not care for its food, its weight falling to $1\frac{1}{2}$ lbs. There was nothing calling for special note in its mode of death or in the appearances subsequently observed.

Exp. XIII. Dose $\frac{1}{1000}$ gr., 11 given, total weight of poison exhibited $\frac{1}{91}$ gr. = 0·71 mgm. Weight of cat $5\frac{1}{4}$ lbs., fell to 5 lbs.

Death one day after last dose, which was given fourteen days after the first one.

Exp. XIV. Dose $\frac{1}{1000}$ gr., 65 given, total weight of poison given $\frac{1}{15}$ gr. = 4·2 mgms. Weight of cat 6 lbs., *rose* to $6\frac{1}{2}$ lbs. Death nearly two days after last dose, which was given eleven weeks after the first one.

Exp. XV. Dose $\frac{1}{1000}$ gr., 22 given, total weight of poison exhibited $\frac{1}{46}$ gr., = 1·42 mgm. Weight of kitten $1\frac{3}{4}$ lbs., fell to $1\frac{1}{2}$ lbs. Death two days after last dose, which was given twenty-six days after the first one.

Three other cats were experimented on in a similar manner, two of them receiving about 100 squares each, and the other about 40 up to the time when the experiments were discontinued. Except very occasional vomiting these cats appeared in perfect health; none of them lost weight, while one gained considerably. Only a portion of the squares of poisoned paper were taken from the same lot as those used in Experiments XIII, XIV, and XV, most of them were from a new batch prepared from a solution which had been kept for a long time. I have found that weak solutions are apt in time to lose their bitterness, indeed, the papers prepared from this old solution had but a very slight bitter taste; this test, however, was not applied until the absence of effect on the cats led me to suspect that something was wrong. In all probability, for the greater part of the time, these three cats were receiving but a fraction of the dose intended.

Effect of Urechitoxin on a Dog, given by Mouth.

Experiment XVI.

One gr., *i.e.*, 65 mgms. of crystallized hydrated urechitoxin, was put on a piece of meat, another piece laid over it and the two bits of meat lightly tied together with a piece of cotton and given to a medium-sized dog; no effect followed for an hour, then the animal vomited four times in quick succession, the meat containing the urechitoxin was found in the vomit, the quantity of poison apparently undiminished, one edge of the layer of poison, however, was free, *i.e.*, uncovered by meat, and must have been in contact with the juices of the stomach. The dog was watched for an hour and a-half after the vomiting was over, during which time it slept naturally, showing no ill effects whatever. It was then left for the night, with food and water at its side, in the full expectation of finding it quite well in the morning; however, it was discovered dead and cold, the food and drink untouched.

The internal organs were found to be congested, all the cavities of the heart contained much thick black blood, the auricles being gorged. There were marks of irritation on the inner face of the stomach. The brain was normal.

Effect of a large dose on Cats, subcutaneously injected.

Experiment XVII.

Injected $\frac{1}{8}$ gr., *i.e.*, 8·1 mgm. pure hydrated urechitoxin, dissolved in $\frac{1}{2}$ cub. centim. (40 per cent.) spirit, under the skin of neck of a strong full grown cat. For ten minutes nothing particular was noticed, unless, indeed, the animal was slightly uneasy and indisposed to move; at the end of this time it became very uneasy, mewing a good deal. When thirteen minutes had elapsed the cat vomited violently and then passed fæces. During the next twenty minutes it vomited very violently three times and passed fæces and urine involuntarily, the breathing also became very hurried, more than 100 respirations per minute. Very unwilling to move, seemed giddy, tail twitching; two or three times, however, the animal showed unexpected strength, getting up and walking a few steps. About thirty-five minutes after the injection most violent convulsions came on and death took place thirty-seven minutes after the poison had been injected. When the convulsions came on the pupils enlarged so as to reduce the irises to mere circular lines, this enlargement remained for a considerable time after death, then the irises began slowly to contract.

The appearances noted on opening the animal were the same as those usually seen in other cats poisoned by urechitoxin. The stomach contained about a teaspoonful of white frothy liquid and was very much distended with gas. The intestines were remarkably tense and wiry, and showed frequent knots in their course, like the knots of a bamboo cane but much more exaggerated.

Experiment XVIII.

Injected $\frac{1}{15}$ gr. pure hydrated urechitoxin, dissolved in spirit, under skin of neck of a half-grown kitten, immediately afterwards gave it a mouse which it caught and ate. In five minutes from time of injection vomiting came on and the mouse was ejected. The kitten now very uneasy, convulsive twitchings appeared, speedily followed by strong convulsions, in which it died within eight minutes of the injection. It was opened directly, the stomach was loaded with food and the colon with fæces. The heart, which was very distended, was not cut into; it contracted considerably as it cooled. The other appearances were similar to those noted in other experiments.

Experiment XVIII*a*.

In this case a quantity of urechitoxin was injected sufficient to prove fatal very speedily. Experiments XVIII and XVIII*a* were performed simply in order that some medical friends might see the great activity of the poison, and I mention No. XVIII*a* here, for the sake of the following extract from my note book. "The cat was

opened immediately after death, no irritability of the heart was noticeable on pricking that organ; this want of irritability was observed on subsequent occasions."

Experiment XIX.

Injected $\frac{1}{20}$ gr. = 3·2 mgms. pure hydrated urechitoxin, under the skin of neck of a full grown cat. The poison was in solution in spirit.

10.35 A.M. Poison injected.
10.50 ,, Fæces passed.
10.53 ,, Vomited violently.
10.54 ,, Do.
10.59 ,, Do. merely bringing up a little frothy liquid.
11.2 ,, Do. do. do.
11.7 ,, Do. do. do.
11.12 ,, Vomited violently, merely bringing up a little frothy liquid; it now appeared sleepy.
11.22 A.M. Again vomited frothy liquid, now very sleepy.
11.27 ,, Passed urine.
11.30 ,, Vomited violently and immediately lay down again.
11.40 ,, Momentary convulsions.
11.46 ,, Do. do.
11.48 ,, Very violent convulsions accompanied with cries. During the fit life became extinct, 1 hour and 13 minutes after the injection of the poison. At 11.10 o'clock a mouse was put with the cat, which noticed but did not attempt to catch it.

In each of these experiments on the action of large doses, the first effect was to cause the animal to vomit, which, in Experiment XVIII, was very speedily followed by convulsions and death, but, in Experiments XVII and XIX, the vomiting occurred repeatedly, urine and fæces were passed, and there was a marked disinclination to muscular exertion before the final convulsions came on and life became extinct.

Effect on Cats of very small doses, subcutaneously injected.

Experiment XX.

Injected $\frac{1}{100}$ gr., = 0·65 mgm., pure hydrated alpha echitoxin, dissolved in $\frac{1}{4}$ cub. centim. (40 per cent.) spirit, under the skin of neck of a small full grown cat, weighing $3\frac{1}{2}$ lbs. Neither food nor drink was taken by the cat after the injection.

10.50 A.M. Poison injected, a little appeared to escape from the wound on withdrawing the syringe.
11.10 A.M. Uneasy, licking lips, vomited.
11.20 ,, Do. do.
11.35 ,, Do. do.
11.50 ,, Do. do.

12.0 o'clock. Attempted repeatedly to vomit, but brought up nothing.

12.20 P.M. Attempted repeatedly to vomit, but brought up nothing, the retching not so violent; up to this time had been restless, now seemed fatigued and disposed to lie down.

12.35 P.M. Again ineffectually tried to vomit, continuing its efforts for five minutes.

12.45 P.M. Vomited violently a yellow frothy liquid.

1.45 ,, Passed urine, tried to vomit, appeared sleepy. Dribbled much from its mouth.

2.20, 3.20, and 4.20. Vomited violently a yellow frothy liquid. This vomiting was repeated three times between 4.20 and 11 o'clock P.M., when it was last seen alive. At 5 o'clock next morning it was found dead, stiff, and cold. It had evidently died in convulsions, the pupils were so expanded as to almost obliterate the irises. Death must have occurred within sixteen hours of the injection. On being opened, a generally congested state of the vessels of the viscera and brain was found, the intestines, as usual, tense, and the auricles and right ventricle of the heart gorged with blood, while the left ventricle contained but very little.

Between two and three hours after the injection a mouse was put with the cat, it immediately caught and killed it, but neither played with nor ate it.

Experiment XXI.

Injection of $\frac{1}{50}$ gr., = 1·3 mgm., pure hydrated urechitoxin, followed twenty-four hours afterwards by a second injection of $\frac{1}{100}$ gr. of the same sample of poison. The cat used was a very strong one, weighing 6 lbs. 6 ozs., it was last fed fourteen hours before the first injection.

10.20 A.M. Injected $\frac{1}{50}$ gr. urechitoxin, dissolved in $\frac{1}{2}$ cub. centim. (40 per cent.) spirit, under skin of neck.

10.35 A.M. Very restless, seemed to want to pass fæces and urine.

10.40 ,, Passed fæces.

11.6 ,, Passed urine.

11.15 ,, Vomited violently, bringing up partly digested food. Panting much as if very hot. The usual licking of lips preceded the vomiting, after which the cat appeared much easier.

11.17 A.M. Passed fæces.

11.20 ,, Again panting, lying down extended, respirations over 100 per minute and irregular.

11.25 A.M. Vomited much partly digested food, afterwards became very restless, still panting.

11.30 A.M. Lay down extended, panting, pupils appeared dilated, but were quite sensitive to light. Soon got up, restless again.

11.45 A.M. Vomited twice, brought up very little. Respirations over 120 per minute and irregular.

Put a mouse in the cage; the cat noticed and touched it, and when it fled, started after it, but did not follow it; remained looking at, but did not attempt to catch it. The mouse was removed, when the cat lay down and remained lying down from 11.50 to 12.15 o'clock. Respirations varying from 100 to 90 per minute, irregular in both rate and intensity, but becoming less violent and rapid.

12.15 P.M. Got up, restless, crying, licking lips till

12.17 ,, Vomited violently a little frothy liquid. Soon lay down again.

12.30 P.M. Respirations 104 per minute, but not so strong or irregular as before. Heart beating 150 per minute. Beats strong.

12.40 P.M. Got up and walked slowly about the laboratory, seemed several times as if it wanted to jump up on window-ledges, &c., but did not attempt to do so. On being put back in the cage it struggled to get out for some ten minutes, then lay down as if asleep, but not really so.

2.0 P.M. Still lying down. Respirations 24 per minute. Put a mouse in the cage; the cat looked at it, but did not move.

3.20 P.M. Respirations 24 per minute; the cat had a very sleepy look. A little after this it sat up and appeared as if it would vomit, but did not do so. Now very sluggish, and remained so, neither vomiting or passing fæces or urine till 10.30 next morning, when its respirations were irregular and 45 per minute; the heart-beats were also irregular and over 110 per minute. It refused to eat, and had taken nothing, except possibly some water, from the time when the poison was injected.

Second day.—10.38 A.M. Injected $\frac{1}{100}$ gr. urechitoxin in $\frac{1}{4}$ cub. centim. 40 per cent. spirit under skin of neck.

10.43 A.M. Passed urine.

11.0 ,, After licking lips, vomited a little yellow frothy liquid.

11.25 ,, Do. do. do.

11.43 ,, Do. do. white do.

12.5 P.M. Passed hard fæces.

12.25 P.M. After licking lips, vomited a little yellow frothy liquid.

1.30 ,, Do. do. white do.

During the rest of the day remained in a very dull state, looking very ill. Would have nothing to do with a mouse. Took no food, but late in the day just tasted some milk. Passed urine once in the evening.

Third day.—10.0 A.M. Weight 5 lbs. 14 ozs. During this day it looked rather better, but still very dull. Tasted milk, passed urine twice, would have nothing to do with a mouse, remained quite still till

4.30 P.M. Vomited white frothy liquid. Took no other food than the one taste of milk. Passed urine during the night.

Fourth day.—7.0 A.M. Vomited twice, bringing up much white frothy liquid. Afterwards tasted some milk—drank no appreciable quantity.

10.30 A.M. Vomited a quantity of liquid, looking like dilute milk.

11.30 ,, Would have nothing to do with a mouse. Lay down almost wherever it was placed, and even if it wanted to reach another spot, it moved but a few steps, trembling and but half-raised on its legs, in danger of falling over on its side. It would lie down without reaching the desired place, never moving two feet at one time.

Weight 5 lbs. 11 ozs. It gradually got worse, twitching of the ears, and indeed whole head, now occurred on its being forced to make the slightest movement. The pupils very contracted, and not sensitive to light. I think, however, that if the cat's attention could have been aroused they would have expanded and proved sensitive.

2.0 P.M. The cat now unable to keep on its feet; even when placed upon them it sank on to the ground without any attempt to take up a restful or easy position. Saliva flowing from its mouth.

3.0 P.M. to 3.10 P.M. While being moved convulsions, accompanied with cries, came on, followed by vomiting and a second attack of convulsions, in which it died.

Weight now 5 lbs. 10 ozs. The eyes presented the usual enlarged pupils. The body was immediately opened. Stomach inflated, contained a little yellow mucus. Intestines contracted. Colon contained a little hard fæces. Bladder contained a few drops of urine. Liver and kidneys appeared normal, the veins of latter beautifully injected. Lungs pink with a tinge of yellow, became cream-coloured as they cooled. Heart filled. Pericardium, right ventricle full; on cutting into it some thick dark blood flowed, but a mass of coagulated blood remained in it; left ventricle contained a little red blood, which did not coagulate until it was exposed to the air. External vessels of brain congested; no blood-points were seen in its substance on cutting into it. Death took place 77 hours after the first injection and 53 hours after the second. The total weight of poison injected was $\frac{3}{100}$ gr., *i.e.*, nearly 2 mgms., equivalent to one-and-a-half millionth part of the weight of the cat.

Experiment XXII.

Injected $\frac{1}{100}$ gr. pure hydrated urechitoxin, dissolved in spirit, under skin of neck of a cat weighing 4 lbs. $10\frac{1}{2}$ ozs. The animal was last fed 16 hours before the injection.

10.50 A.M. Poison injected. After it was liberated the animal lay down, and remained so till

11.25 A.M. When it began to lick its lips, but the uneasiness passed off, and only occasionally did it give a lick, still lying down.

11.55 A.M. Got up uneasy, mewing, licking lips.

12 o'clock. Vomited frothy liquid.

12.12 P.M. Passed fæces consciously. A freshly killed mouse was given to it; it seized it, but laid it down again almost immediately, and would not notice it afterwards. Food, even milk, refused.

1.0 P.M. Heart-beats barely perceptible.

1.45 ,, A mouse put in cage; the cat immediately caught and killed it, then left it.

3.20 P.M. Passed urine. Since 12 o'clock lying down nearly all the time, unwilling to move, but not asleep.

3.30 P.M. Passed a little fæces with difficulty.

3.40 ,, Vomited white frothy liquid. Heart-beats *not* perceptible. Vomited twice during the evening and passed fæces once. Vomited again in the morning before 10 o'clock. Ate nothing.

Second day.—10.30 A.M. Weight 4 lbs. 7 ozs. Sluggish, but quite able to move vigorously. Smelt at, but would not eat, meat. Heart-beats very rapid and hardly perceptible. Quiet all day, refused all food.

Third day.—Weight 4 lbs. 5½ ozs. Early in the morning vomited, bringing up a worm. During the night had neither passed urine or fæces.

10.0 A.M. Very sluggish, would not move unless forced to. Heart beating as yesterday. A mouse put in the cage; the cat looked at, but did not attempt to catch it, indeed, it seemed frightened at it.

It remained in this dull state all the third day, and up to 10 o'clock on the morning of the fourth day, neither eating, drinking, vomiting, or passing urine or fæces.

Fourth day.—Weight 4 lbs. 4 ozs. Remained in the same dull state all day, taking nothing and passing nothing.

Fifth day.—Weight 4 lbs. 3 ozs. At 10 A.M. very sluggish, but proved itself quite capable of action by running across the room, and jumping up to the cage on being frightened while being weighed, yet would not touch a mouse put in the cage before the cat was taken out to be weighed. At 10.30 A.M. passed about ½ oz. of urine. In the evening ate pretty well and drank milk freely.

Sixth day.—Weight 4 lbs. 2 ozs. Early in the morning passed very hard fæces. Looked better, but still very sluggish.

Eighth day.—Weight 4 lbs. 2 ozs. Nothing particular to note since the sixth day. The cat now appeared well, but had not energy to catch a mouse put with it. Next day still better, caught and ate a mouse. On the tenth day was still stronger, and on the eleventh was quite recovered.

The poison injected was not the three-millionth part of the weight of the cat. No food whatever was taken until the evening of the fifth day, and as nothing had been taken for sixteen hours before the injection, nothing was eaten for five whole days.

Experiment XXIII.

Injection of $\frac{1}{100}$ gr. crude principle under skin of neck of a strong black cat weighing 4 lbs., followed three days after by a second injection of $\frac{1}{100}$ gr. hydrated urechitoxin.

First day.—11.20 A.M. Injected $\frac{1}{100}$ gr. crude principle, dissolved in $\frac{1}{4}$ cub. centim. 40 per cent. spirit.

11.50 A.M. Lay down as if to sleep, but if it did sleep it was not soundly. It continued lying down with its eyes closed, but opened them on being spoken to. No other effect followed, and on the second and third days it appeared quite well and ate fairly.

Fourth day.—10.30 A.M. Injected $\frac{1}{100}$ gr. urechitoxin in $\frac{1}{4}$ cub. centim. 40 per cent. spirit. The cat had not been fed since the previous afternoon, and judging from the results of its vomiting, its stomach must have been empty at the time of this second injection.

10.50 A.M. Vomited violently a little mucus. Licked its lips for a few minutes previously.

10.55 A.M. Licked lips. Vomited violently a little mucus.

11.2 ,, Do. do. do.

11.12 ,, Do. do. do. Appeared sleepy.

12.20 ,, Licked lips. Vomited violently a little mucus. Appeared sleepy.

11.29 A.M. Licked lips. Vomited violently a little mucus. Appeared sleepy.

11.32 A.M. Thick frothy saliva flowed from mouth. The cat noticed but did not attempt to catch a mouse.

11.35 A.M. Licked lips. Made frequent attempts to vomit, but brought up nothing more than a little mucus.

11.58 A.M. Licked lips and attempted to vomit as at 11.35.

11.20 P.M. It was disturbed by being handled, after which it dribbled and licked its lips and seemed as if it would vomit, but did not do so. Soon went to sleep again.

1.20 P.M. Was again disturbed, when the same symptoms came on as at 12.20 P.M.

1.30 P.M. Passed urine consciously.

1.45 ,, Drank a good deal of water, but did so very slowly. The rest of the day it remained still in a dozing state, able to notice, but there was much difficulty in arousing it sufficiently. Would not stir a single step from the place were it lay unless obliged to do so by force, then it moved as if very weak. It appeared thirsty, drinking water occasionally. It was seen to vomit three times during the evening, and three times before 10 o'clock next morning. It may have vomited at other times in the night.

Fifth day.—10.0 A.M. Sleepy if not actually sleeping. Very unwilling to move, and when it did, unsteady in its gait.

11.30 A.M. Tried twice to vomit, and, after violent effort, brought up a little frothy mucus; there was the usual licking of lips before the vomiting.

1.30 P.M. Tried to vomit at 11.30 o'clock, with the same result. During the rest of the day remained quite quiet, taking nothing but water.

Six and seventh days.—Still in the same state, apparently sleeping nearly the whole time, refused to eat, but drank water. It was *seen* to vomit once on the sixth and twice on the seventh day.

Eighth day.—In the same state. Vomited at 8 A.M. and 10.30 A.M., and at 12 o'clock; in the mucus brought up at 12 o'clock there were two worms. Drank a very little milk which it had hitherto refused; still refused all solid food. Now very weak. Noticed but did not try to catch a mouse put in the cage with it.

2 P.M. Vomited a little curdled milk.

4 ,, Weight 3lbs. 10½ ozs. During the rest of this day and night remained in the same lifeless, listless state. Refused all solid food, but lapped a very little milk.

Ninth day.—Weight 3 lbs. 9 ozs. At 10 A.M. in much the same condition as on the previous day, but noticed a little more when it was stroked.

11 A.M. Put the cat in the cage with a mouse; no attempt was made to catch it, and soon the cat vomited a quantity of yellow liquid.

In the afternoon put the cat in a large glass vessel with two mice; it hardly noticed them; lay still for some time allowing them to run over her, then lazily jumped out of the vessel, more active than in the morning. If not disturbed, was always asleep; had to awaken; took no solid food, merely lapped a few drops of milk; it seemed as if it was too much trouble to do even that.

Seventh day.—Very sluggish but more active than before; weak in its movements. When put in glass vessel with mice would not notice them, but jumped out and soon after vomited. Took no food whatever.

Eleventh day.—Weight 3 lbs. 6 ozs. Plainly much better, noticed when spoken to, purring, &c. Took no food, however, and would have nothing to do with mice.

Twelfth day.—Weight 3 lbs. 4 ozs. Better than yesterday. Would not attempt to catch a mouse. Ate a little bit of meat. After this it rapidly and completely recovered.

The injection of the crude principle produced no other effect than disinclination to activity for a few hours afterwards.

The injection of the urechitoxin (on the fourth day from that of the crude poison) was followed in twenty minutes by vomiting. On the fourth, fifth, sixth, and seventh days, all food was refused. On the eighth day took a little milk which was speedily ejected by vomiting.

On the ninth day took a few drops of milk. Refused all food on the tenth and eleventh days. On the twelfth ate a bit of meat. Practically no food was taken for eight days.

4th day was seen to vomit or retch 11 times. Took no food.
5th do. do. 6 do. do.
6th do. do. 1 do. do.
7th do. do. 2 do. do.
8th do. do. 4 do. Drank and vomited a little milk.

9th day was seen to vomit or retch once. Took a few drops of milk.

10th day was seen to vomit or retch once. Took no food.

11th day. No vomiting. Took no food.

12th day. ,, Ate a bit of meat.

The weight of the cat fell from 4 lbs. to $3\frac{1}{4}$ lbs. on the twelfth day, *i.e.*, in the eight days succeeding the second injection, it lost nearly one-fifth of its weight.

Effect of Amorphous Urechitoxin on Cats.

Experiment XXIV.

This and the three following experiments were all made with the same sample of amorphous urechitoxin, a sample from which the crystalline urechitoxin had been removed.

Injected $\frac{1}{100}$ gr. amorphous urechitoxin in $\frac{1}{4}$ cub. centim. of spirit (40 per cent.) under skin of neck of a full grown cat weighing 4 lbs. 12 ozs. The *only* effect observed was that the animal became dull and sleepy, and would eat on the day of injection, or on the two following days, thus fasting for three days. It ate a little, very little, on the fourth and fifth days, but still remained in the same sleepy state, taking no exercise. After that it rapidly recovered. There was no vomiting.

Experiment XXV.

Injected $\frac{1}{100}$ gr. amorphous urechitoxin in $\frac{1}{4}$ cub. centim. (40 per cent.) spirit under skin of neck of cat weighing 5 lbs. 14 ozs.

The *only* effect noticed was that the cat was sleepy, dull, and apparently weak, and did not recover its appetite for full five days after the injection of the poison. There was no vomiting.

Experiment XXVI.

3.32 P.M. Injected $\frac{1}{25}$ gr. amorphous urechitoxin in $\frac{1}{2}$ cub. centim. (40 per cent.) spirit under skin of neck of a kitten weighing 1 lb. 1 oz.

3.41 P.M. Passed fæces apparently involuntarily. Seemed very uncomfortable, mewing and licking its lips, then vomited.

3.43 P.M. Passed more fæces. Apparently no difficulty in walking.

2.45 P.M. Vomited violently and continued trying to vomit, with cries and slight unsteadiness on its legs until—

3.48 P.M. When convulsions came on, speedily ending in death.

The appearances noted after death were similar to those recorded as observed on opening other cats.

Experiment XXVII.

10.45 A.M. Injected $\frac{1}{100}$ gr. amorphous urechitoxin under skin of neck of a cat weighing 4 lbs. 6 ozs. The animal had had no food for 16 hours.

11.10 A.M. Uneasy, crying, licking lips until—

11.22 „ When it vomited, brought up little else than frothy liquid. Cat panting slightly.

11.34 A.M. Passed fæces and urine. The panting increasing. The animal disposed to lie down, looking as if fatigued by heat.

11.40 A.M. Vomited white frothy liquid, this somewhat relieved the panting. Soon it became very indisposed to move; had been very restless, now it lay quite extended, its heart beating very hard and fully 140 per minute.

12.2 P.M. Vomited white frothy liquid.

12.12 P.M. A mouse was put in the cage, the cat instantly jumped up and killed it, but did not play with or eat it. The exertion of killing it, though momentary, was too much for it.

1.40 P.M. Put another mouse in the cage, the cat looked at it but for five minutes did not stir, then it got up and smelt it, and on the mouse bounding away, the cat started after but did not follow it.

1.50 P.M. Saliva flowing from cat's mouth.

3.0 P.M. Not so disposed to continue lying down. Heart beating hard but not so hard as before, but quicker, 220 beats per minute.

During the evening it vomited once, and again in the morning before 10 A.M. From the time of the injection refused all food, merely drinking a little water.

Second day.—10.30 A.M. Sluggish, but able to move somewhat vigorously. Weight 4 lbs. 2 ozs. Urinated once during the day. Tasted but did not swallow a bit of meat. Took no other food.

Third day.—Weight 4 lbs. 0½ oz. Ate nothing. Tasted but did not drink some milk. Passed neither fæces or urine. Remained lying down; heart beating quickly, but not so hard as before. Looked at, but did not attempt to catch, a mouse; indeed, seemed frightened at it.

Fourth day.—Weight 4 lbs. Ate some meat in the morning, after which it sat up and cleaned itself; remained lying down all the rest of the day. Passed no fæces, but some urine during the night.

Fifth day.—Weight 4 lbs. 2 ozs. Looking better, but still very dull;

noticed, but did not attempt to catch, a mouse. Drank a little milk in the morning, and fed well in the evening. Remained still all day.

Sixth day.—Weight 4 lbs. 5 ozs. Much better.

Eighth day.—Barely sufficient energy to catch a mouse which was put in the cage. Might now be considered recovered.

These four experiments show that amorphous urechitoxin has the same physiological action as the crystalline urechitoxin, but perhaps is not so active, weight for weight.

Effect of Urechitin on Cats, given by Mouth.

Experiment XXVIII.

A dose of finely powdered urechitin, weighing $\frac{1}{10}$ gr., *i.e.* 6·5 mgms., was given to a cat on a piece of meat; the whole was swallowed, and the animal ate a hearty meal directly afterwards.

11.20 A.M. Poison administered.

12.40 P.M. Vomited a little frothy liquid; during the next hour it twice vomited food, and once frothy liquid.

2.15 P.M. Had twice since 1.40 P.M. vomited a little frothy liquid. Now very indisposed to move, when it did was very sluggish and also uncertain in its movements. Before the poison began to act, it had been a more than usually active cat. Though so unwilling, or perhaps unable readily, to move, it was too uneasy to remain long in one spot. Breathing laboured and abdominal.

3 P.M. Vomited frothy liquid very violently. Very weak. After this time it remained very quiet, as if too weak to move, yet it appeared decidedly easier. Next morning it was still very feeble.

At 10 A.M. it vomited some frothy liquid, in which was a worm. It had been vomiting during the night. It refused to eat all day, but after this time recovered rapidly and completely.

Urechitin is very insoluble in watery liquids; and as the dose was given in the solid form, and was immediately followed by a hearty meal, which was entirely ejected within two and a half hours from the exhibition of the poison, no doubt but little was absorbed into the system.

Experiment XXIX.

To the perfectly recovered cat used in Experiment XXVIII, administered a similar dose, *i.e.*, $\frac{1}{10}$ gr. urechitin; but this time the poison was dissolved in a few drops of very strong alcohol, and the solution diluted with weak spirit until the poison began to crystallize out of the mixed solution, and very fine crystals were then poured into the throat of the cat, and its mouth held shut until they seemed to be swallowed. The bulk of the dose undoubtedly was swallowed, but little escaping.

10.40 A.M. Poison given on empty stomach.
11.0 „ Vomited clear liquid mixed with white froth.
11.10 „ Do. do.
11.20 „ Do. yellow liquid mixed with yellow froth.
11.40 „ Do. do.
12.5 P.M. Do. do.

The vomiting very violent and accompanied with cries.

1.10 P.M. Made violent attempts to vomit, but brought nothing up. Very weak.

3.4 P.M. Vomited violently white frothy liquid.

7.0 „ Do. do. immediately afterwards convulsions came on, in which it died.

After 1 o'clock it gradually became weaker, and was very unwilling to move. When the effects of the poison were well developed, the skin of neck was observed to be of a bright pink colour; the colour was easily seen on the neck, as the skin was very thin there; when the poison was given the skin was almost colourless. The eyes were not affected, their pupils remaining sensitive to light; in death, however, they enlarged, the irises becoming mere lines.

Experiment XXX.

A dose of $\frac{1}{10}$ gr. of pure finely powdered urechitin was distributed on two small pieces of meat, which were then given to a cat; one piece was swallowed, the other rejected after being chewed (no doubt the poison was tasted); a considerable proportion of the dose, however, must have been swallowed even from that second piece of meat, for the poison lying on its exterior would be swept off in the act of chewing.

11.0 A.M. Dose given on empty stomach.
11.40 „ Vomited.
12.0 Do.
12.15 P.M. Do.
1.0 „ Do. but not again up to the time of leaving the laboratory (4 P.M.). After mid-day it remained in the same spot, never moving.

Next morning it was found in the same state as when left; there were no signs of any vomiting. On being let out of its cage it slowly walked to a bush, under the shade of which it remained all day. It refused to eat anything. In the evening it was left under the bush. Third morning—The cat was found lying in a cool place some 40 feet from the spot where it was left on the second day. It was very weak, and had lost the power of locomotion completely, but could move its limbs languidly. At 10 A.M. it was carried into the laboratory, that it might be more closely watched. If undisturbed it lay perfectly still, asleep. Liquid ran from its mouth. Pupils sensitive to light, but

moved sluggishly. Now and then a slight convulsive movement of the body and limbs was seen. It became weaker and weaker, until the breathing was imperceptible, and only by touch could I be certain it was not dead. Urine ran from it. At a few minutes before 12 o'clock it died. I did not see it die, but I had examined it about ten minutes before I found it dead. There was no rigidity, but the usual enlargement of the pupil was very marked immediately after death, which took place 49 hours after the administration of the poison.

The bladder was found distended with urine, and the lower bowels full of hard fæces. The smaller intestines were empty, but not tense or contracted. The gall bladder distended with bile, and colouring the stomach yellow. The lungs pink. The right side of the heart gorged with dark blood, and the veins also; the left side contained a little thin red blood. The body was opened very shortly after death.

The experiments I have detailed show the four forms of poison extracted from *Urechites suberecta* to be practically identical in their physiological action. In mice they cause muscular paralysis, respiratory difficulty, and convulsions ending in death. In cats, large and moderate doses always produced vomiting, which (in the cases where very speedily fatal doses were exhibited) was followed by convulsions and death; but where death was delayed, both fæces and urine were evacuated, the vomiting frequently renewed, pulse and respiration quickened, notably the former, and muscular paralysis gradually developed, the end being preceded by convulsions. Very small doses produced on the first day vomiting, defecation and micturition; the vomiting persisting for several days, but the bowels becoming confined and the secretion of urine apparently lessened. But perhaps the most marked symptoms following the exhibition of small doses, was the refusal of all food for many days, and the great disinclination to muscular exertion. Salivation was also sometimes produced by the *injection* of very small quantities of the poison.

In the four experiments on cats, in which but $\frac{1}{1000}$ gr. of the poison was administered daily, there was no vomiting, indeed no visible effect for many days; then a gradual loss of appetite and general dulness, succeeded by sudden convulsions, followed in a few hours by death. In Experiment XIV, however, no symptoms of poisoning appeared for eleven weeks, and then they preceded death by less than twenty-four hours.

The smallest quantity of the pure crystallized poison which proved fatal to a cat was $\frac{1}{100}$ gr., injected subcutaneously in one dose—see Experiment XX. This was equivalent to one two-and-a-half millionth of the cat's weight. The next smallest fatal dose is that recorded in Experiment XIII, in which eleven separate doses were administered, amounting in all to $\frac{1}{91}$ gr., or one three-and-a-quarter millionth of the cat's weight. In other experiments no fatal effect resulted until three

to six times as much, proportionally to the weight of the animal, was swallowed.

The Society then adjourned over the Easter Recess, to Thursday, May 2.

May 2, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President in the Chair.

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

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

John Gilbert Baker, F.L.S.
Francis Maitland Balfour.
Rev. Thomas George Bonney, M.A.
Prof. James Henry Cotterill, M.A.
Sir Walter Elliot, K.C.S.I.
Rev. Canon W. Greenwell, M.A.
Thomas Hawksley, C.E.
John Hopkinson, M.A., D.Sc.
John Hughlings Jackson, M.D.
Lord Lindsay, P.R.A.S.
Samuel Roberts, M.A.
Edward A Schäfer, M.R.C.S.
Herman Sprengel, Ph.D.
George James Symons.
Charles S. Tomes, M.A.

The following Papers were read:—

I. "On the Life-History of a Minute Septic Organism: with an account of Experiments made to determine its Thermal Death Point." By Rev. W. H. DALLINGER. Communicated by Professor HUXLEY, Sec. R.S. Received March 26, 1878.

[PLATES 8, 9.]

Nearly four years since, in examining an infusion of animal matter, which had, unfortunately, been diluted with water, and had vegetable substances placed in it, I observed a minute and intensely active organism, which, on closer and more careful examination, I found to be of a form entirely new to me. I therefore determined to endeavour to discover its life-history; but the diluted infusion was unsuited to it, and in the course of five days no living form remained. During this time I had been able to determine very little that was consecutive, but had seen enough to lead me to desire to find it again; and for the two following years I steadily examined all the infusions within my reach, as well as all probable places besides, but in vain. And it was

not until the latter part of the summer of 1876 that I found it again. It appeared then, in a maceration, in which the body of a vole was decomposing. The maceration was only three weeks old, and this organism had evidently only just arisen; it increased daily in vigour and numbers, and in three weeks I was at liberty to study it continuously.

For this purpose I employed the "continuous stage," jointly devised for preceding researches,* by means of which a drop of the infusion could be kept under examination, without evaporation, for an indefinite time, and with the most powerful lenses.†

My method was to follow out, as far as possible, the morphological details separately; and then to steadily follow one form from its earliest to its most mature condition; thus discovering how the different states were related, and making out an unbroken life-history.

Most of the more difficult and delicate work was done with an entirely new lens, made specially for me by Messrs. Powell and Lealand; and which is nominally a $\frac{1}{35}$th inch lens. It was made specially for this class of investigation, for which it is admirably adapted. Its "working distance" is sufficient; its "penetration," for such a power, is extremely great, its angle being moderate; and its "definition" is as sharp and clear, when properly used, as that of the finest $\frac{1}{4}$th or $\frac{1}{8}$th. I also had the advantage of the use of the four new lenses made on a "new formula" by the same makers, viz., a $\frac{1}{8}$th of low angle and great penetrating power; and a $\frac{1}{8}$th, $\frac{1}{12}$th, and $\frac{1}{16}$th of high angles. I also used the $\frac{1}{25}$th and $\frac{1}{50}$th lenses.

One of the difficulties attending the study of this organism was its extreme rapidity and caprice of movement. Its normal form is depicted at fig. 1, Plate 8. It is there magnified 3,000 diameters. Its sarcode is clear, and to all the lenses employed structureless. It is usually found to have minute vacuoles scattered through it. The form of the body is distinctive. Its greater part is a long oval slightly constricted a little above the middle. But from the front, or shorter portion, a kind of neck (*a*, fig. 1) protrudes, from which proceeds the front flagellum, which is extremely fine, and from one and a-half times to twice as long as the body. Below this and at the sides or "shoulders" of the organism, two other long and fine flagella arise, proceeding backwards, as seen at *b*, *c*, *ibid.* In addition to this, there is always a nucleus-like body slightly to one side of the lower part of the organism, as seen at *d*; and with the higher lenses and delicate manipulation there was frequently seen, within this, an ex-

* *Vide* "Further Researches into the Life-History of the Monads." Monthly Micro. Journal, vol. xi, pp. 97–99.

† Linen instead of bibulous paper is now used as the agent for conveying a constant supply of evaporating moisture to the chamber in which the organisms are examined in this piece of apparatus.

tremely minute globule, also indicated at *d*. But as this was not always present, even in the normal state of the organism, it was probably not important.

The extreme length of the body was the $\frac{1}{4000}$th of an inch. It rarely exceeded this and was often less.

It swam, as I have said, with great rapidity; and all its movements were most graceful, varied, and controlled. Its usual mode of motion is in a direct line, or in curves; and with its trailing flagella behind and its active anterior one, it is a very remarkable object. But the suddenness with which it can arrest its most rapid movement, or change it in any direction up, down, or directly reversed, is still more remarkable. In this the flagella at the sides are brought into active operation; sometimes both being spread out like long arms; at other times one being stretched out and in vigorous action, while the other is closely pressed to the other side, and so forth, giving this organism a control over its movements which is of extreme interest. Indeed the apparent volitional mastery which this seemingly structureless speck exercises over these equally structureless filaments, for the determination of its course, cannot be seen and studied without wonder.

But besides this free-swimming movement, it was capable of the most vigorous motion in an "anchored" state. Most of the larger septic organisms belong to, or are associated with, certain conditions of the decomposing matter; and disappear before the advent of other forms, when that condition is past. This one was associated with the general breaking up of the decomposing animal matter; and the movement which I am about to describe apparently contributed to this. It was a powerful springing motion. The trailing flagella became attached to the floor as at *a*, *b*, fig. 2, by what means could never be discovered; but the attachment was very secure and could be discontinued at any moment. Directly the anchorage is made, the two anchored flagella with the utmost rapidity coil in a spiral, as at *c*, fig. 2, bringing the body of the organism nearly to a level with the floor. With equal suddenness it darts up in the line indicated by the arrow *d*, and thus having reached the limits of its flagella it moves down, with equal rapidity, in an arc of a circle of which the flagella are the radius; indicated by the arrow *e*. In this way the whole body is brought down on the point *f*. But the matter to be noticed is, that this is never done except upon a fragment of the decomposing tissues in the infusion. And if a small fragment be taken out at any time and examined, it would soon be encircled by thousands of these forms incessantly darting up and down upon it in the way described; and careful observation showed that they, by this means, rapidly changed the form and diminished the size of the little particles so attacked. No morphological changes are at all apparent in those that

are in this condition, but they are continually freeing themselves and swimming away and others are constantly coming.

The changes now to be described will be understood to be given as the result of long-continued and repeated effort. Failure is very much more frequent than success in working out the preliminary details, but pre-eminently so in steadily following to the end the changes undergone by a minute organism.* I merely record the final results, which have issued both from the study in detail of phases in the life-cycle of the organism, such, for example, as the minutiæ of the method of fission and fusion—and also from an unbroken observation of its comparatively short life-cycle, which was three times repeated.

Following then, steadily, a normal form, which has just freed itself from the springing condition, as fig. 1, the first real indication of change, though by no means the first to be *discovered*, is a splitting of the anterior flagellum, as seen at *a* and *b*, fig. 3, and the moving of the nucleus to the centre, as seen at *c*. With a power of from three to four thousand diameters, there may also be seen a delicate line under the base of the flagellum, as shown at *d*. In the course of from thirty to sixty seconds this has widely opened and the base of the flagellum has divided as seen at *a*, fig. 4, while at the same time the nucleus shows an incision in the direction of its length, as seen at *b* in the same figure; and a similar incision has commenced at the posterior end of the body, as shown at *c*. In a very few seconds more, this slight incision (*c*, fig. 4) is the origin of a wide opening, as seen at *a*, fig. 5, above which it will be seen that the nucleus has almost divided and a pale line runs through the body-substance from the upper to the lower opening. The posterior opening widens much more rapidly than the anterior one, as fig. 6 shows, depicting a condition ensuing in from one minute to four minutes after that shown in fig. 5. And at this stage the nucleus has split into two and the divided parts occupy distinct positions, as shown at *a*, *b*. The upper or anterior split now widens as well as the posterior one, as shown in fig. 7, leaving the two parts merely united by a neck of sarcode.

* In fact hundreds are followed which from one or more of many causes do not complete the cycle of their lives. It may be arrested—and frequently is—by death in the earlier stages, in the middle, or still worse towards the end. In the same way there may be failure by the individual form being lost amidst a crowd of others, or by slowly working its way to the little ring of liquid at the edge of the "cover," and then going out into it, and so making further study impossible. Or the apparatus may be at a critical moment a source of trouble; the delicate balance between the moisture carried over by capillarity, into the chamber containing the fluid with its organisms, and the evaporation taking place, may by some means, such as a sudden change of temperature, be broken, and nullify hours of patient and, but for that, successful work. In every such case there is but one method open—it is to begin again *de novo* upon another form.

Great vigour of movement now ensues, but of an irregular kind; and instead of being forward, is from side to side; when suddenly the distance between the two parts widens, as in fig. 8, a stretched fibre of sarcode alone uniting them. Movement is still apparently concerted, and is *from* each other, the fibre of sarcode becoming speedily thinner until it has stretched to double the length of an ordinary trailing flagellum; this is shown in fig. 9; and in a few seconds more, by a vigorous movement of each body in opposite directions, as the arrows indicate, the fine filament of sarcode snaps in the middle, and two perfect forms are thus set free by fission.

The process is carried on with great vigour, and is *apparently* the only metamorphosis to which the organism is subject. In nine separate cases a single form was carefully followed, that is to say, a vigorous form in each case was watched from its first act of fission; one part of the divided organism being constantly followed. The whole process of division, from its visible beginning to its close, took place in from four to seven minutes; and was recommenced during the first hour in periods not exceeding three minutes. For the next two hours the intervals would be from seven to ten minutes, and after this the intervals became indefinite, not being less than twenty nor more than forty minutes.

Following the first separated segments, as represented by one part of its divisions, in all its subsequent separations, as begun and carried on by nine separate organisms in which fission happened for the first time, the terminus of the process of fission in the last of the segment-forms in all the cases, was *death* in six of the nine instances, and entire metamorphosis in the remaining three. In the cases in which death ensued the process of fission was continued for seven hours in one case; six hours in three cases; and five-and-a-half hours in the remaining two. In the cases in which metamorphosis took place, three hours in one case, and four hours in the remaining two, terminated the period of fission. There was then inaction in this last segment-form, and the appearance indeed of the loss of vitality; but the first indication of a difference between this and the more frequent cases of vital collapse, was the rapid "clubbing," or gathering into knots of the trailing flagella, followed by a rapid enlargement of the nucleus, and a glittering, rapidly amoeboid condition of the entire body. This condition is shown in fig. 10. The changes are now very rapid: not more than seventy seconds elapse before the trailing flagella are wholly fused with the body substance, and while the head-and-neck-like protuberance pointed out in fig. 1, is preserved, the body, having lost the lateral or trailing flagella, becomes oval; with an immensely developed nucleus, as shown in fig. 11. It swims now with great ease, but always merely in a straight line, and always takes a sudden dart backwards before changing the direction of its motion. It may swim in this way for from a quarter to half an hour; but during this time a band of gra-

nules is formed, at first faintly, and afterwards very distinctly, which is shown at *a*, fig. 11. In this condition it swims directly into the midst of a group of forms in the springing state shown in fig. 2, and the utmost care is needed to keep it in view; but as a rule not many seconds ensue before it has firmly attached itself to one of the springing forms, which at once unanchors itself, and both together swim freely and vigorously about, as shown in fig. 12. They now swim in concert for a very variable period, but generally from thirty-five to forty-five minutes, when their movements become sluggish; the trailing flagella of the lower form become inert and fall upon, and become fused with, the mass of sarcode; meanwhile the bodies have rapidly been uniting, and the two nuclei become fused together, as seen in fig. 13, while the anterior flagella become, in their extremely sluggish movements, at last, entangled with each other, and also melt together, as shown at *b*, and all movement ceases. In the course of ten minutes, from this condition, all trace of the separate bodies is lost, a more or less regular oval form is taken, as shown in fig. 14; at the same time a slow amoeboid disturbance of the sarcode is visible, which diminishes until perfect fusion results, which occurs in the course of twenty minutes, all trace of the nuclei having vanished; and the oval gradually elongates until it reaches the tight, glossy, still condition shown in fig. 15.

This, as previous experience had led me to expect, proved to be a cyst charged with spores. On the first occasion, I anticipated a long continuous watch; but at the end of three hours there was a sudden falling in the middle of the spindle-shaped sac, and a general alteration of form, which led me to earnest effort to discover some discharge. And that it was taking place there could be no doubt, for there was a rapid diminution in the size of the sac; and there were apparent fractures in the delicate investing membrane; but nothing more than cloudiness at the ends could then be made out. I was using the $\frac{1}{16}$th new formula lens (dry), and exhausted all expedients, in vain, to make out the real cause of the change; and as it is hopeless to attempt to change a lens successfully under such circumstances, there was nothing to be done but await another opportunity; using another lens. This $\frac{1}{16}$th had proved of great value throughout in the detection of minute structure and change of form; but the kind of illumination which best reveals extremely minute semi-opaque bodies, is the perfect central illumination which I have endeavoured to describe in the "Monthly Microscopical Journal," vol. xv, page 165. That is to say, the illumination of the whole field through a circular aperture in the diaphragm from the $\frac{1}{80}$th to the $\frac{1}{100}$th of an inch in diameter, through the centre of which aperture the optical axis of the sub-stage condenser, if continued, would pass. But for this special method the new $\frac{1}{16}$th is not so efficient in my hands as the $\frac{1}{25}$th, $\frac{1}{35}$th, or $\frac{1}{50}$th.

Having, therefore, ultimately secured another sac, I determined to use the $\frac{1}{35}$th, as being, as I believed, the most suitable lens for the purpose. In the still condition of the sac, ample time was given for the most delicate adjustment of the light and of the lens, and the watching was begun under the best conditions.

Not the slightest movement, either within or without, is visible while the cyst is in this condition; and the time over which this inactivity extended was never less than three hours, nor more than five hours, in the eight separate instances which I studied. In the second instance the length of time which elapsed was four hours. At the end of that time, without reason discoverable at the moment, there was as before a falling in of the centre of the cyst, and an irregularity in its shape, with a visible and rapid diminution in size. At first nothing but a cloudiness at the two ends could be discovered; but, by a little delicate adjustment of light and lens there became distinctly visible an outflow, from the two ends and the middle of the cyst, of the most exquisitely minute particles which I have ever seen; and as the quantity flowing out diminished, their segregation became more complete, and their clearness and independence more manifest.

The nature of the "continuous stage"—which keeps the drop of fluid under examination from evaporating—is such as to compel the use of the microscope in an upright condition, in order that the stage of the instrument may be perfectly horizontal. On this account the ordinary "camera lucida" is of no avail, or only of use by special and difficult arrangements; but a beautiful little instrument, made by M. Nachet, of Paris, specially meets this emergency, and enables us to make camera lucida drawings with great ease and accuracy. I had, therefore, by anticipation, arranged this apparatus: and when the outflow had continued for about five minutes, made a drawing of the cyst, of which fig. 16, Plate 9, is an accurate copy. As, of course, the minute particles were in rapid movement of outflow, they were put into the drawing, not with camera lucida, but subsequently; and they accurately represent what was seen at the time. The emission continued for about forty minutes, becoming more and more feeble, until at last there was no more movement. At this stage there were many of these minute particles well segregated round the ends of what remained of the cyst; and then I was able, not only to make a drawing of the delicate white film of the cyst, but to indicate by fine dots the relative appearance of the cyst and the still particles. This drawing is reproduced at fig. 17.

The entire details of the subsequent history of these minute bodies were of course only made out by several successive and continuous observations on the emissions of different cysts. But, in each case, the lens was fixed upon some of the minute bodies that remained near the exhausted cyst, and then the observation was unbroken

and continuous to the end. When first seen, these exquisitely minute specks are strongly opaque; but as they are attentively watched with a magnification of 5,000 diameters, they, in the course of from twenty to forty minutes, become quite clear, and would be much more difficult to find in this condition than when first poured out. But, following upon this, there is a distinct elongation, which is indicated at fig. 18, and this proceeds so that, at the end of an hour and a half, they were in the shape and relative size shown at fig. 19.

On the first and second occasion on which the observations were made, very little was made out, except the succession of the metamorphoses; but on subsequent occasions, when a careful scrutiny for the discovery of delicate changes was made, the clear, sharp, defining power of the new $\frac{1}{16}$th objective was of great service; by its means I was enabled to discover the first indication of the origin of the anterior and lateral flagella in the developing germ. At the end of an hour and a half after emission from the sac, the long oval form which the germ had taken (indicated at fig. 19) was always associated with a sharpening at one end. This became rapidly sharper and longer, as seen in fig. 20, at 4 and 6. This always developed into the anterior flagellum. But at about the same time—generally about two hours after emission from the cyst—there were pushed out two delicate points of sarcode laterally, and at right angles to the beak in front; 1, 2, 3, 5, fig. 19, are copies of camera drawings of this condition in the minute germinating body; and the extremely delicate lateral points were found by close watching to develop into the lateral flagella of the perfect form. The growth was now much more rapid; fig. 21 represents the changes that had taken place in forty minutes after the condition depicted in fig. 20. And now slight movements began in some of the better developed forms, such as *a*, fig. 21. They were not changes of place, but movements of the lateral and anterior flagella, and occasionally a change of the direction in which the bodies lay. In the course of another hour the neck-like protrusion at the base of the anterior flagellum generally appeared as shown in *a*, *b*, fig. 22, and very soon—in every instance less than a quarter of an hour after—short movements of translation begin, which resulted in ten minutes more, in graceful swimming. Fig. 23 is a copy of a drawing made of one of these forms that had reached the condition the drawing depicts in a little over four hours from the time of emission from the sac. It was then the $\frac{1}{8000}$th of an inch in long diameter, just one-half the size of the adult; but it was perfect in form, even to the possession of a nucleus, and swam with freedom and grace. Its increase of size from this time was even more rapid, for in the course of another hour it had passed through the size and condition seen in fig. 24, to the normal parental size depicted in fig. 25. And if, after it had reached this state, it were still persistently followed, it might be in ten or

fifteen minutes, but certainly within an hour, the first signs of fission would show themselves, as indicated in *a*, *b*, *c*, *d*, fig. 25; and from that time fission, in successively separated individuals, continued in active progress for hours, as before described.

Thus the life-cycle was completely made out.

It may be noticed:—

1. That the method of fission is both delicate and complex, being not merely a separation into two parts of a particle of living protoplasm, but a division of distinct organs, such as nucleus and flagellum.

2. That the transformation of the individual form, at the terminus of a series of fissions, into an apparently new organism, prior to genetic blending (as shown in figs. 10, 11, Plate 8), is remarkable. And

3. That the life-cycle of this form is rigid, and marked by no caprice. It passes regularly through the same metamorphoses. If seen in any given state, it could be positively affirmed through what stages it had passed, and through what remaining stages it must subsequently pass, to reach the adult condition.

It now remained to determine whether the spore, or germinal particles, emitted by the sac, possessed any capacity to resist the action of heat not possessed by the fully developed form.

It was first carefully determined, by a method to be presently described, that the adult organism never survived after exposure for five minutes to a temperature of 142° F.

For determining the amount of heat resistance possessed by this spore, two methods were employed:—

1. The ordinary three by one inch slips of glass, used for microscopical purposes, were cut into three pieces, each of which was thus an inch square. These could be put into and worked with the "continuous stage." The septic fluid containing the organism was placed upon it, and covered in the usual way with the thinnest covering glass. It was then watched with suitable lenses until, either in that or in some successive drop of the fluid, a cyst was seen to pour out its spores, as in fig. 16, Plate 9. It was then at once taken and placed in a small brass chamber, into the middle of which the bulb of a delicate thermometer was fixed, the tube fitting air-tight into the cover of the chamber, and the expansion of the mercury being read off outside.

The small square of glass containing the fluid and spores was placed, as it came from the stage of the microscope, in the middle of the chamber. The chamber was then closed, and heat so applied as to slowly raise the temperature of the air within. In half an hour it had reached the temperature I desired, which was 210° F.; and the chamber was kept accurately at this point for ten minutes.

The square of glass was then taken out, cooled, and examined, and, of course, to the lens presented a dry amorphous aspect.

Previously some of the fine capillary tubes known as "vaccine tubes"

had been filled with water, to which a little filtered fowl broth was added as nutriment. These tubes were then hermetically sealed, and heated up to 310° F. in a digester. These were kept in absolute alcohol, so that the outsides of the tubes might not contract dust, and so forth.

The contents of these tubes were now used to remoisten the dried film beneath the cover-glass. The ends of the tubes were broken off, and by capillarity the fluid was drawn beneath the cover, at the edge of which the now open end of one or more of the tubes was applied. In this way a plentiful supply of moisture was given.

It was now placed again in the "continuous stage" and examined. At the end of four to five hours the fluid film looked almost everywhere clear, and as it appeared before heating; but I was not able by any method I employed to discover the minute spores, nor could I find in this one the earlier stages of development; but at the end of seven hours, while "searching" with a low angled $\frac{1}{12}$th inch objective, I saw three forms in about the state of development shown in fig. 23, Plate 9; and in the course of another hour a great increase in their number had taken place, and in three separate instances they were followed from the time they were first seen into full development and the commencement of fission.

When more cysts were found this entire process was again repeated twice at the same temperature, and at the expiration of ten and eleven hours respectively adult forms in full activity were found; and in the latter of these two instances I discovered the organism when it was in the still condition of development, represented in figs. 20 and 22, Plate 9. It was thus manifest that by this method of heating a temperature of 210° F. could be completely resisted.

Following the above process in every particular, I next, as they could be found, subjected the spores to 260° F. I did this on three glass squares (as before) in succession. But in no one of these did the organism reappear, although examination was repeatedly and carefully made for five days.

It thus appeared that a temperature of 260° F. applied in this way was fatal to the form, for on taking a needle-point of the septic fluid containing the organism in abundance, and, touching the edge of the fluid with it, I found that in the course of four or five hours there were hundreds swimming in full vigour, showing that the fluid was capable of sustaining the organism, if it were in a vital condition in the fluid.

I next experimented at 10° F. lower, viz., 250° F. I did this on the contents of two separate cysts. In the one case, after ten hours, there was a re-appearance of the living organism, but only two or three adults could be then found; and the increase that afterwards ensued was dependent upon fission, for no small forms could be seen. In the other case, there was rather a larger number of adults at the end of

between nine and ten hours, but their number was far fewer than in those instances where a lower temperature had been employed.

I followed this with four more experiments, separately and successively made. Two of them were at a temperature of 248°, and two at 252° F. In both of the former, at the end of nine or ten hours, the complete organism in full vigour could be seen ; and, in one of the cases, it was discovered in the still condition shown at fig. 20, Plate 9, and watched until the organisms had attained the condition indicated in fig. 24, Plate 3.

But in the two latter instances (heated up to 252°) the living form did not re-appear during the six days following, although repeatedly looked for.

I concluded, therefore, that the temperature of 250° F. was the limit of endurance which the spore of this form could bear by this method of heating.

Now, it must be noticed that in testing the death-point of the adult, as I shall presently show, a fluid heat was employed. But the spore can hardly be said hitherto to have been heated in the same conditions. Doubtless, in the heating chamber the higher temperatures are endured after evaporation, and consequently the heat is "dry." But after years of thought on the matter, no other way has suggested itself to me as possible, for the attaining of the result, by other means. What is required is, that we should know beforehand, that in the drop of fluid exposed to a given heat, the spores of a given organism *in a freshly emitted condition*, and, therefore, before development had begun, were present. And, moreover, that it should be so placed that after heating it should be in such conditions as would admit of the use of the highest power lenses to discover if the spores, known to have been deposited there, would, after subjection to the thermal conditions given, develop as they had been demonstrated to do before heating.

Now, there are two ways of doing this. The one is approximate. It is by long and intimate acquaintance with the history of the organism, and a careful and close study of the condition of the fluid—by which means it is possible to come very near to the highest probability—that the spores in the required condition are there. But in this case negative results must be extremely perplexing. The other way is to see the spores actually emitted in the drop of fluid that is to be heated. This is the only certain method ; it is most exhausting and laborious, but its results are certain, and therefore I have employed it in this case. But to adopt the supposition which Dr. Bastian has recently done,* that to heat a given monad spore in a given condition

* On the Conditions Favouring Fermentation, &c., "Linnean Soc. Jour.," vol. xiv. This paper contains some curious inaccuracies of statement as to facts—evidently the results of misapprehension on Dr. Bastian's part—concerning the results of thermal experiments made by Dr. Drysdale and myself on the Monads. These occur

in a fluid is "perfectly easy," is a serious mistake. He thinks that we have nothing to do but to "put one or two drops of the fluid into a small tube . . . . hermetically seal it, and then heat it for ten minutes or more to different degrees before subjecting the fluid to a prolonged microscopical examination."

Certainly this is a "perfectly easy" method; but it must fail entirely to secure the end in view. What certainty could we have that the spore—in the only useful condition for experiment—the freshly emitted state—would be in the "one or two drops?" Spores in various stages of development undoubtedly would be there. But I make no affirmation about these. There can scarcely be a doubt that germs *partly developed* must succumb at a far lower temperature. Then it follows that the certainty and the uniformity of results must depend upon the certainty we possess, as to whether the spores were in the fluid before heating or not. And when we remember that the phenomenon of fission may, in some monads, go on in a drop of fluid under examination as the sole method of increase for a week or more; and that, in all cases the production of the spore is extremely rare in relation to the other morphological changes, the force of this will be even more manifest.

But in thinking out the best method of determining as delicately as possible the thermal death-point of the adult, it occurred to me, that having demonstrated the existence of the spore, and having watched the development after it had endured given heat-conditions, that it would be possible to adopt means for comparing the differences that might exist between the destructive influence of a given temperature endured in "dry" and moist conditions respectively.

My plan was to arrange the form of the vessel in which the fluid was heated, so that without subsequent transference or exposure the fluid could be directly examined by the lens. This end was secured by means of extremely delicate glass blowing. The apparatus is presented in diagram at figs. 26 and 27.* In fig. 26 A is a hollow bulb, intended as a reservoir for an infusion containing any required organism or organisms. The infusion is put in through the funnel and tube B. The bulb A opens into a tube D on the opposite side,

on the 77th page of the paper, and will be referred to in a paper which I hope shortly to send to the Royal Microscopical Society.

* A large number of these pieces of apparatus have been made for me by a friend, an amateur. But for exquisitely finished and accurate work I have been enabled to obtain them nowhere as they are produced by Mr. Gimingham, whose remarkable dexterity in the production of the radiometer bulbs is well known. He has from time to time made them for me, in various forms, and in a manner that leaves nothing to be desired. The principle of the apparatus is one that was adopted jointly by Dr. Drysdale and myself in the study, still proceeding, of the bacteria. But the forms of the apparatus, as described above, were made for these present investigations.

and this tube terminates in a delicate closed and flattened cell C. In a microscopical point of view this cell is the most important part of the apparatus. It is a flattened bulb, and its upper and under walls are films of glass, varying in different pieces of the apparatus from the $\frac{1}{50}$th to the $\frac{1}{200}$th of an inch in thickness; and the space between these walls may vary from the $\frac{1}{20}$th to the $\frac{1}{100}$th of an inch in depth.

We thus have a perfect cell, completely closed, the contents of which can readily be studied with the aid of the most powerful lenses. The walls of the cell are of course not as absolutely even and smooth as the thin "covering glass" usually employed with high power lenses: but it is in the majority of cases beautifully level and clear, when the manner of its production is remembered. Of course I should never have employed these cells for the purpose of discovering delicate and unknown details; but they answer admirably for determining the presence or absence of phenomena, the nature of which is well known beforehand.

It is manifest then, that if a fluid be put in the bulb and stand at the level of the dotted line A, that it will fill and be in communication with the cell C.

E is a hollow bulb filled with calcined air, and hermetically sealed; but it does not communicate with the bulb A on account of the presence of a thin glass partition or septum F. The object of this is, that when by boiling, the air has been driven out of the bulb A, and the whole interior space has been hermetically sealed at J, air may be again introduced; which is accomplished thus: H is a pointed piece of platinum wire, heavy enough by a sharp shake to break the septum F, but too large in diameter to pass through the neck G, but of course the calcined air immediately enters and restores to the fluid its normal conditions.

Fig. 27 is precisely the same as the above, the same letters referring to the same parts in both; but it has an addition to it marked MN. This is a tube opening at N into the bulb A, but until needed the communication of the tube with the interior of the bulb is prevented by the thin partition L. The object of this is, that supposing a given infusion to have become sterilized in relation to a certain organism, by any ascertained temperature, to determine whether nevertheless the fluid is still capable of *sustaining* the organism if it be reintroduced. To do this, a piece of platinum wire, as before, is taken and touched with a fluid in which the living organism abounds; it is then placed in the tube K. The tube is then sealed at M; the piece of platinum is shaken sharply, breaks the septum L, and falls into the fluid, inoculating it. In relation to the particular organism now being considered, this, however, was not required.

The first important matter to be taken into account was the death point of the adult. A piece of apparatus similar to fig. 26 was used.

When the fluid containing the organism was inserted, a large number of the organisms was seen by the lens in the cell C. The whole apparatus was now placed in a copper vessel with cold water, the water just covering the bulb A, and a thermometer was placed in the water. A Bunsen's burner was used for heating, which had minute jets springing from a tube bent in the form of a parallelogram, corresponding to the form of the vessel. The heat was now applied, so that the water rose in temperature with extreme slowness. It was taken out directly the thermometer read 100° F., and at once examined. Several of the organisms were at once visible: but the great majority of them were quite still, and the only movement discoverable was a more or less languid movement of flagella, the bodies being motionless. In seven minutes a form sailed slowly across the field, but I preferred to keep my attention fixed on those which I had seen from the first. In the course of three-quarters of an hour the flagella of ten forms were all vigorously working, and the bodies began to move, and in an hour and a-half from the time of being taken out, the whole field was as active as before heating.

In the same manner the fluid was next raised to 130° F. On examination the immediate effect was more marked, the inaction more complete. Only one flagellum was seen to move in the whole field. But in the course of half an hour the flagella were gently swaying, and in two hours the same individuals were swimming freely.

When, however, the fluid was raised to 140° F., the inaction was absolute when the cell was first examined, and only one-fourth of the individuals in a selected field revived. These did so in the course of three hours, but the remainder did not move to the end of the day, and were in the same position and inactive on the morrow; and it was subsequently shown, by the same means, that a temperature of 142° F. was entirely destructive of the vitality of the adult.

The next step was to ascertain the effect of the boiling point upon the *spores*.

To be quite sure that spores, in the condition required, were in the fluid, a thin circle of glass was cut that would lie upon and take the place of the ordinary floor of the "continuous stage." By this means if a deposit of spores were seen to take place, it could be readily removed bodily from the microscope. On the under surface of this circle of glass a few lines with the diamond were made, in convenient places, to facilitate its subsequent fracture. On this then, in the stage, properly covered as usual, the fluid was hunted and watched, and fresh drops taken until a cyst was seen, and then watched until it burst. The circle of glass on which this happened was now taken carefully away from the microscope and broken, so that the pieces containing the fluid drop fell into the funnel B, fig. 26. The fluid containing the organism in abundance was now poured over these down the tube and

into the bulb A, carrying with it the fluid on the fragments of glass which contained the just emitted spore. Thus it was known that spores in the condition required were actually in the apparatus.

The whole was now placed in a suitable vessel with solid white paraffin, which was slowly melted, covering when in a fluid state the whole bulb A. The heating of the paraffin was extremely gentle, and it was raised as much above the boiling point of water as was consistent with a *gentle* ebullition of the infusion A. This was continued for ten minutes. The tube J was then hermetically closed before the ebullition had ceased, and was removed from the paraffin and carefully wiped while hot. The piece of platinum H was now shaken sharply upon the septum F, and the calcined air contained in E was admitted into the bulb A, the platinum remaining on the top of the passage G, which was too small to admit of its entrance into A. In this way the normal conditions of the infusion were as far as possible restored.

The cell C was now fixed upon the stage of the microscope, and could be carefully examined with the most suitable lenses; and from the fact that the cell was closed, "immersion" lenses could be employed, which gave an advantage in working.

I first employed an immersion $\frac{1}{16}$th. But nothing was at all visible but Brownian movement, and during the next seven hours no trace of the organism could be found. The whole was now left for six hours more, and then the entire cell was carefully "searched" with an immersion $\frac{1}{12}$th. At the close of an hour and a half three of the organisms, one in the adult condition, and two in the semi-developed state shown in fig. 24, were found swimming freely; and in the course of the next few hours the cell was freely visited by the organism in full vigour.

The length of time that elapsed before the organism appeared in the cell, I presumed to be explicable by the fact, that it took a considerable time for it to arise, in sufficient quantity, in the reservoir A to render it probable that it would migrate into the cell C; and this supposition was sustained by subsequent facts.

It was clear, then, that 212° F. was a temperature not destructive of the germ when endured in a fluid condition.

To reach higher temperatures it was, of course, needful to employ a digester. For this purpose tubes alone were employed, which were terminated in the cell C, fig. 26, but the bulb A and all its appendages were dispensed with; the tube D, for example, terminating at one end in the delicate cell C as before, but at the opposite end in the place of A was a simple funnel for the delivery of the infusion into D. When the spore in the required condition had been found and placed in the funnel as before described, the fluid was inserted as before, and the tube was then hermetically sealed.

It was now placed in a small digester, with a register for pressures,

and heated up to the temperature of 240° F., 10° F. below the death-point in the dry condition. It was kept at this for five minutes.

As soon as it could be conveniently taken from the digester it was placed in a "cradle" on the stage of the microscope. Nothing but violent Brownian movement was visible anywhere; and nothing more than this could be seen by the end of twelve hours, although the search was very strict and systematic. For the next twenty-four hours no other result ensued, and at the end of six days there was no trace of the organism. The question now arose as to whether any incapacity to sustain the organism had been superinduced. For this purpose the tube was opened at its sealed end, and a small piece of platinum wire which had been immersed in the fluid containing the living organism was dropped into it. It was then sealed again and examined. In the course of twenty minutes seven of the introduced organisms were seen, and in the next two days they were present in great abundance.

It thus became manifest that the previous sterility of the fluid in the cell was due to the destruction of the germs, or spores, by the heat to which the infusion had been subject.

Another tube was now taken, and after preparation in all respects in the same way, was heated in the digester to 230° F., and kept at that for five minutes.

The result was precisely the same as in the preceding instance. There was no trace of the living organism from the first, on to the end of a week. But in this case also, inoculation reintroduced it successfully, and it flourished.

I now determined that 220° F. should be tried; and after the tube and its contents had been arranged as before, it was heated to that point, and kept in that condition for five minutes.

When first examined with a suitable lens, as before, nothing but the well-known Brownian movement was visible. And this remained true for the first five hours, although the whole cell was carefully examined with the new formula $\frac{1}{12}$th and $\frac{1}{16}$th inch objective, and repeatedly "searched" with the $\frac{1}{8}$th. But during the sixth hour I became convinced that the organism in a developing condition was present; it was in a part of the cell immediately under the tube; and in the course of a few minutes I was perfectly satisfied that the organism in the condition represented at fig. 22 was there, but only five could be seen; these, however, increased in size as usual, and in the course of forty minutes three of them began gracefully to move, and following one of them I saw it attain to the usual adult condition. The next day the cell had scores of the organisms swimming in it.

This experiment was now repeated, every detail of preparation being as before. The results were similar; that is to say, the living organism reappeared; but it was not found until the seventh hour, and then it was freely swimming and in an almost perfect condition. But I could

not at this time find more than this one; but in the course of some hours several more were present, perhaps as the result of fission.

From this it appeared plain that the spores had survived a temperature of 220° F. in a fluid heat: but by a series of subsequent tests conducted in precisely the same way, I found that if 220° F. might not be considered the limit of temperature which the spore could survive, at least this organism never showed itself again after heating the spore to 222° F.

It thus appears that in this particular organism the dry heat is considerably less destructive of the vitality of the spores than the moist heat. That there is, indeed, a difference of 28° or 29° F.; or, speaking broadly, we might say 30° F.

This is undoubtedly a difference of considerable importance. But a temperature of 220° F. resisted successfully at all, implies of necessity actual protection of some kind. I shall not attempt to theorize upon or suggest what that may be; but it is manifestly not beyond the reach of chemical and physical science to approximate to an explanation, whilst biological science furnishes analogy in higher and more complex departments of its researches. One thing is absolutely certain; which is, that the *optical condition* of the freshly emitted spore is quite distinct from that which it presents in from three-quarters of an hour to an hour after development has commenced. It is to the comparative opacity of the newly emitted germs that their visibility at that time is due. Hence they are most certainly discovered by means of a shaft of almost parallel light the $\frac{1}{100}$ of an inch in diameter at right angles to the plane on which the spores are lying, and their imperfect transmission of this reveals them. But in the course of an hour they become far more transparent than even the adult form. This is undoubtedly the result of the vital processes involved in germination. The sarcode is in a fluid condition and cannot resist the heat that surrounds it—a constant current of liquid must be passing in and out of the sarcode by imbibition and exosmosis. The result must be the circulation through the sarcode of fluid at a temperature destructive of the vital processes. Hence the destruction of the adult at 142° F., and there can be little doubt but the young germinating form would be destroyed, for the same reasons, at the same or—perhaps from the probably greater intensity of the vital processes—even a lower temperature. But in the spore, the vital activities of the developing protoplasm have not commenced; the sarcode is in all probability in a fixed state; and a protective condition of that sarcode, resisting the diffusion of heat through it, is by no means difficult to understand. Indeed it will be palpably of service to the organisms, and like desiccation enable them to overcome the extreme vicissitudes of condition in temperature, drought, and so forth, to which over the surface of the earth, they must, as septic

organisms, be exposed. But from a series of experiments on the spore of such of the "monads" whose Life-Histories were worked out by Dr. Drysdale and myself, as I have been enabled to get again during the last two years, I have been able to satisfy myself that 30° F. is by no means the fixed difference between the power of thermal resistance possessed by the spores of these forms when heated in a dry and a moist condition. There is considerable variety in this matter, and variety which in all probability, nothing but a perfect acquaintance with the vicissitudes through which by adaptation they have survived in their evolutional history, could explain.

In conclusion, without entering into details, I may observe that these forms have, as far as my observations have extended, a far greater capacity to resist heat than the bacteria; but their distribution is far more limited.

DESCRIPTION OF PLATES.

Plates 8 and 9.

Figure 1. The normal form of the organism described; *d* being the nucleus.

Figure 2. The representation of the mode in which it appears by mechanical means to break up the decomposing tissues of the animal matter in the maceration. *a*, *b*, are points at which its lateral flagella are "anchored;" it then draws its body down, coiling the flagella into spirals seen at *c*; it then springs up and forward in the direction of the arrow *d*, and with the flagella as a radius darts rapidly down upon a piece of tissue in the arc of a circle shown by the arrow *e*.

Figure 3. The first indications of fission are seen at *a*, *b*, and the nucleus has moved to the centre.

Figure 4. Fission is in progress as seen at *a* and *c*, and in the nucleus *b*.

Figure 5. It is still progressing; a pale line runs through the dividing body, and the nucleus has separated.

Figure 6. Fission still further advanced.

Figure 7. Only a neck of sarcode now unites the bodies; this is sharply pulled, and results in a thin fibre connecting two forms as seen in fig. 8.

Figure 9. This has stretched as here drawn; the bodies pulling in the direction of the arrows, until the fibre snaps in the middle, and two perfect forms go free.

Figure 10. At the terminus of a series of fissions in about a third of the cases, the body becomes changed, slow of movement, and amœboid, as here drawn: and then rapidly changes into the shape seen in fig. 11.

Figure 12. The form shown at fig. 11, after forming a band seen at *a* goes into the midst of a group in the "springing" state, and speedily unites itself to one, which at once goes free; and they swim together as in this figure.

Figures 13, 14, 15, 16, 17. Represent different progressive stages of fusion, which terminate in a still sac (15), which ultimately opens and pours out spores (16, 17).

Figures 18—25. Different progressive stages in the growth of the emitted spores.

Figure 26. A piece of glass apparatus for testing the thermal death-point of the adult, and for discovering whether or not the boiling-point in a fluid destroys the germ. The cell *c* enables this to be determined by micro-

scopical examination directly, without any transference of the infusion; and the bulb E being filled with calcined air, enables the experimenter, when the whole piece of apparatus is closed at J, to restore air to the bulb A containing the infusion, by breaking the thin septum F by means of the comparatively heavy piece of platinum H, which opens a communication between E and A.

Figure 27. Is the same as the above, except for the added tube MN. This is intended to be used if the fluid should prove to be sterilized, for the purpose of inoculating it again by means of a piece of platinum which is charged with the fluid in which the organism is abounding. This is introduced into the tube, which is at once closed at M. It is then made to break the septum L, and falls into the fluid inoculating it, to discover if it still has the power to sustain the organism.

All the drawings of the organism are magnified 3,000 diameters, except where otherwise indicated on the plates.

II. "On the Reversal of the Lines of Metallic Vapours." By G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. No. II. Received March 26, 1878.

Since our last communication to the Society we have succeeded in reversing characteristic lines of the vapours of rubidium and cæsium. Considering the known volatility of these elements, and the small quantity of their compounds at our disposal, we thought it better to try the effects first in glass tubes. For this purpose a piece of combustion tubing had one end drawn out and the end turned up sharply, and sealed off (like an ill-made combustion tube of the usual form) so as to produce an approximately plane face at the end of the tube; a small bulb was then blown at about an inch from the end, and the tube drawn out at about an inch from the bulb on the other side, so as to form a long narrower tube. Some dry rubidium or cæsium chloride was next introduced into the bulb, and a fragment of fresh cut sodium, and the narrow part of the tube turned up, so as to allow the tube and bulb to be seen through in the direction of the axis of the tube. The open end was then attached to a Sprengel pump, and the air exhausted; the sodium was then melted, and afterwards either dry hydrogen or dry nitrogen admitted, and the end of the tube sealed off at nearly the atmospheric pressure. We found it necessary to have this pressure of gas inside the tube, otherwise the metal distilled so fast on heating it that the ends were speedily obscured by condensed drops of metal. Through these tubes placed lengthways in front of a spectroscope, a lime light was viewed. On warming the bulb of a tube in which rubidium chloride had been sealed up with sodium, the D lines were of course very soon seen, and very soon there appeared two dark lines near the extremity of the violet light, which, on measure-

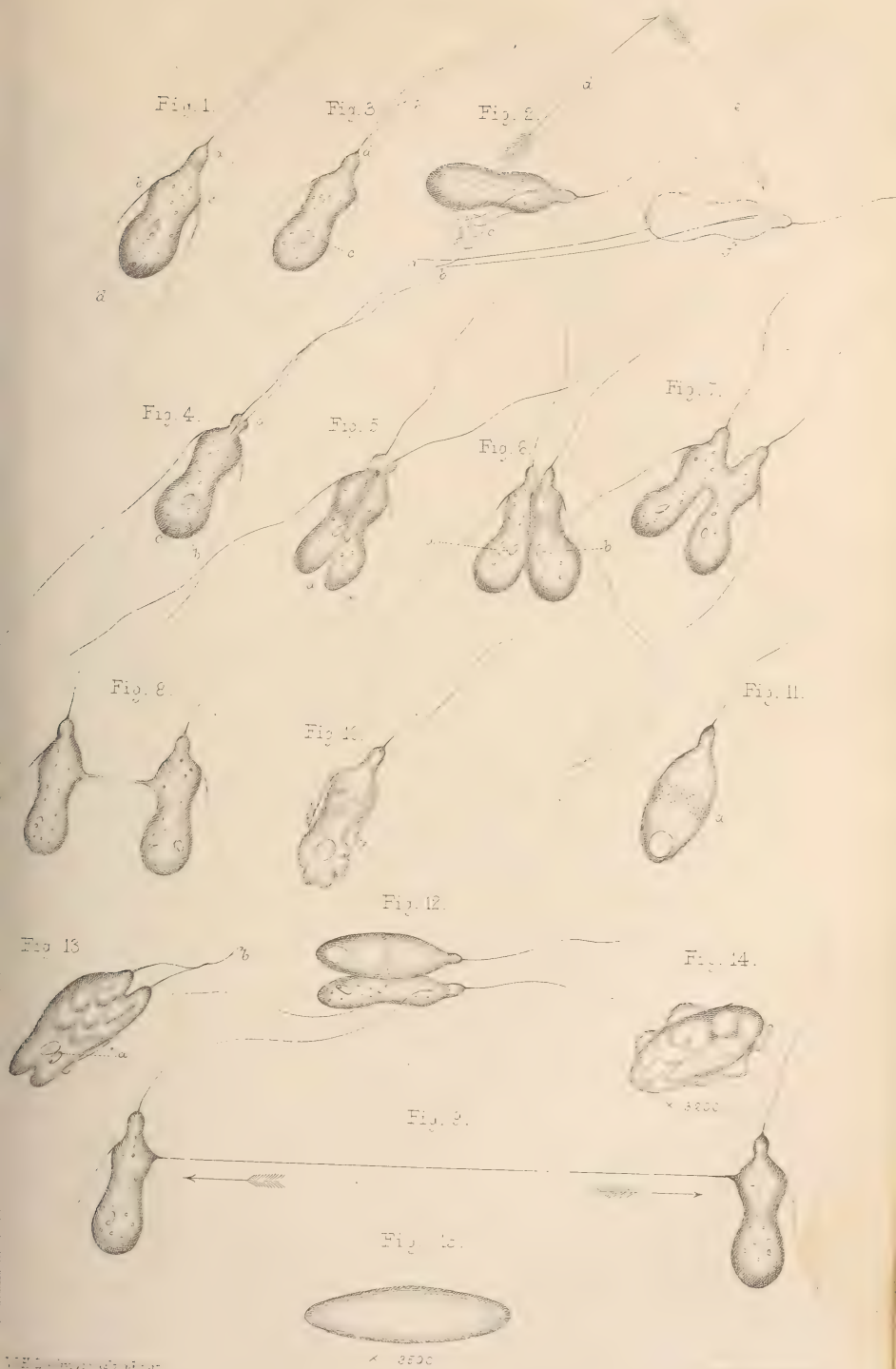


Fig. 1.
Fig. 3.
Fig. 2.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13
Fig. 14.
× 3200
Fig. 9.
Fig. 15.
× 3200

Fig. 16.

Fig. 17.

Fig. 18.

Fig. 19.

Fig. 20.

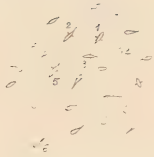


Fig. 21.

Fig. 22.

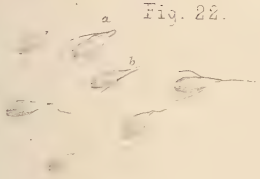


Fig. 23.

Fig. 24.

Fig. 25.

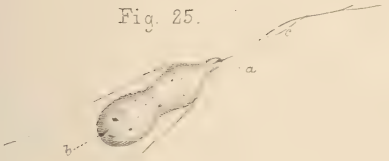
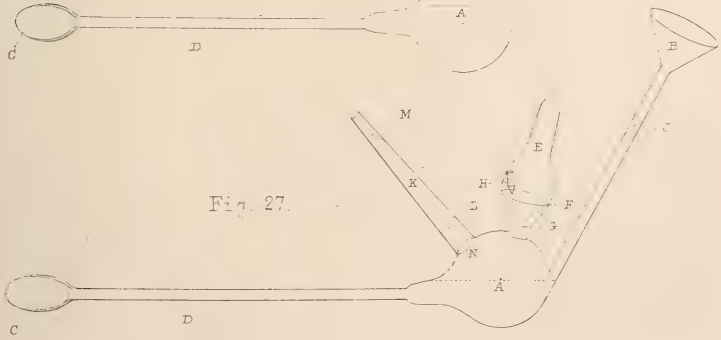


Fig. 26.

Fig. 27.



ment, were found to be identical in position with the well-known violet lines of rubidium. Next appeared faintly the channelled spectrum of sodium in the green, and then a dark line in the blue, very sharp and decided, in the place of the more refrangible of the characteristic lines of cæsium in the flame spectrum. As the temperature rose these dark lines, especially those in the violet, became sensibly broader; and then another fine dark line appeared in the blue, in the place of the less refrangible of the cæsium blue lines. During this time no dark line could be observed in the red, but as the temperature rose a broad absorption band appeared in the red, with its centre about midway between B and C, ill-defined at the edges, and though plainly visible not very dark. The lines in the violet had now become so broad as to touch each other and form one dark band. On cooling, the absorption band in the red became gradually lighter without becoming defined, and was finally overpowered by the channelled spectrum of sodium in that region. The double dark line in the violet became sharply defined again as the temperature fell. There are two blue lines in the spectrum of rubidium taken with an induction-coil very near the two blue lines of cæsium, but they are comparatively feeble, and the two dark lines in the blue which we observed in the places of the characteristic blue lines of cæsium we believe must have been due to a small quantity of cæsium chloride in the sample of rubidium chloride. These blue lines were not, however, visible when some of the rubidium chloride was held in the flame of a Bunsen's burner, nor when a spark was taken from a solution of the chloride; but the more refrangible of them (Csα) was visible in the spark of an induction-coil, without a Leyden jar, taken between beads of the rubidium chloride fused on platinum wires.

When a tube containing cæsium chloride and sodium was observed, in the same way as the former, the two dark lines in the blue were seen very soon after the heating began, and the more refrangible of them broadened out very sensibly as the temperature increased. The usual channelled spectrum of sodium was seen in the green, and an additional channelling appeared in the yellow, which may be due to cæsium or to the mixture of the two metals. We have at present no metallic cæsium wherewith to decide this question. Indeed the cæsium chloride used was not free from rubidium, and the dark lines of rubidium were distinctly seen in the violet.

It is remarkable that these absorption lines of cæsium coincide with the blue lines of cæsium as seen in the flame, not with the green line which that metal shows when heated in an electric spark of high density. It is to be observed, however, that when sparks from an induction-coil without a jar are taken between beads of cæsium chloride, fused on platinum wires, a spectrum similar to the flame spectrum is seen, and it is only when a Leyden jar is used that the spectrum is reduced to a green line. In like manner both the violet lines of

rubidium are reversed in our tubes, and both these violet lines are seen when the spark of an induction-coil, without jar, is passed between beads of rubidium chloride fused on platinum wire, though only one of them appears when a Leyden jar is used.

We have not repeated these observations on cæsium and rubidium in iron tubes, because the light emitted from the hot iron does not extend so far into the violet as the rubidium lines, and the amount of cæsium at our command at present is very small.

We have extended our observations on the absorption of magnesium and of mixtures of magnesium with potassium and sodium, using iron tubes placed vertically in a small furnace fed with Welsh coal, as described in our former communication.

The result of several observations, when commercial magnesium (*i.e.*, magnesium with only a small percentage of sodium in it) was used, is that the absorption produced by magnesium consists of—

(1.) Two sharp lines in the green, of which one, which is broader than the other, and appears to broaden as the temperature increases, coincides in position with the least refrangible of the *b* group, while the other is less refrangible, and has a wave length very nearly 5,210. These lines are the first and the last to be seen and very constant, and we at first took them for the extreme lines of the *b* group.*

(2.) A dark line in the blue, always more or less broad, difficult to measure exactly, but very near the place of the brightest blue line of magnesium. This line was not always visible, indeed rarely when magnesium alone was placed in the tube. It was better seen when a small quantity of potassium was added. The measure of the less refrangible edge of this band then gave a wave length of very nearly 4,615. It was also seen when sodium and magnesium were used together, but it was more difficult to get a measure of it in this case, as the sodium obscures the blue part of the spectrum. A measure taken when sodium was used gave a wave length 4,580, but this can only be regarded as an approximation.

(3.) A third line or band in the green rather more refrangible than the *b* group. This is best seen when potassium as well as magnesium is introduced into the tube, but it may also be seen with sodium and magnesium. The less refrangible edge of this band is sharply defined, and has a wave length about 5,140, and it fades away towards the blue.

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

But besides these, other absorptions are seen which appear to be due to mixed vapours.

* On one occasion a dark line corresponding to b_2 was also seen.

(4.) When sodium and magnesium are used together a dark line, with ill-defined edges, is seen in the green, with a wave length about 5,300. This is the characteristic absorption of the mixed vapours of sodium and magnesium, it is not seen with either vapour separately, nor is it seen when potassium is used instead of sodium.

(5.) When potassium and magnesium are used together, a pair of dark lines are seen in the red. The less refrangible of these sometimes broadens into a band with ill-defined edges, and has a mean wave length of about 6,580. The other is always a fine sharp line, with a wave length about 6,475. These lines are as regularly seen with the mixture of potassium and magnesium as the above-mentioned line (5,300) is seen with the mixture of sodium and magnesium, but are not seen except with that mixture.

(6.) On one occasion, with a mixture of potassium and magnesium, another dark line was seen in the blue, with a wave length nearly 4,820. This line is very near one of the bright lines, seen when sparks from an induction-coil, without a Leyden jar, are taken between electrodes of magnesium, and may very likely be due to magnesium alone, and not to the mixture of vapours, as we only observed it on one occasion.

There is a certain resemblance between the absorptions above ascribed to magnesium, and the emission spectrum seen when the sparks of a small induction-coil, without Leyden jar, are taken between electrodes of magnesium. This emission spectrum is the same, with the addition of some blue lines, as that seen when the sparks are taken from a solution of magnesium chloride, as accurately described by Lecoq de Boisbaudran (*Spectres lumineux*, p. 86), and as that seen in burning magnesium (Dr. W. M. Watts, Phil. Mag., 1875), and consists, as we have observed, of lines with the following wave lengths:—4,481, 4,590 (Thalèn 4,586), 4,570 (Thalèn 4,565), 4,797, 4,930, 4,945, 4,957, 4,969, 4,980, 4,990, 5,000, the well-known *b* group, and 5,528 (Thalèn).

The pair of lines (1) correspond nearly with the *b* group, but slightly displaced towards the red; the shaded band (3) corresponds less closely to the series of seven lines 5,000 to 4,930, which progressively decrease in brightness towards the blue, and is also a little less refrangible than that series; the broad line in the blue (2) corresponds to the pair of lines 4,570 and 4,590; and the remaining line (6) with the line 4,797, also both displaced a little towards the red. No absorption corresponding to the extreme lines 4,481 and 5,528 was observed. There is plainly no exact reversal except of the line b_1, and even in that case it may be an accident if we suppose the two dark lines (1) to represent the extreme line of the group *b*. It may be noted in connexion with this that the absorption lines described by us in our former communication as seen with sodium and potassium

(wave lengths 5,510 and 5,730) are near to, but *more* refrangible than, well-known emission lines of those elements.

The absorptions produced by the mixtures of vapours plainly offer a wide field for further observation. At present we have not succeeded in observing those produced by mixtures of vapours other than those here recorded; and it seems needful to use tubes of a less fusible material than iron, which, notwithstanding the protection of fire-clay, very quickly gives way at the temperatures we have used.

With regard to those above described, we may observe that there is in the solar spectrum an absorption line, hitherto unaccounted for, closely corresponding to each of them. Thus, on Ångström and Thalèn's map there are dark lines at 6,580 and 6,585, with more or less continuous absorption between them, a broad dark line between 6,474 and 6,475, and a dark line at 5,300. There are also dark lines nearly, if not exactly, coincident with the series of seven bright lines of magnesium above described, which we have not seen strictly reversed. The coincidences of the series of the solar spectrum hitherto observed have, for the most part, been with lines given by dense electric sparks; while it is not improbable that the conditions of temperature, and the admixtures of vapours in the upper part of the solar atmosphere, may resemble much more nearly those in our tubes.

We intend to pursue our observations, using higher temperatures, if we can obtain tubes which will stand under those circumstances.

III. "Preliminary Note on Experiments in Electro Photometry." By Professor James Dewar, F.R.S., Jacksonian Professor University of Cambridge. Received March 26, 1878.

Edmond Becquerel, in the year 1839, opened up a new field of chemical research through the discovery that electric currents may be developed during the production of chemical inter-actions excited by solar agency.

Hunt, in the year 1840, repeated, with many modifications, Becquerel's experiments, and confirmed his results.

Grove, in 1858, examined the influence of light on the polarized electrode, and concluded that the effect of light was simply an augmentation of the chemical action taking place at the surface of the electrodes.

Becquerel, in his well-known work, "La Lumière," published in 1868, gives details regarding the construction of an electro-chemical actinometer formed by coating plates of silver with a thin film of the sub-chloride, and subsequent heating for many hours to a temperature of 150° C.

Egeroff, in 1877, suggested the use of a double apparatus of

Becquerel's form, acting as a differential combination, the plates of silver being coated with iodide instead of chloride.

The modifications of the halogen salts of silver when subjected to the action of light have up to the present time been used most successfully in the production of electric currents, and although mixtures of photographically sensitive salts have been shown by Smee to produce currents of a similar kind, yet no attempt has been made to examine the proper form of instrument required for the general investigation of the electrical actions induced by light on fluid substances.

This subject has occupied my attention for some time, and the completed investigation I hope to present to the Society. In the meantime the following description will give an idea of the method of investigation.

A little consideration shows that the amount of current produced by a definite intensity and quality of light acting during a short period of time on a given sensitive substance in solution, is primarily a function of the nature, form, and position of the poles in the cell relatively to the direction in which the light enters, and the selective absorption, concentration, and conductivity of the fluid.

The diffusive action taking place in such cells complicates the effects and is especially intricate when insoluble substances are formed. In order to simplify the investigation in the first instance, poles that are not chemically acted upon, and a sensitive substance yielding only soluble products on the action of light, were employed. For this purpose platinum poles and chlorous acid or peroxde of chlorine were selected.

The best form of cell had one of the poles made of fine platinum wire fixed as closely as possible to the inner surface where the light enters, the other pole being made of thicker wire placed deeper in the fluid.

As the action is confined to a very fine film where the light enters, the maximum amount of current is obtained when the composition of the fluid is modified deep enough to isolate temporarily the front pole in the modified medium. Under these conditions the formation of local currents is avoided, and the maximum electromotive force obtained.

In cells of this construction the amount of current is independent of the surface of the fluid acted upon by light, so that a mere slit sufficient to expose the front poles acts as efficiently as a larger surface. This prevents the unnecessary exhaustion of material and enables the cell to be made of very small dimensions. By means of such an apparatus the chemical actions of light and their electrical relations may be traced in many new directions.

The amount and direction of the current in the case of chlorous acid is readily modified by the addition of certain salts and acids, and

thus electrical variations may be produced, resembling the effects observed during the action of light on the eye.

Certain modifications taking place in the chlorous acid by exposure to light increase its sensibility, and as a general result it is found that the fluid through these alterations increases in resistance. We have thus an anomalous kind of battery where the available electromotive force increases with the resistance. The addition of neutral substances which increase the resistance without producing new decompositions improves the action of the cell.

Care has to be taken in these experiments to use the same apparatus in a series of comparative experiments, as infinitesimal differences in the contact of the active pole render it difficult to make two instruments giving exactly the same results. Cells have been constructed with two, three, and four poles, and their individual and combined action examined. Quartz surfaces have also been employed instead of glass, thus enabling the chemical opacity of different substances to be determined.

The electrical currents derived through the action of light on definite salts are strong in the case of ferro- and ferri-cyanide of potassium, but remarkably so in the case of nitroprusside of sodium.

Of organic acids the tartrate of uranium is one of the most active. A mixture of selenious acid and sulphurous acid in presence of hydrochloric acid yields strong currents when subjected to light in the form of cell described. The list of substances that may be proved to undergo chemical decomposition by the action of light is very extensive; full details will be found in the completed paper.

IV. "On the Determination of the Scale Value of a Thomson's Quadrant Electrometer used for Registering the Variations in Atmospheric Electricity at the Kew Observatory." By G. M. Whipple, B.Sc., Superintendent of the Kew Observatory. Communicated by Robert H. Scott, M.A., F.R.S. Received April 3, 1878.

The Meteorological Council, being desirous of discussing the photographic traces produced by their electrograph at the Kew Observatory some time since, requested the Kew Committee to institute a series of experiments, with the view of determining the scale value of the instrument, in order to prepare a suitable scale for measuring the curves.

The Kew Committee, at their meeting in November, entrusted the matter to me, and accordingly, having obtained the loan of a battery of 300 Bunsen cells, some preliminary experiments were made, which

showed that the greatest potential which could be obtained with them was very inadequate for the purpose.

Having named this fact to Dr. De La Rue, he very generously placed his large chloride of silver battery at the disposal of the Observatory, and by its means we have been able to test the value of the deflections of the instrument at different points of the scale throughout its entire range.

Owing to the difficulty of transporting the large battery the experiments were all made in Dr. De La Rue's laboratory, Charlotte-street, Portland-place, London.

The electrometer (Thomson's Quadrant, No. 19, White, Glasgow, maker) was dismounted from its position in the Observatory on the 3rd December, 1877, careful measurements having first been made of the distance of a mark on the instrument from the source of light and from the point of incidence of the reflected spot of light on the circumference of the cylinder of the registering apparatus.

The acid was then removed from the jar, and the needle fixed for transit.

Before moving the instrument the exact position of the quadrants, which are kept separated to some distance in ordinary use, was marked by lines drawn on the cover, so as to ensure as far as possible that the same inductive power should be acting on the needle during the experiments as there had been whilst the electrometer was in daily use at the Observatory.

The time of vibration of the needle was also carefully determined and noted, with the view of detecting any change that might occur in its sensibility from derangement of its supporting fibres during transit.

The apparatus was then conveyed to Dr. De La Rue's laboratory, and there fitted up on a bed prepared for the purpose, on which the distances as mentioned above were marked.

The jar having been refilled with acid the needle was liberated, and its time of vibration being found unaltered, it was charged positively, and the instrument left for several days in order that its interior might become thoroughly dried.

In the first experiment readings were taken on a scale divided into fortieths of an inch, which was placed in the exact position occupied by the front face of the cylinder of the registering apparatus of the electrograph.

The battery terminals were then attached to the electrodes of the instrument, and the cells joined up in series as required. The deflections produced by the different potentials were read off on the scale with the following results, the quadrants being put to earth at the beginning of each experiment with the view of obtaining a correct value of the zero.

Experiment 1. In this experiment only the potential of the positive pole was measured.

No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
0	0·000	200	1·762	440	3·300	680	4·275
20	0·200	240	2·050	480	3·487	720	4·362
40	0·400	280	2·375	520	3·637	760	4·425
80	0·900	320	2·650	560	3·812	800	4·500
120	1·112	360	2·887	600	3·950		
160	1·450	400	3·100	640	4·087		

In the next experiment the positive deflections were read for potentials increasing by hundreds up to 800 cells, and the negative for potentials increasing by twenties up to 200 cells.

Experiment 2.

Positive.				Negative.			
No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
0	0·000	600	3·725	0	0·000	120	1·200
100	0·900	700	3·951	20	0·125	140	1·450
200	1·701	800	4·051	40	0·287	160	1·575
300	2·387			60	0·525	180	1·850
400	2·951			80	0·725	200	2·100
500	3·401			100	0·975		

The negative deflections in the next experiment were obtained for potentials diminishing from 200 cells. The positive again increased by hundreds.

Experiment 3.

Positive.				Negative.			
No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
0	0·000	600	3·750	200	2·250	80	0·825
100	0·950	700	3·975	180	2·000	60	0·612
200	1·750	800	4·100	160	1·725	40	0·400
300	2·400			140	1·512	20	0·200
400	2·950			120	1·287	0	0·000
500	3·450			100	1·050		

The scale was then dismounted, and a frame substituted for it con-

taining a sensitized sheet of paper (such as is used in the self-registering photographic instrument at Kew), and having a sliding shutter.

The image of the illuminated slit reflected from the mirror of the electrometer, falling on this paper made a photographic impression of each deflection of the needle with various numbers of cells in circuit. The time of exposure of the paper was two minutes.

After the sheets had been developed and fixed the deflections were measured with a tabulating instrument, and the following results obtained.

Experiment 4.

Positive.		Negative.	
No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
0	0·000	0	0·000
100	0·949	40	0·410
200	1·764	80	0·880
300	2·412	120	1·323
400	3·013	160	1·805
500	3·452		
600	3·806		
700	4·049		

Experiment 5.

Positive.		Negative.	
No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
0	0·000	0	0·000
100	0·918	40	0·410
200	1·725	100	1·135
300	2·406	160	1·701
400	3·003		
500	3·454		
600	3·784		
700	4·052		

After these experiments the quadrants were put to earth, and the instrument left until the following morning, when observations were again made of a similar nature.

It was however observed that the deflections noted were found generally to be somewhat larger than those of the previous day for the same number of cells joined up.

Experiment 6.

Positive.			
No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
100	0·937	500	3·662
200	1·837	600	4·125
300	2·625	700	4·337
400	3·125	800	4·525

In the next experiment the deflections were measured for every twenty cells added on from 200 negative to 800 positive.

Experiment 7.

Positive.						Negative.	
No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
0	0·000	320	2·475	600	3·925	0	0·000
40	0·525	340	2·600	620	4·000	20	0·100
80	0·650	360	2·750	640	4·075	40	0·275
100	(0·700)	380	2·875	660	4·125	60	0·500
120	0·900	400	3·000	680	4·175	80	0·725
140	1·075	420	3·075	700	4·225	100	0·950
160	1·250	440	3·175	720	4·275	120	1·175
180	1·425	460	3·300	740	4·325	140	1·425
200	1·600	480	3·400	760	4·350	160	1·675
220	1·775	500	3·525	780	4·375	180	1·925
240	1·950	520	3·625	800	4·400	200	2·175
260	2·100	540	3·700				
280	2·200	560	3·775				
300	2·350	580	3·850				

After this the photographic slides were again mounted in the place of the scale and the deflections registered, which on the sheets being measured gave results as follow:

Experiment 8.

Positive.				Negative.	
No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
100	1·058	500	3·942	100	1·108
200	1·943	600	4·358	200	2·323
300	2·686	700	4·625	300	3·663
400	3·488	800	4·810		

Experiment 9.

Positive.				Negative.	
No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
100	0·976	600	4·097	100	1·149
200	1·843	700	4·359	200	2·429
300	2·591	800	4·561	300	3·853
400	3·203	900	4·686		
500	3·708				

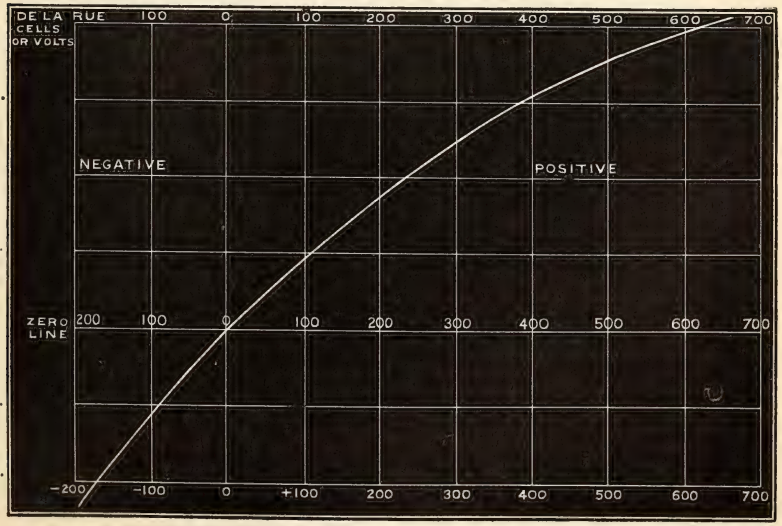
By combining the results of all the above experiments, and taking the means for every hundred cells, we have the following table:

Positive.				Negative.	
No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.	No. of Cells.	Deflection in inches.
100	0·93	600	3·95	100	1·04
200	1·77	700	4·20	200	2·34
300	2·48	800	4·42	300	3·75
400	3·09	900	4·69		
500	3·57				

On laying down these values in a curve, making use only of those between the limits of − 200 cells and + 700 cells, as the others are beyond the capability of correct registration by the electrograph, we find a regular smooth curve is produced, which being projected upon one of the ordinates gives a scale by means of which the electrograms are now easily tabulated.

The value of the electromotive force of one De La Rue chloride of silver cell being 1·03 volt, as determined by Messrs. De La Rue and Müller (Proc. Roy. Soc., vol. xxvi, p. 324), the scale thus formed has been assumed to represent volts with sufficient accuracy for the required purpose.

My best thanks are due to Mr. Seaton and Mr. R. W. F. Harrison for assistance rendered me in the prosecution of the experiments.



May 9, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

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

The following Papers were read:—

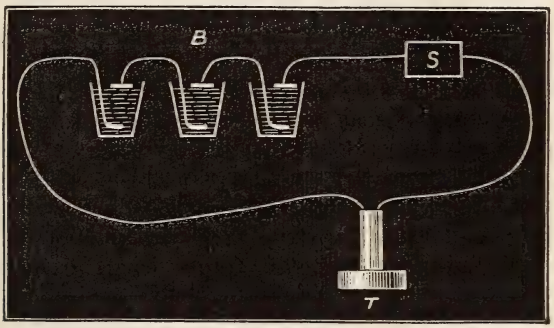
I. "On the Action of Sonorous Vibrations in varying the Force of an Electric Current." By Professor D. E. HUGHES. Communicated by Professor HUXLEY, Sec. R.S. Received May 8, 1878.

The introduction of the telephone has tended to develop our knowledge of acoustics with great rapidity. It offers to us an instrument of great delicacy for further research into the mysteries of acoustic phenomena. It detects the presence of currents of electricity that have hitherto only been suspected, and it shows variations in the strengths of currents which no other instrument has ever indicated.

It has led me to investigate the effect of sonorous vibrations upon the electrical behaviour of matter. Willoughby Smith has shown that the resistance of selenium is affected by light, and Börnstein has led us to believe that many other bodies are similarly affected. We know also that the resistance of all bodies is materially influenced by heat. Sir William Thomson and others have shown that the resistance to the passage of currents offered by wires is affected by their being placed under strains, and, inasmuch as the conveyance of sonorous vibrations induces rapid variations in the strains at different points of a wire, I believed that the wire would vary in its resistance when it was used to convey sound. To investigate this I made a rough-and-ready telephone, with a small bar magnet four inches long, half the coil of an ordinary electro-magnet, and a square piece of ferro-type iron, three inches square, clamped rigidly in front of one pole of the magnet between two pieces of board. When using the pendulum beats of a small French clock, or the voice, as a source of sound, I found this arrangement supplied me with an extremely delicate *phonoscope* or sound detector.

All the experiments detailed in this paper were made with the simplest possible means, and no apparatus of any kind constructed by a scientific instrument maker was employed. The battery was a simple Daniell's cell, of Minotto's form, made by using three common tumblers, a spiral piece of copper wire being placed at the bottom of each glass and covered with sulphate of copper, and the glass being

then filled with well-moistened clay and water. A piece of zinc as the positive element was placed upon the clay. Insulated wires were attached to each plate, and three of these cells were joined in series. All experiments were made on a closed circuit, the telephone being used as a phonoscope to detect variations in the current and the consequent reproduction of sound. The apparatus, or materials experimented upon, were used in the same way as the transmitter of the speaking telephone of Bell. The attached sketch will make this clear.



B is the battery, S the source of sound or material examined, T the telephone or phonoscope.

I introduced into the circuit at S a strained conductor—a stretched wire—listening attentively with the telephone to detect any change that might occur when the wire was spoken to or set into transverse vibrations by being plucked aside. Gradually, till the wire broke, the strain was varied, but no effect whatever was remarked except at the moment when the wire broke. The effect was but momentary, but invariably at the moment of breaking a peculiar "rush" or sound was heard. I then sought to imitate the condition of the wire at the moment of rupture by replacing the broken ends and pressing them together with a constant and varying force by the application of weights. It was found that if the broken ends rested upon one another with a slight pressure of not more than one ounce to the square inch on the joints, sounds were distinctly reproduced although the effects were very imperfect.

It was soon found that it was not at all necessary to join two wires endwise together to reproduce sound, but that any portion of an electric conductor would do so even when fastened to a board or to a table, and no matter how complicated the structure upon this board, or the materials used as a conductor, provided one or more portions of the electrical conductor were separated and only brought into contact by a slight but constant pressure. Thus, if the ends of the wire terminate in two common French nails laid side by side, and are separated

from each other by a slight space, were electrically connected by laying a similar nail between them, sound could be reproduced. The effect was improved by building up the nails log-hut fashion, into a square configuration, using ten or twenty nails. A piece of steel watch chain acted well. Up to this point the sound or grosser vibrations were alone produced, the finer inflections were missing, or, in other words, the *timbre* of the voice was wanting, but in the following experiments the *timbre* became more and more perfect until it reached a perfection leaving nothing to be desired. I found that a metallic powder such as the white powder—a mixture of zinc and tin—sold in commerce as "white bronze," and fine metallic filings, introduced at the points of contact, greatly added to the perfection of the result.

At this point articulate speech became clearly and distinctly reproduced, together with its *timbre*, and I found that all that now remained was to discover the best material and form to give to this arrangement its maximum effect. Although I tried all forms of pressure and modes of contact, a lever, a spring; pressure in a glass tube sealed up while under the influence of strain, so as to maintain the pressure constant, all gave similar and invariable results, but the results varied with the materials used. All metals, however, could be made to produce identical results, provided the division of the metal was small enough, and that the material used does not oxidize by contact with the air filtering through the mass. Thus platinum and mercury are very excellent and unvarying in their results, whilst lead soon becomes of such high resistance, through oxidation upon the surface, as to be of little or no use. A mass of bright round shot is peculiarly sensitive to sound whilst clean, but as the shot soon become coated with oxide this sensitiveness ceases. Carbon again, from its surface being entirely free from oxidation, is excellent, but the best results I have been able to obtain at present have been from mercury in a finely divided state. I took a comparatively porous non-conductor, such as the willow charcoal used by artists for sketching, heating it gradually to a white heat and then suddenly plunging it in mercury. The vacua in the pores, caused by the sudden cooling, become filled with innumerable minute globules of mercury, thus, as it were, holding the mercury in a fine state of division. I have also tried carbon treated in a similar manner with and without platinum deposited upon it from the chloride of platinum. I have also found similar effects from the willow charcoal heated in an iron vessel to a white heat, and containing a free portion of tin, zinc, or other easily vaporized metal. Under such conditions the willow carbon will be found to be metallized, having the metal distributed throughout its pores in a fine state of division. Iron also seems to enter the pores if heated to a white heat without being chemically combined with the carbon as in graphite, and, indeed, some of the best results have been

obtained from willow charcoal containing iron in a fine state of division.

Pine charcoal treated in this manner (although a non-conductor as a simple charcoal) has high conductive powers, due to the iron; and from the minute division of the iron in the pores, is a most excellent material for the purpose.

Any one of these preparations confined in a glass tube or a box, and provided with wires for insertion in a circuit, I call a "transmitter."

Reis, in 1860, showed how, by the movement of a diaphragm, intermittent voltaic currents could be transmitted, agreeing in exact number with the sonorous waves impinging on the diaphragm, and thus reproducing music at a distance by causing an electro-magnet to vibrate in unison with the diaphragm; and, with an iron diaphragm, Graham Bell showed how the vibrations of that diaphragm in front of a polarised electro-magnet could similarly induce magneto-currents, corresponding in number, amplitude, and form, with the sonorous vibration, and thus reproduce all the delicacies of the human voice. Edison and others have produced variations in the strengths of a constant current by causing the diaphragm to press directly upon some elastic conductor, such as carbon, spongy platinum, &c., the varying pressure upon these materials varying the resistance of the circuit, and consequently the strength of current flowing. Graham Bell and others have produced the same effect, by causing the vibrations of the diaphragm to vary the electromotive force in the circuit. It will be seen, however, that in the experiments made by myself, the diaphragm has been altogether discarded, resting as it does upon the changes produced by molecular action, and that the variations in the strengths of the currents flowing are produced simply and solely by the direct effect of the sonorous vibrations.

I have found that any sound, however feeble, produces vibrations which can be taken up by the matter interposed in the electrical circuit. Sounds absolutely inaudible to the human ear affect the resistance of the conductors described above. In practice, the effect is so sensitive, that a slight touch on the board, by the finger nail, on which the transmitter is placed, or a mere touch with the soft part of a feather, would be distinctly heard at the receiving station. The movement of the softest camel hair brush on any part of the board is distinctly audible. If held in the hand, several feet from a piano, the whole chords—the highest as well as the lowest—can be distinctly heard at a distance. If one person sings a song, the distant station, provided with a similar transmitter, can sing and speak at the same time, and the sounds will be received loud enough for the person singing to follow the second speech or song sent from the distant end.

Acting on these facts, I have also devised an instrument suitable for magnifying weak sounds, which I call a *microphone*. The microphone,

in its present form, consists simply of a lozenge-shaped piece of gas carbon, one inch long, quarter inch wide at its centre, and one-eighth of an inch in thickness. The lower pointed end rests as a pivot upon a small block of similar carbon; the upper end, being made round, plays free in a hole in a small carbon-block, similar to that at the lower end. The lozenge stands vertically upon its lower support. The whole of the gas carbon is tempered in mercury, in the way previously described, though this is not absolutely necessary. The form of the lozenge-shaped carbon is not of importance, provided the weight of this upright contact piece is only just sufficient to make a feeble contact by its own weight. Carbon is used in preference to any other material, as its surface does not oxidise. A platinum surface in a finely-divided state is equal, if not superior, to the mercurised carbon, but more difficult and costly to construct. I have also made very sensitive ones entirely of iron.

The best form and materials for this instrument, however, have not yet been fully experimented on. Still, in its present shape, it is capable of detecting very faint sounds made in its presence. If a pin, for instance, be laid upon or taken off a table, a distinct sound is emitted, or, if a fly be confined under a table-glass, we can hear the fly walking, with a peculiar tramp of its own. The beating of a pulse, the tick of a watch, the tramp of a fly, can thus be heard at least a hundred miles distant from the source of sound. In fact, when further developed by study, we may fairly look for it to do for us, with regard to faint sounds, what the microscope does with matter too small for human vision.

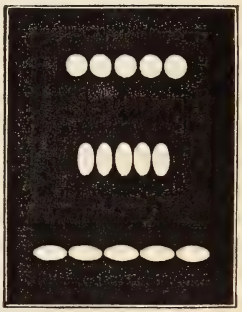
It is quite evident that these effects are due to a difference of pressure at the different points of contact, and that they are dependent for the perfection of action upon the number of these points of contact. Moreover, they are not dependent upon any apparent difference in the bodies in contact, but the same body in a state of minute subdivision is equally effective. Electrical resistance is a function of the mass of the conductor, but sonorous conduction is a function of the molecules of matter. How is it therefore that a sonorous wave can so affect the mass of a conductor as to influence its electrical resistance? If we assume a line of molecules, we know that a sonorous wave is accompanied by alternate compressions and rarefactions. If we isolate the part under compression from the part under dilatation we vary the dimensions of the mass, and we alter its electrical resistance. In any homogeneous conductor of finite dimensions the effect of the one will exactly compensate for the effect of the other, and we get no variation of current, but if we break up this homogeneous conductor into a series of minute subdivisions without actually breaking their electrical continuity we destroy this neutralizing influence, and we render evident the effect of sonorous vibrations in varying the dimensions of

the mass of the conductor, and therefore in varying its electrical resistance, for we reduce the length of a portion of the conductor to a fraction of the length of a sonorous wave. Molecular action alone explains to me all the effects produced. Size or shape does not affect them. A piece of willow charcoal, the size of a pin's head, is quite sufficient to reproduce articulate speech. I regard the action as follows:—If we have two separate conductors joined simply by contact this contact offers a certain resistance. Now we can vary or lessen the resistance by increasing the pressure, thus bringing more points in contact or closer proximity. Now, as I employ a constant pressure on the contact, which is exactly under the same influence of the vibrations as the points of contact, more points or closer proximity can only be obtained through the molecular swelling or movement of the contact points.

If we assume a line of molecules at the point of contact of the minute masses of conducting matter in their neutral condition to be arranged thus:—

they will appear thus under compression:—

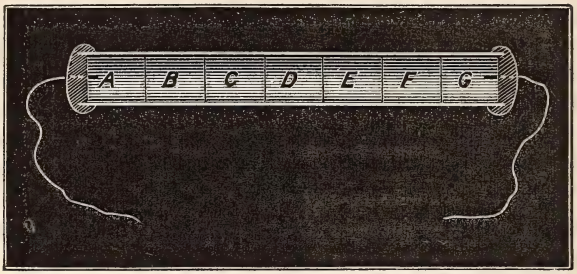
and thus under dilatation:—



In the former case the electrical resistance would be *less*, and in the latter case *more* than in the normal condition. Hence we should get variation in their electrical resistance, and thus sonorous waves could vary the strength of an electric current, and the variations of the electric current can be made to reproduce sonorous vibrations. These, however, would only produce the result in a certain line, say horizontal; but those perpendicular, while producing the same result, would be a half vibration behind, and thus if two contacts, the one horizontal and the other perpendicular, were on the same piece of charcoal and the conducting line joined to both, we should have interference. The contrary takes place as the more contacts we have, and the more varied their direction on the same, the louder and purer the sound becomes. Hence there is no interference, and consequently the whole mass must swell and diminish equally in all directions at the same instant of time.

The tube transmitter, which I exhibit this evening, consists of an exterior glass tube two inches long and one quarter of an inch in

diameter. In it are four separate pieces of willow charcoal, each one quarter of an inch long, and two terminals of the same material. The terminals are fastened in the tube, and connect exteriorly with the line and interiorly with the four loose pieces, thus:—



Here A is made to press on B, C, D, E, F, and G, until the resistance offered to the electrical current is about one-third that of the line upon which it is to be employed. It may be attached to a resonant board by the ends A or G. If the result was simply due to vibrations, we should have A and B making greater contact at a different time from F and G, and consequent interference. If it was a simple shaking or moving of B, C, D, E, and F, it could produce no change, as if B pressed more strongly on C, it would be less on A, and also if the tube was attached by the centre we should have no effect; but if the effect is due to a swelling or enlargement of B, C, D, E, F, it would make no difference where it is attached to the resonant board, as is actually the case. Again reduce the pressure of A upon B, &c., until they are not in contact, and no trace of current can be perceived by shaking the tube. The instant the sonorous vibrations pass in the tube there is electric contact to a remarkable degree, which could only have taken place by the molecules enlarging their sphere under the influence of the sonorous vibrations.

It is impossible to say what can be the applications or the effects of the discovery which I have had the honour of bringing before the Royal Society, for the whole question has been studied with crude materials, and scarcely sufficient time has elapsed to enable me to consider its ultimate uses. I do not desire to assert that there is anything in what I have brought forward that is superior to or equal to other transmitters used for telephony. It is as loud and far more sensitive than any I have yet heard, and it may be increased by multiplication of transmitting contacts in quantity or intensity; the loudness is at present limited by the capability of the receiver. The materials at my disposal, and the arrangement of them, have not yet been sufficiently studied. I only wished to show that it is possible to transmit clear and intelligent articulate speech, and to render audible

sounds which have hitherto been inaudible by the mere operation of sonorous vibrations upon the conducting power of matter.

My warmest thanks are due to Mr. W. H. Preece, electrician to the Post Office, for his appreciation of the importance of the facts I have stated, and for his kind counsel and aid in the preparation of this paper.

I do not intend to take out a patent, as the facts I have mentioned belong more to the domain of discovery than invention. No doubt inventors will ere long improve on the form and materials employed. I have already my reward in being allowed to submit my researches to the Royal Society.

II. "Note on the Minute Anatomy of the Thymus." By Herbert Watney, M.A., M.D. Cantab. Communicated by Dr. Klein, F.R.S. Received April 8, 1878.

The thymus is composed of lobes, lobules, and follicles.

Each follicle consists of a cortical and a medullary portion; the medullary parts of two neighbouring follicles are often united; and at one point, therefore, the medullary portion may extend through the cortex of the follicle; in some follicles the medullary portion may be found in the form of two or more islands situated in the interior of the follicle.

The follicle is composed (*a*) of a reticulum of nucleated cells, and (*b*) of cells; the reticulum forms an adventitia to the blood-vessels.

The cells forming the reticulum in the cortical part of the follicle consist of a disk-shaped nucleus, a cell body very little larger than the nucleus, and of very long, fine, branching processes.

The reticulum of the medullary portion is composed of cells with coarse, short processes; the body of the cell is more than twice, or even three times, as large as the nucleus, and contains one, or at times, two nuclei; in places, large protoplasmic masses are met with, forming part of the reticulum composed of two or three cells united together. There are also found in the medullary portions, in certain states of the thymus, connective tissue trabeculæ.

The cells are of four kinds:—

(1.) Small cells, resembling the lymph cells of a follicle of a lymphatic gland. Staining fluids act differently on these cells in the cortical and in the medullary parts of the follicle.

(2.) Large granular cells of various sizes; many of them have long processes by which, in some cases, they are attached to the trabeculæ and to the blood-vessels: these cells contain one or two nuclei, and help to form (partly by a process of vacuolation) the concentric corpuscles of the thymus.

(3.) Giant cells. Multinuclear masses of protoplasm.

(4.) Concentric corpuscles of various sizes, sometimes attached to one another by long processes. The corpuscles are formed of two parts—a central portion, which is granular, and is acted on in a peculiar manner by staining fluids; and a peripheral portion formed of flattened epitheloid cells continuous with the reticulum. The concentric corpuscles are concerned in the formation of blood-vessels and trabeculæ.

The granular cells, giant cells, and concentric corpuscles, are almost entirely confined to the central portions of the follicles.

In fresh preparations, colourless nucleated cells are seen, which contain granules and spherules of hæmoglobin; these cells either form parts of the concentric corpuscles, or are in close connexion with them.

The blood-vessels of the cortical portions of the follicle are of small size; they run in lines from the periphery of the follicle to the edge of the medullary portion. The medullary portion of the follicle is surrounded by a ring of blood-vessels; the vessels are larger in the medullary portion than in the cortical portion of the follicle.

III. "On the Classification of Loci." By W. K. Clifford, F.R.S., Professor of Applied Mathematics in University College, London. Received April 8, 1878.

(Abstract.)

"A curve," is to be understood to mean a continuous one-dimensional aggregate of any sort of elements, and therefore not merely a curve in the ordinary geometrical sense, but also a singly infinite system of curves, surfaces, complexes, &c., such that one condition is sufficient to determine a finite number of them. The elements may be regarded as determined by k co-ordinates; and if these be connected by $k-1$ equations of any order, the curve is either the aggregate of common solutions, or, when this breaks up into algebraically distinct parts, the curve is one of these parts.

In the paper, of which this is an abstract, theorems are established relating to the nature of the space in which such curves can exist, to the mode of representing them in flat space of lower dimensions, and to some of their properties. The following are the leading theorems:—

I. Every proper curve of the nth order is in a flat space of n dimensions or less.

II. A curve of order n in flat space of k dimensions (or less) may be represented, point for point, on a curve of order $n - k + 2$ in a plane.

III. A curve of order n, in flat space of n dimensions (and no less), is always unicursal.

From this the author obtains a representation of the points of an n dimensional space by means of groups of n points on such a unicursal curve, corresponding to the methods of Hirst and Darboux for 3-dimensional space.

When n is even, the system corresponds to that of poles and polars in regard to a quadric locus upon which the curve lies.

When n is odd, every point is co-flat (*i.e.*, $n + 1$ points lie in the same $n - 1$ flat), with the n points of the osculant $(n - 1)$ flats which can be drawn through it.

IV. Every curve of order n in flat space of $n-1$ dimensions is either unicursal or elliptic.

V. When the curve is unicursal, and n is odd, the n points of super-osculation, or points of stationary osculant $(n - 2)$ flats, are on the same $(n - 2)$ flat. But when n is even, this will be the case only under a certain condition.

VI. When the curve is elliptic (or bi-cursal) the class of the curve is $n\ (n - 1)$, and the number of superosculants n^2.

If we consider a curve of the order n and deficiency p, existing in k dimensions, a $(k - 1)$ flat cuts such a curve in n points, such that the sum of each of the p parameters (Abel's theorem gives p equations between the parameters of n points which lie on a $(k - 1)$ flat), for these n points is zero. And we obtain the theorem

VII. A curve of order n and deficiency p, not greater than $\frac{1}{2}\ n$, can at most exist in $n - p$ dimensions.

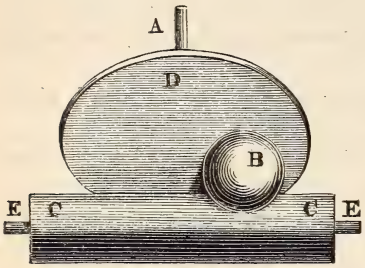
IV. "Harmonic Analyzer." Shown and explained by Sir William Thomson, F.R.S., Professor of Natural Philosophy in the University of Glasgow. Received May 9, 1878.

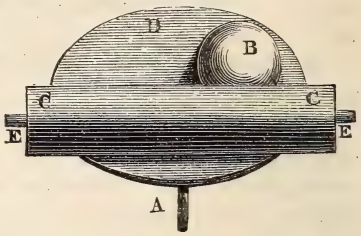
This is a realization of an instrument designed rudimentarily in the author's communication to the Royal Society ("Proceedings," February 3rd, 1876), entitled "On an Instrument for Calculating $(\int \phi(x)\psi(x)dx)$, the Integral of the Product of two given Functions."

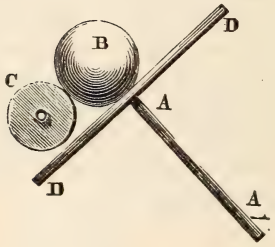
It consists of five disk globe and cylinder integrators of the kind described in Professor James Thomson's paper "On an Integrating Machine having a new Kinematic Principle," of the same date, and represented in the annexed woodcuts.

The five disks are all in one plane, and their centres in one line. The axes of the cylinders are all in a line parallel to it. The diameters of the five cylinders are all equal, so are those of the globes; hence the centres of the globes are in a line parallel to the line of the centres of the disks, and to the line of the axes of the cylinders.

One long wooden rod, properly supported and guided, and worked by a rack and pinion, carries five forks to move the five globes and a pointer to trace the curve on the paper cylinder. The shaft of the paper cylinder carries at its two ends cranks at right angles to one another; and a toothed wheel which turns a parallel shaft, and a third shaft in line with the first, by means of three other toothed wheels. This third shaft carries at its two ends two cranks at right angles to one another.







Another toothed wheel on the shaft of the paper drum turns another parallel shaft, which, by a slightly oblique toothed wheel working on a crown wheel with slightly oblique teeth, turns one of the five disks uniformly (supposing to avoid circumlocution the paper drum to be turning uniformly). The cylinder of the integrator, of which this one is the disk, gives the continuously growing value of $\int y dx$.

Each of the four cranks gives a simple harmonic angular motion to one of the other four disks by means of a slide and crosshead, carrying a rack which works a sector attached to the disk. Hence, the cylinders moved by the disks, driven by the first mentioned pair of cranks, give the continuously growing values of

$$\int y \cos\frac{2\pi x}{c} dx, \text{ and } \int y \sin\frac{2\pi x}{c} dx;$$

where c denotes the circumference of the paper drum: and the two remaining cylinders give

$$\int y \cos\frac{2\pi\omega x}{c}dx, \text{ and } \int y \sin\frac{2\pi\omega x}{c}dx;$$

where ω denotes the angular velocity of the shaft carrying the second pair of shafts, that of the first being unity.

The machine, with the toothed wheels actually mounted on it when shown to the Royal Society, gave $\omega=2$, and was therefore adopted for the meteorological application. By removal of two of the wheels and substitution of two others, which were laid on the table of the Royal Society, the value of ω becomes $\frac{39\times109}{40\times110}$* (according to factors found by Mr. E. Roberts, and supplied by him to the author, for the ratio of the mean lunar to the mean solar periods relatively to the earth's rotation). Thus, the same machine can serve for analysing out simultaneously the mean lunar and mean solar semi-diurnal tides from a tide-gauge curve. But the dimensions of the actual machine do not allow range enough of motion for the majority of tide-gauge curves, and they are perfectly sufficient and suitable for meterological work. The machine, with the train giving $\omega=2$, is therefore handed over to the Meteorological Office to be brought immediately into practical work by Mr. Scott (as soon as a brass cylinder of proper diameter to suit the $24h$ length of his curves is substituted for the wooden model cylinder in the machine as shown to the Royal Society): and the construction of a new machine for the tidal analysis, to have eleven disk globe and cylinder integrators in line, and nine crank shafts having their axes in line with the paper drum, according to the preceding description, in proper periods to analyse a tide curve by one process for mean level, and for the two components of each of the five chief tidal constituents—that is to say,

(1.) The mean solar semi-diurnal;
(2.) „ „ lunar „
(3.) „ „ lunar quarter diurnal, shallow water tide;
(4.) „ „ lunar declinational diurnal;
(5.) „ „ luni-solar declinational diurnal;

is to be immediately commenced. It is hoped that it may be completed without need to apply for any addition to the grant already made by the Royal Society for harmonic analysers.

Counterpoises are applied to the crank shafts to fulfil the condition that gravity on cranks, and sliding pieces, and sectors, is in equilibrium. Error from "back lash" or "lost time" is thus prevented simply by frictional resistance against the rotation of the uniformly rotating disk and of the tertiary shafts, and by the weights of the sectors attached to the oscillating disks.

* The actual numbers of the teeth in the two pairs of wheels constituting the train are 78 : 80 and 109 : 110.

May 16, 1878.

Sir JOSEPH HOOKER, K.C.S.I., in the Chair.

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

The following Papers were read:—

I. "Experimental Researches on the Electric Discharge with the Chloride of Silver Battery. Part II. The Discharge in Exhausted Tubes." By WARREN DE LA RUE, M.A., D.C.L., F.R.S., and HUGO W. MULLER, Ph.D., F.R.S. Received April 10, 1878.

(Abstract.)

We cannot flatter ourselves that we have done more during our three and a half years' work than contribute a few facts towards the data necessary for the solution of the problem of the cause of stratification produced by the electric discharge in vacuum tubes. We refrain for the present from suggesting any hypothesis to account for this beautiful phenomenon, in the hope of being able to confirm, experimentally, certain views which we entertain concerning it. The paper we have now the honour of laying before the Society consists mainly of a record of the various appearances presented by the discharge in residual gases at various pressures and with various currents.

Throughout our labours we have felt strongly the necessity for obtaining measurements of the physical conditions (nature and pressure of the gas, strength of the current, difference of potentials between the electrodes, size of the tubes, forms of electrodes) which accompany the various phenomena observed during our experiments.

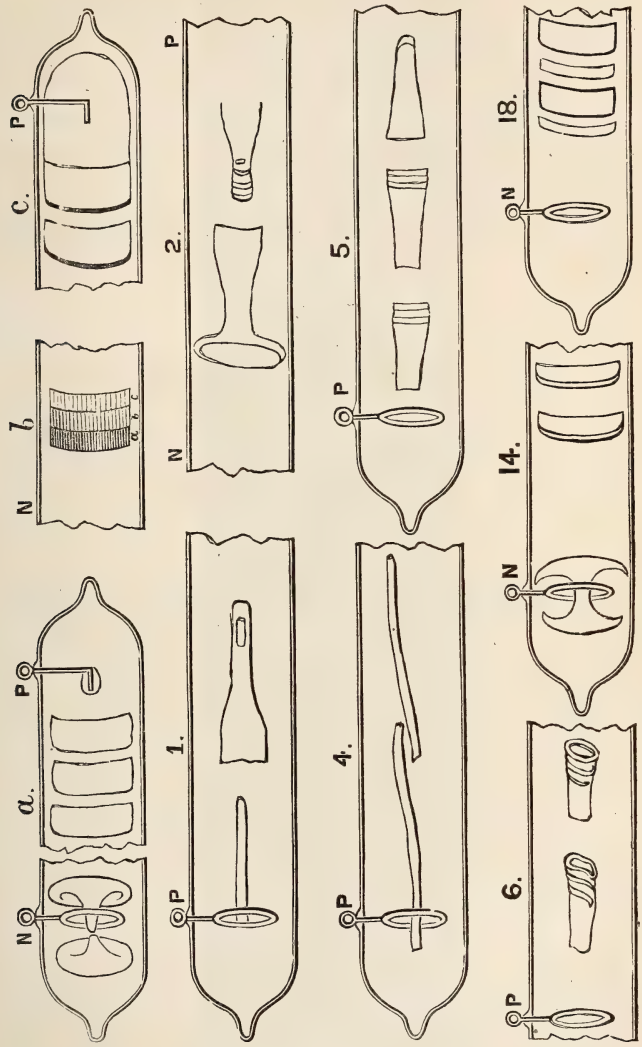
In order that others may be in a position to theorize as well as ourselves, we have given a more detailed account than we otherwise should have done of the phenomena observed, which, for the same reason, we have illustrated by copious diagrams and many copies of photographs.

For example, in diagram, fig. 43 are shown some of the very great variety of phases in rarefied hydrogen, observed with tube 129, 32 inches long, 1·6 inch diameter; the terminals being a straight wire and a ring about 1 inch in diameter, both aluminium. We quote a few observations by way of illustration:—

Pressure 16 millims., 21,053 **M** (millionths of an atmosphere), 8,040 cells. The ring being positive, curiously formed luminous entities shot at intervals from it, remained stationary for a time, and

then disappeared, to be replaced by others; the flow, as seen in a rotating mirror, was towards the negative. The phenomena are depicted in 1, fig. 1.

Fig. 1.



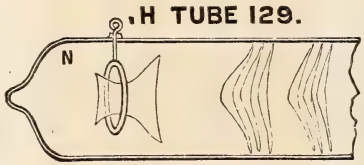
Pressure 12·2 millims., 16,053 **M**, 8,040 cells. The luminosities as in 5, fig. 1. The flow of the luminosities was shown by the rotating mirror to be towards the negative. There was much heat developed

in the neighbourhood of the luminosities, a little heat at the positive terminal, the negative being quite cool.

Pressure 10·8 millims., 14,211 **M**. Luminosities as in 6, fig. 1, which reminded one of a fish's mouth, especially as they opened and closed continually; they extended along 9 inches of the tube.

At a pressure of 1 millim., 1,316 **M**, the most beautiful phase of all was produced as shown in fig. 2, in some of its chief features, the current used was that of 2,160 powder cells, current 0·011520 Weber. The strata grouped themselves in threes and reached to within 6 inches of the negative ring; when 200,000 ohms resistance was introduced, C. 0·005658 **W**, the dark space extended to 9 inches. Only a portion of the luminosity about the negative ring is shown in the diagram; besides this, the ring was surrounded with a cylindrical nebulosity of about a $\frac{1}{4}$ inch in diameter, and a glow filled up the end of the tube.

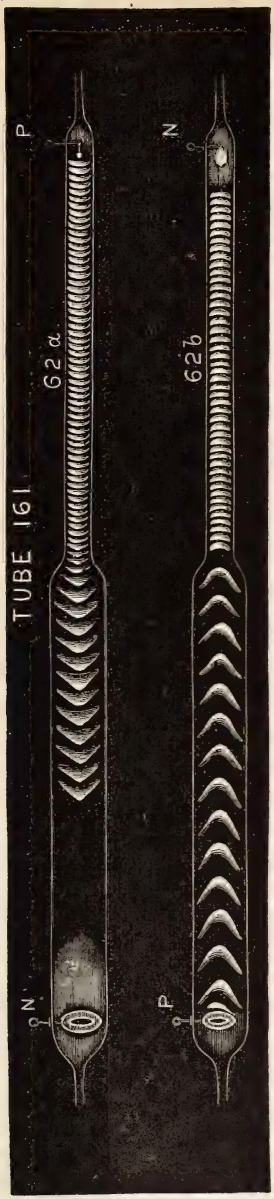
Fig. 2.



Tube 161, *Hydrogen.*

The difference of the strata in tubes of different diameter at the same pressure and with the same current is very clearly brought out in tube 161, fig. 3, composed of two portions, one being 18 inches long and 1·65 inch internal diameter, the other 17·5 inches long and 0·975 inch diameter, the ratio of the sectional areas being 2·864 to 1. The terminal in the small tube is a point, in the large one a ring. With 4,800 cells, the point (small tube) positive, C. 0·02825 **W**, there were produced in the small tube 62 disk-shaped strata, and in the large tube twelve cup-shaped strata occupying half of the length of the large tube; beyond these the discharge was dark. With the point negative C. 0·02451, there were produced in the small tube 54 disks, and in the large tube thirteen cup-shaped, completely filling it. The number of strata does not therefore appear to be in the inverse ratio of the areas. The strata in the small tube were blue, but at times, with a larger current, carmine, as in the capillary part of a spectrum analysis tube, the strata in the large tube being much fainter and pink. 62*a* and 62*b*, fig. 3, copied from photographs obtained, the former in 15 seconds, the latter in 10 seconds, show respectively the appearances at another phase, when the small tube was positive or negative respectively.

Fig 3.



We have found it necessary to exhaust the tubes ourselves, in order to be certain of the conditions as to pressure and purity of the gas (which we examined spectroscopically at various degrees of exhaus-

tion). By operating in this way we have been able to reproduce over and over again phases which are more or less transient in all tubes; indeed, we encountered great difficulties when we first commenced our labours with tubes supplied by various makers, for after a very short time they completely and permanently changed so as no longer to present the splendid stratifications witnessed on a first trial.

All the tubes which we exhausted were provided with two glass stop-cocks, one of which was connected with the gas generator, the other to the system of pumps. The rinsing of the apparatus was first effected many times with a high pressure water *trompe* (head of water 106 feet, producing a vacuum of 12 millims.); the tube was then exhausted by a mercurial pump (Alvergniat's make), and finally by the Sprengel. In our later experiments the measurements of low pressures were made by means of the M'Leod gauge, which indicated pressures down to 0·00005 millim.

By absorbing carbonic acid with potash, and, more especially, hydrogen with spongy palladium, a vacuum was obtained so perfect (pressure 0·000055 millim., 0·066 **M.**) that the potential of 11,000 was insufficient to cause a discharge.

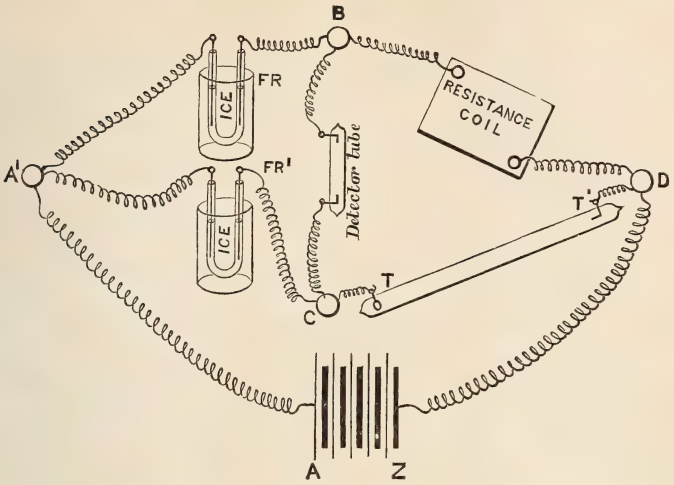
We have adopted several expedients for ascertaining the resistance of the tubes. In the first instance we placed the tube in one of the arms of a Wheatstone bridge, but the oscillations of the current were so great that it was necessary to substitute a vacuum tell-tale tube for the galvanometer; this became illuminated whenever the currents in the two branches were unequal. In spite of this device it was impossible to balance *exactly* the resistance of a tube by a metallic resistance. It was found that the resistance of a vacuum tube varied greatly as soon as the current passed and usually diminished, but when once the current was interrupted it was necessary to increase considerably the balancing resistance in the symmetrical branch of the Wheatstone bridge, in order to reproduce an illumination. After standing at rest for some time the tube took up its original condition.

In fig. 4 A Z is the battery, the A terminal of which is connected at A′, in the bridge arrangement, with two equal fluid resistance tubes, FR and FR′, of 420,000 ohms, placed in vessels containing ice, to keep them at a constant temperature; an adjustable coil resistance is inserted between B and D; the tube T T′, to be tested, is placed between D and C, the Z terminal of the battery being connected to D. When the resistance is greater or less than that of the tube to be tested there is an illumination in the detector tube between B and C; but when a current passes in T T′, balanced by a proper adjustment of the coil resistance, then the glow in the detector ceases.

Better results were obtained either by reproducing the deflections of a galvanometer, observed during the course of the experiments, with metallic resistance substituted for the tube; or by measuring with an

electrometer, the differences of potential between the electrodes of the tube, and between the ends of any convenient known resistance in the same circuit.

Fig. 4.



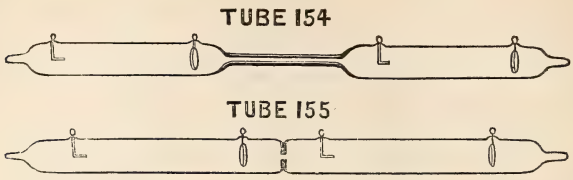
We were induced to extend the investigation by means of the electrometer, to the determination of the rate of fall of the potential throughout the length of a vacuum tube. For this purpose we had several tubes constructed with aluminium rings fixed to platinum wires inserted in the glass at nearly equal intervals. It was found that the fall of potential for equal spaces had a maximum value in the neighbourhood of the negative electrode; in the neighbourhood of the positive, though considerably less than near the negative, it was greater than in the middle of the tube, where for equal intervals the fall of potential was sensibly constant.

The most important result of these measurements was the establishment of the *constancy* of the difference of potential between the electrodes of a given tube and gas, at a given pressure, for all variations in the value of the current, even when these were as great as 1 to 150. This conclusion was arrived at subsequently to the observation of the majority of the phenomena recorded in the paper. Accordingly, it must be understood that what has been termed "resistance of a tube" indicates that the difference of potential between its terminals was the same as that which would have existed between the ends of a metallic wire of the resistance given if substituted for the tube in the particular circuit.

We have, moreover, by experimenting with such tubes as 154 and 155, fig. 5, wherein there is a constricture of the same bore, but of different lengths, established the fact that the constricture greatly increases the

resistance of the tube, but that this increase depends more upon the smallness of the diameter of the constricture than upon its length.

Fig. 5.



In the course of our experiments we have arrived at the following facts:—

1. *The discharge in a vacuum tube does not differ essentially from that in air and other gases at ordinary atmospheric pressures; it cannot be considered as a current in the ordinary acceptation of the term, but must be of the nature of a disruptive discharge, the molecules of the gas acting as carriers of electrification. The gases in all probability receive two impulses in opposite directions, that from the negative being the more continuous of the two.* Metal is frequently carried from the terminals and is deposited on the inside of the tube, so as to leave a permanent record of the spaces between the strata.*

2. *As the exhaustion proceeds the potential necessary to cause a current to pass diminishes up to a certain point, whence it again increases, and the strata thicken and diminish in number, until a point is reached at which, notwithstanding the high electromotive force available, no discharge through the residual gas can be detected.*

3. *All strata have their origin at the positive pole.* Thus, in a given tube, with a certain gas, there is produced at a certain pressure, in the first instance, only one luminosity which forms on the positive terminal, then, as the exhaustion is gradually carried further it detaches itself, moving towards the negative, and being followed by other luminosities, which gradually increase in number up to a certain point.

4. *With the same potential the phenomena vary irregularly with the amount of current.* Sometimes, as the current is increased, the number of strata in certain tubes increases, and as it is diminished their number decreases; but with other tubes the number of strata frequently increases with a diminution of current.

5. *A change of current frequently produces an entire change in the colour of the strata.* For example, in a hydrogen tube from a cobalt blue to a pink. It also changes the spectrum of the strata; moreover, the spectra of the illuminated terminals and the strata differ.

6. *If the discharge is irregular and the strata indistinct, an alteration*

* "Phil. Trans.," Part I, vol. clxix, pp. 90 and 118.

of the amount of current makes the strata distinct and steady. Most frequently a point of steadiness, sufficient for photographing, is produced by the careful introduction of external resistance; subsequently the introduction of more resistance produces a new phase of unsteadiness, and still more resistance another phase of steady and distinct stratification.

7. *The greatest heat is in the vicinity of the strata.* This can be best observed when the tube contains either only one stratum, or a small number separated by a broad interval. There is reason to believe that even in the dark discharge there may be strata; for we have found a development of heat in the middle of a tube, in which there was no illumination except on the terminals.

8. *Even when the strata are to all appearance perfectly steady, a pulsation can be detected in the current; but it is not proved that the strata depend upon intermittence.*

9. *There is no current from a battery through a tube divided by a glass division into two chambers, and the tube can only be illuminated by alternating charges.*

10. *In the same tube and with the same gas, a very great variety of phenomena can be produced by varying the pressure and the current. The luminosities and strata, in their various forms, can be reproduced in the same tube, or in others having similar dimensions.*

11. *At the same pressure and with the same current, the diameter of the tube affects the character and closeness of the stratification.*

II. "Note on Legendre's Coefficients." By I. Todhunter, F.R.S. Received April 16, 1878.

In the "Proceedings of the Royal Society," vol. xxvii, pp. 63–71, Professor Adams has given, by an inductive process, the development of the product of any two of Legendre's Coefficients in a series of the Coefficients; from this is immediately deduced the value of the integral between the limits -1 and $+1$ of the product of any three of the Coefficients. On the other hand, if we know the value of this definite integral, we can immediately deduce the development of the product of any two of the Coefficients. Thus it may be of interest to give a brief investigation of the value of the definite integral. I follow the notation adopted by Professor Adams.

The formula to be established is

$$\int_{-1}^{1} P_m P_n P_p d\mu = \frac{2}{2s+1} \frac{A(s-m)A(s-n)A(s-p)}{A(s)},$$

where $2s=m+n+p$, and the functional symbol $A(r)$ is thus defined: if r is a positive integer

$$A(r)=\frac{1\,.\,3\,.\,5\,..\,(2r-1)}{1\,.\,2\,.\,3\,..\quad r},$$

and in all other cases $A(r)$ is to be considered zero, except when $r=0$, and then it is to be considered $=1$.

It will be observed that $A(r+1)=\frac{2r+1}{r+1}A(r)$, and this is the essential property of the function for our purpose. The demonstration consists of three parts.

I. The proposed formula holds when $m=0$, whatever n and p may be. This is seen to be true in virtue of the known theorem that $\int_{-1}^{1} P_n P_p d\mu$ is zero when n and p are unequal, and is equal to $\frac{2}{2n+1}$ when $p=n$.

II. The proposed formula holds when $m=1$, whatever n and p may be. For by a known theorem we have

$$P_1 P_n=\frac{n+1}{2n+1}P_{n+1}+\frac{n}{2n+1}P_{n-1},$$

therefore

$$\int_{-1}^{1} P_1 P_n P_p d\mu=\int_{-1}^{1}\left(\frac{n+1}{2n+1}P_{n+1}+\frac{n}{2n+1}P_{n-1}\right)P_p d\mu;$$

and this vanishes unless $p=n+1$ or $n-1$, and then its value is $\frac{2s}{(2s+1)(2s-1)}$, where $2s=n+p+1$. And if we examine the formula to be established we shall find if $m=1$ the product $A(s-n)A(s-p)$ vanishes, except when $p=n+1$ or $n-1$, and then it becomes unity. Also $\frac{A(s-1)}{A(s)}=\frac{s}{2s-1}$; and thus the required result is obtained.

III. We shall now give an inductive process by which we show that if the formula holds for all values of n and p combined with the values $m-1$ and m of the third integer, it will hold for all values of n and p combined with the value $m+1$ of the third integer.

For
$$P_{m+1}P_n P_p=\left\{\frac{2m+1}{m+1}P_m P_1-\frac{m}{m+1}P_{m-1}\right\}P_n P_p$$

$$=\frac{2m+1}{m+1}P_m P_p\left\{\frac{n+1}{2n+1}P_{n+1}+\frac{n}{2n+1}P_{n-1}\right\}-\frac{m}{m+1}P_{m-1}P_n P_p.$$

Thus we can express $\int_{-1}^{1} P_{m+1}P_n P_p d\mu$ by means of cases of the formula already established. If we now put $2s=m+n+p+1$ we thus obtain for the definite integral $\int_{-1}^{1} P_{m+1}P_n P_p d\mu$

$$\frac{2}{2s+1}\frac{2m+1}{m+1}\frac{n+1}{2n+1}\frac{A(s-m)\,A(s-n-1)\,A(s-p)}{A(s)}$$
$$+\frac{2}{2s-1}\frac{2m+1}{m+1}\frac{n}{2n+1}\frac{A(s-1-m)\,A(s-n)\,A(s-1-p)}{A(s-1)}$$
$$-\frac{2}{2s-1}\frac{m}{m+1}\frac{A(s-m)\,A(s-n-1)\,A(s-1-p)}{A(s-1)}.$$

It is now a matter of easy algebraical work to put these three terms together by the aid of the essential property of the function $A(r)$. If we put the second and third terms together we shall get

$$\frac{2}{(m+1)(2n+1)}\frac{(m-n)(s-p)}{(s-m)(2s-2n-1)}\frac{A(s-m-1)\,A(s-n)\,A(s-p)}{A(s)},$$

and the first term is equal to

$$\frac{2}{2s+1}\cdot\frac{(2m+1)(n+1)(2s-2m-1)(s-n)}{(m+1)(2n+1)(s-m)(2s-2n-1)}\frac{A(s-m-1)A(s-n)A(s-p)}{A(s)};$$

the aggregate of the last two expressions is

$$\frac{2}{2s+1}\frac{A(s-m-1)\,A(s-n)\,A(s-p)}{A(s)};$$

as it should be.

I may observe that the definite integral was discussed by Dr. N. C. Schmit in a dissertation published at Brussels in 1858; but no simple result is there obtained.

III. "On the Spectra of Metalloids. Spectrum of Oxygen." By Arthur Schuster, Ph.D., F.R.A.S. Communicated by J. Clerk Maxwell, F.R.S., Professor of Experimental Physics in the University of Cambridge. Received April 25, 1878.

(Abstract.)

The many unexplained phenomena attending the passage of electricity through gases will probably for some time to come occupy the attention of experimental physicists. It is desirable that the subject should be approached from as many different sides as possible. One of our most powerful instruments of research is the spectroscope, but before it can be applied to the study in question we have to settle the chemical origin of the different spectra, which we observe in vacuum tubes, and to discuss in what way such spectra are liable to change under different circumstances. A special investigation has to be made

for each gas; we have to study the effect of various impurities, the influence of the electrodes, and that of the glass which in the tubes generally used is considerably heated up by the spark. I have chosen oxygen as a first subject of investigation. Though Plücker and Wüllner have, as far as their experiments went, accurately described the phenomena seen in oxygen tubes, the following contains much that is new, and will put some of the older facts on a firmer basis.

As some of the facts brought to light by the investigation bear directly on the question of double spectra, our knowledge on that point must be briefly referred to. We divide all known spectra into three orders. Continuous spectra, channelled space spectra, and line spectra. With regard to continuous spectra, it is shown that the older statement which limited them to liquid and solid bodies is no longer tenable. Most gases give continuous spectra long before they condense. Two theories of continuous spectra are noticed. The one considers that the vibrations of a molecule always tend to take place in a fixed period, but that the impacts of other molecules may, when the pressure is great or in liquid and solid bodies, prevent complete oscillations taking place, and thus produce a continuous spectrum. The other theory considers that, when a gas condenses, molecular combinations take place, which make the molecular structure more complicated, and may produce channelled space spectra or continuous spectra. According to the latter theory such molecular combinations are possible before the gas condenses, and thus the state of aggregation of the gas only indirectly affects the spectrum. The latter theory seems to be more consistent with experiment than the former one. For instance, it is shown that oxygen gives a continuous spectrum at the lowest temperature at which it is luminous. If the temperature be raised, the continuous spectrum is replaced by a line spectrum. This seems to be inexplicable by theory of molecular impacts.

With regard to channelled space spectra attention is drawn to a new kind, which finds a representative in the spectrum of the negative pole in oxygen. The ordinary channelled space spectra show bands, which when seen under small dispersion appear to have a sharp boundary on one side, and fade gradually away towards the other. When seen under large dispersion these bands appear to be made up of a number of lines which get nearer and nearer to each other as they approach the sharp boundary. The spectrum of the negative pole in oxygen, when looked at with small powers, seems to be made up of a number of bands which are uniformly illuminated throughout, but with high powers each band is found to be made up of a number of lines at about equal intervals.

With regard to line spectra it is shown that a body may at a low temperature show a different set of lines altogether from what it shows at a high temperature. It is difficult to decide whether the

change from one line spectrum to another is due to the same cause as that of a band spectrum to a line spectrum.

The chief difficulty in the way of a complete investigation of the spectrum of oxygen consists in the great disturbing influence of the presence of even a small quantity of any carbon compound. Amongst a great many oxygen tubes which were filled by various makers, I only found one which showed the spectrum of pure oxygen; all the others gave a spectrum of carbonic oxide. It is therefore necessary in filling oxygen tubes, to avoid all greased joints and all india-rubber tubings. I have used a Sprengel air-pump, which communicated with the vacuum tube by means of a ball and socket joint. The joint was kept air-tight solely by means of strong sulphuric acid. The vacuum tube was fused directly to the ball of the joint. To one end of the vacuum tube a piece of hard glass tubing had been fused. This was filled with different substances which, on heating, gave off pure oxygen. The oxygen therefore came only into contact with glass, mercury, and sulphuric acid, and the metal of the electrode. By repeatedly filling and exhausting the tube all extraneous matter, which always is attached to the inner surface of the glass, could be swept away. Permanganate of potash, oxide of mercury, and chlorate of potash, were used in turn to prepare the oxygen, but no effect was observed which could be traced to the substance used. The effect of the electrodes was eliminated by varying the metals. Aluminium, platinum, silver, brass, and iridium were used as electrodes. Any possible effect of the glass was eliminated by finally repeating all experiments in a glass receiver six inches in diameter, so that no part of the spark came nearer than $2\frac{1}{2}$ inches to the glass. In this way it is believed all possibility of error due to the presence of any possible impurities was avoided.

All measurements were reduced to wave-lengths by interpolation. The lines of iron or any metal whose lines are reversed in the solar spectrum were taken as reference lines. The wave-length of the reference lines could therefore always be found on Ångström's solar map. As a rule the dispersion was equal to that of four heavy flint-glass prisms of a refracting angle of 62°, and it is believed that the accuracy of the measurements equals that of any other spectroscopic measurements.

Four different spectra of oxygen must be distinguished. At the lowest temperature at which oxygen becomes luminous it gives a continuous spectrum. As the temperature is gradually raised the continuous spectrum is successively transformed into two distinct line spectra, which I call respectively the compound line spectrum and the elementary line spectrum. It is one of the principal objects of this paper to show that these two line spectra which have been much mixed up together have a separate existence. The generation of one

always involves the destruction of the other. The fourth spectrum is that which is always seen at the negative pole in vacuum tubes filled with oxygen.

The Continuous Spectrum.—The following facts prove the statement that at the lowest temperature at which oxygen is luminous it shows a continuous spectrum.

1. The wide part of a Plücker tube generally shines with a faint yellow light. When looked at by means of a prism the spectrum is perfectly continuous.

2. If a spark of an ordinary Ruhmkorff coil is taken in oxygen at atmospheric pressure, one of the line spectra generally appears, but when the break is put out of adjustment so as to weaken the spark, the lines disappear and are replaced by a continuous spectrum which has its maximum of intensity in the greenish-yellow, and gradually fades away towards both ends of the spectrum.

3. Becquerel mentions an observation according to which the point of the oxyhydrogen flame takes a yellow colour when an excess of oxygen is present. The description of the somewhat characteristic colour which Becquerel gives coincides exactly with the colour of the spark in oxygen, when it shows the continuous spectrum. According to Plücker an excess of hydrogen shows the hydrogen lines, and it is therefore reasonable to suppose that in Becquerel's experiment the oxygen was sufficiently heated up to become luminous.

The continuous spectrum must not be confounded with the continuous spectrum which under high pressure forms the background to the line spectrum.

The Elementary Line Spectrum.—This is the spectrum which is seen when a strong spark passes through oxygen at the atmospheric pressure. It can be seen at all pressures when a jar and air break are introduced into the circuit. Several measurements exist of these lines, but only those of Thalèn are given in absolute measure. Thalèn's measurements, however, refer to the lines seen when a spark passes through air. Under these circumstances the oxygen lines are weak compared to the nitrogen lines, and some strong oxygen lines do not appear at all in Thalèn's list. I have made a careful measurement of all the lines. Some of the weaker bands have a different appearance and suggest, therefore, a doubt whether they are really due to oxygen; but they always appear with the same relative intensity, and I could not obtain any evidence which would justify their exclusion from the list of oxygen lines.

The Compound Line Spectrum.—Plücker in his first investigation of oxygen says it consists of four lines, one in the red, two in the green, and one in the blue. In his later drawing of the spectrum of oxygen, he gives a great number of lines of which these four form a part. Wüllner says that the four lines in question are always the

first to appear in oxygen tubes. Thalèn and Ångström do not give these lines; Huggins does not give them; Salet does not give them. Plücker and Wüllner are the only observers who experimented under the circumstances under which the lines appear. They come out equally well whatever way the oxygen is prepared, whatever the nature of the electrode, and I have seen them under the large glass receiver already mentioned. The following is the appearance of an oxygen tube as it undergoes gradual exhaustion.

When the pressure is sufficiently diminished to allow the spark to pass, it shows a yellow colour and the spectrum is perfectly continuous. Almost immediately, however, the four lines are seen in the capillary part of the tube above the continuous spectrum. The continuous spectrum in the wide part is then stronger than in the narrow part. The four lines seem to have taken away part of the energy of the continuous spectrum. As the exhaustion proceeds the spark spreads out in the wide part, and the continuous spectrum is therefore diminished and becomes less intense than in the capillary part; but it gradually loses in intensity also in the narrow part, until the four lines stand out on a perfectly black background. If under these circumstances the jar and air break is inserted in the circuit, everything will disappear and the elementary line spectrum will come out. We have here as complete a transformation as from the band spectrum of nitrogen to the line spectrum of nitrogen taking place under precisely the same circumstances; and it is therefore not unlikely that the two phenomena are due to the same cause. There are two reasons why the existence of the compound line spectrum of oxygen as a separate spectrum may have escaped previous observers. There is a blue line in the elementary line spectrum which is nearly coincident with the blue line of the compound line spectrum. It requires considerable dispersion to notice the difference; the complete disappearance of the compound line spectrum has therefore escaped notice. The two green lines and the red line widen easily at higher pressures, and as has been remarked by Wüllner, even fuse together to a continuous spectrum. If the experiment is therefore made at a pressure at which oxygen has a continuous background, the disappearance of these lines might be taken for their widening and fusing together. No such mistake is possible when the vacuum is good. I have not been able to determine with certainty whether the red line seen at atmospheric pressure is a remnant of the compound line spectrum, or whether it is a line of the elementary line spectrum closely coincident. I am inclined to the former view, although it often seemed as if the line seen at atmospheric pressure was less refrangible than the red line of the compound line spectrum. I have drawn attention in a letter to "Nature" (December 20), to the fact that the compound line spectrum of oxygen seems to be reversed in the sun. I have no further

information to add on that point, and the wave-length of the lines will be found in my letter.

The Spectrum of the Negative Pole.—This spectrum has first been correctly described by Wüllner. It consists of five bands, one of which is too weak to be measured. The appearance of each band has already been described (page 384). Careful measurements of the bands have been taken. With regard to the explanation of the separate spectra found at the negative pole in nearly all gases, I incline to the view that they are due to separate molecular compounds which are formed at the pole. The following experiments seem to support that view. When the pressure is very small the spectrum of the negative pole extends throughout that half of the tube which encloses the negative pole, and which I shall call the negative half. If the current be suddenly reversed the spectrum of the negative pole will still be seen at first, in that part which was the negative half and now is the positive half of the tube; but it will gradually disappear and a permanent state will be established, in which the spectrum of the negative pole is, as before, only seen in the negative half. That it is the reversal and not the interruption of the current which produces the result is easily proved by interrupting the current and at once closing it again the same way, when no difference will be seen. If, however, the current be left interrupted for some time, say one minute, so that any compounds which may have been formed in the negative half may diffuse into the other half, and if then the current is closed either the same or the opposite way, the negative spectrum will be seen at first throughout the tube, but gradually disappear in the positive half.

If the current be rapidly reversed in succession, after a little while, when the effect of the first reversal has disappeared, the permanent state will always be established at once, and the spectrum of the negative pole will appear only in the negative half.

If after the last experiment the current be interrupted for some time and then closed, the spectrum of the negative pole will at first be seen throughout the tube, and gradually disappear in the positive half.

It is not quite easy to see the explanation of the last two experiments.

The experiments were all made in the Cavendish Laboratory, Cambridge, and I am much obliged to Professor Clerk Maxwell, for the kindness with which he has placed the resources of the Laboratory at my disposal.

IV. "On the Variations of the Diurnal Range of the Magnetic Declination as recorded at the Prague Observatory." By Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at Owens College, Manchester. Received May 2, 1878.

1. The Prague magnetic observations began in July, 1839, and have been continued until the present date. The observation hours, 18h., 22h., 2h., 10h., are common to the whole series, except for the year 1853, during which observations were made only at the hours 18h., 2h., 10h. As far, however, as the estimation of the diurnal range of magnetic declination is concerned, these last three hours are practically as good as the former four, inasmuch as the observations at 22h. are hardly ever made use of in determining the diurnal range.

In the determinations herein recorded, magnetic disturbances are included, and the range is a mean monthly one, obtained by comparing together the mean values of the magnetic declination, corresponding to the hours 18h., 22h., 2h., 10h., for any given month, and taking the difference between the highest and the lowest of these values as representing the mean range for that month. There is reason to believe that the ranges thus obtained are not greatly different from those which would have been obtained from an hourly series of observations.

A. *Annual Variation of Declination-Range.*

2. In order to obtain this variation, the mean monthly ranges obtained, as already described, and extending from the beginning of 1840 to the end of 1876, have been made use of. From these we obtain the following table:—

Table I. Containing mean values, for each month in the year, of the declination-range at Prague, in minutes of arc, taken from the whole series of thirty-seven years.

Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
5′·298	6′·808	8′·636	10′·366	10′·607	11′·513	11′·184	10′·695	8′·361	7′·365	5′·668	4′·699

B. *Variations of Long Period.*

3. In order to investigate the long-period variation of the Prague declination-range, I have treated these observations precisely in the way in which the Kew declination-ranges were treated (Proc. Roy. Soc., March 22, 1877). By this method, proportional values of the declination-range at Prague have been obtained for the middle points of each month for each year, and it is believed that these values are freed from any recognised inequality depending either on the month of the

year or on the relative position of the sun and moon. These are exhibited in Table II.

TABLE II. Exhibiting monthly means of the declination-range, the mean value of the range for the whole series for each month, as given in Table I, being reckoned = 1000.

	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
1839							1214	1396	1204	1355	1392	1047
1840	1299	1420	1168	1234	1088	1061	1057	1007	1190	1184	1147	1626
1841	1193	1181	1085	925	949	1015	845	873	954	970	958	1107
1842	834	817	830	861	921	822	736	832	773	863	783	702
1843	706	839	731	782	789	866	825	780	934	814	556	821
1844	557	473	835	716	753	794	840	803	764	986	864	772
1845	710	718	877	833	977	959	854	970	835	739	734	958
1846	676	671	950	923	1086	999	1066	1029	905	948	958	834
1847	725	880	996	1012	1096	972	1017	1214	1236	1157	1461	1477
1848	1384	1209	1225	1034	1254	1251	1361	1407	1294	1211	1138	1373
1849	1459	1329	1259	1290	1266	1200	1207	1080	1073	1107	1161	983
1850	1316	1113	1152	1127	1270	1281	1206	1087	1136	1155	1094	996
1851	1206	918	830	938	1015	1074	1043	929	1024	1054	981	1286
1852	1193	1486	1164	1023	1000	1048	932	949	1060	1063	1178	1032
1853	934	773	1023	839	767	878	949	829	850	841	930	1030
1854	925	1275	924	976	999	876	942	899	760	850	680	868
1855	897	969	917	872	841	835	870	819	782	953	835	632
1856	649	824	630	786	762	829	805	866	871	817	771	611
1857	734	762	691	767	839	900	927	899	861	950	1034	862
1858	938	1074	908	1027	470	463	621	1003	1265	1359	1037	870
1859	655	920	1220	1365	1263	1167	1177	1239	1629	1442	1256	1704
1860	1104	1193	1395	1116	1290	1355	1336	1429	1142	1111	863	911
1861	946	1241	1004	1169	1113	1122	1034	1086	1014	887	1097	1209
1862	1014	730	719	915	888	1168	1130	1165	895	1295	1111	1015
1863	1306	1135	1072	1044	1150	967	957	928	954	1019	1057	1177
1864	998	845	1085	876	979	1033	915	942	823	1001	1018	819
1865	1072	1044	1173	928	1005	900	846	894	999	834	949	498
1866	1172	1353	835	901	865	849	848	697	804	731	1124	677
1867	831	767	864	840	831	858	910	821	794	703	764	815
1868	874	836	991	1084	884	872	947	972	944	921	1002	1145
1869	1053	1041	1129	1041	1033	1270	1177	1033	1135	1096	932	930
1870	1142	989	1308	1386	1452	1318	1393	1369	1353	1287	1429	1400
1871	1183	1547	1315	1425	1289	1346	1354	1453	1257	1188	1581	1296
1872	1368	1031	1147	1292	1258	1195	1254	1289	1276	1113	1380	1502
1873	1533	1052	1121	1166	1015	869	1017	1012	1018	1018	985	1026
1874	1229	1094	932	928	950	904	939	825	931	846	872	651
1875	464	830	778	848	867	858	783	824	780	695	621	666
1876	721	623	717	711	724	826	880	750	687	796	665	724

4. The numbers of Table II have next been dealt with precisely in the way in which the corresponding numbers were dealt with in the case of the Kew and Trevandrum observations, that is to say, a set of nine-monthly values of declination-range has been obtained, corresponding to similiar nine-monthly values of spotted solar area. These are exhibited in the following tables, up to the end of the year 1852, after which date a comparison between the spots and declination-

ranges has already been made in previous communications (Proc. Roy. Soc., March 22, 1877, and Feb. 7, 1878).

TABLE III. Prague Declination-Range.—Nine-Monthly Values.*

	1839.	1841.	1843.	1845.	1847.	1849.	1851.	1853.
Jan. (0)		1167	776	824	924	1299	1036	974
Feb. (0)		1155	779	844	932	1286	1028	953
Mar. (0)		1126	782	847	940	1280	1019	936
April (0)		1092	780	846	957	1277	1003	911
May (0)		1040	793	855	994	1257	995	882
June (0)		990	812	861	1040	1220	989	866
July (0)		965	802	863	1096	1192	985	870
Aug. (0)		954	791	869	1155	1167	1013	879
Sept. (0)		950	783	864	1203	1153	1052	884
Oct. (0)		937	753	839	1230	1146	1093	917
Nov. (0)		920	734	821	1250	1135	1124	947
Dec. (0)	1278	911	727	824	1266	1128	1128	951

	1840.	1842.	1844.	1846.	1848.	1850.	1852.	1854.
Jan. (0)	1262	914	719	835	1269	1134	1131	962
Feb. (0)	1237	909	710	850	1271	1156	1136	973
Mar. (0)	1213	889	703	877	1283	1173	1130	980
April (0)	1175	869	718	912	1292	1175	1122	984
May (0)	1161	843	729	926	1279	1180	1107	968
June (0)	1163	826	750	938	1259	1178	1088	949
July (0)	1141	826	795	969	1245	1168	1063	912
Aug. (0)	1151	817	813	978	1250	1159	1039	875
Sept. (0)	1175	801	810	961	1281	1154	1027	868
Oct. (0)	1178	789	808	938	1310	1139	1009	862
Nov. (0)	1185	779	810	927	1315	1094	995	862
Dec. (0)	1178	776	814	924	1311	1055	989	861

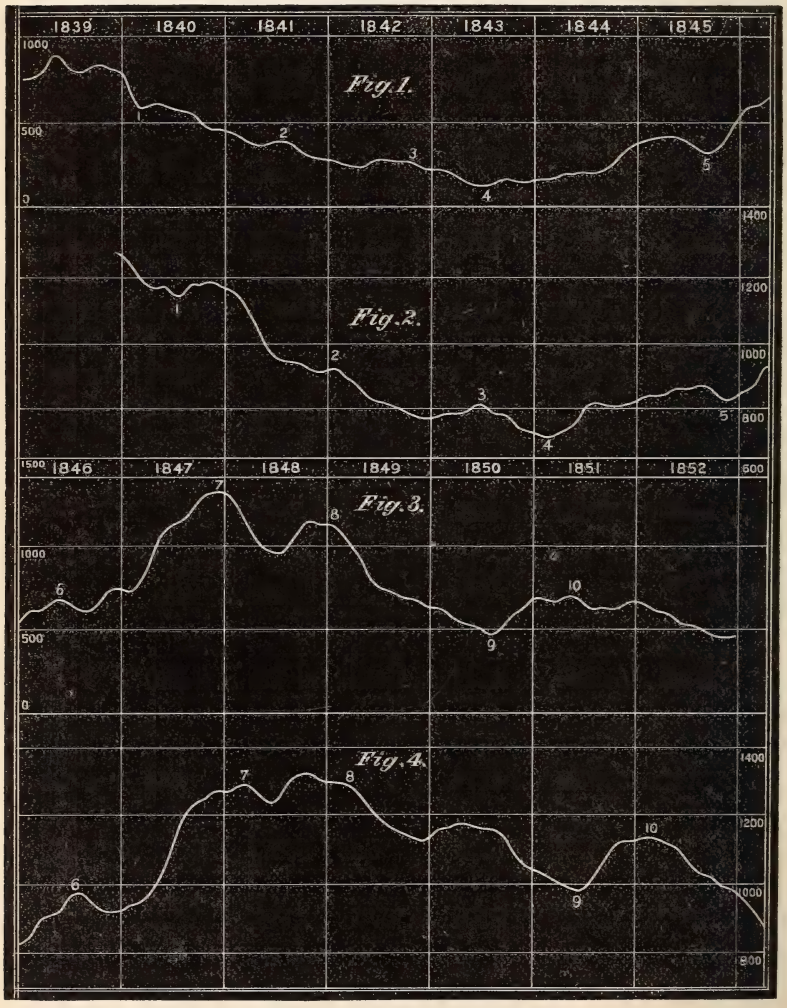
TABLE IV. Spotted Solar Areas.—Nine-Monthly Values.*

	1839.	1841.	1843.	1845.	1847.	1849.	1851.
Jan. (2)	730	456	181	384	722	1113	675
Feb. (2)	731	436	171	398	705	1073	668
Mar. (2)	745	400	144	407	728	1008	663
April (2)	795	362	115	419	805	964	677
May (2)	866	358	101	413	946	890	695
June (2)	853	363	95	403	1068	797	678
July (2)	792	369	102	382	1103	751	632
Aug. (2)	772	365	114	353	1148	726	618
Sept. (2)	787	335	111	348	1231	694	615
Oct. (2)	795	293	102	383	1288	678	613
Nov. (2)	786	264	103	444	1305	661	638
Dec. (2)	775	255	117	506	1309	632	656

* The numbers in Table III correspond to the beginning of each month, while those in Table IV correspond to the middle of each month.

	1840.	1842.	1844.	1846.	1848.	1850.	1852.
Jan. (2)	714	246	129	569	1276	621	646
Feb. (2)	616	221	137	607	1189	617	614
Mar. (2)	568	205	160	618	1104	586	589
April (2)	583	200	175	646	1038	547	577
May (2)	598	196	179	680	996	528	543
June (2)	588	209	183	675	981	510	521
July (2)	577	226	180	647	976	487	517
Aug. (2)	563	224	195	614	1031	481	492
Sept. (2)	526	217	218	609	1115	519	468
Oct. (2)	479	206	260	656	1142	571	451
Nov. (2)	463	187	322	714	1137	604	456
Dec. (2)	468	180	360	737	1124	646	467

Diagram I.



5. The results of Tables III and IV are exhibited in Diagram I above, in which figs. 1 and 3 represent sun-spot values, and figs. 2 and 4 Prague declination-ranges. From these figures it will be seen that a number of points in the sun-spot curves may be fairly identified as corresponding to certain points in the declination-range curve, but that the latter invariably lag behind the former in time. It ought, however, to be borne in mind that here the materials for comparison are not quite of the same order of completeness as in the case of Kew and Trevandrum.

The lagging behind may be well seen by comparing together the epochs of maximum and minimum sun-spot frequency with the corresponding epochs of declination maximum and minimum range. We thus obtain the following result:—

TABLE V. In which the Epochs of Maximum and Minimum Sun-Spots are Compared with those of Declination-Range.

Solar min., June 15, 1843...	Prague, dec.-range, min., Feb. 28, 1844.
Solar max., Dec. 15, 1847...	Prague, dec.-range, point 7, max., March 31, 1848.
Solar min., Sept. 15, 1855	Prague, dec.-range, min., Mar. 31, 1856. Trev., dec.-range, min., Feb. 15, 1856.
Solar max., Nov. 15, 1859	Kew, dec.-range, max., April 15, 1860. Trev., dec.-range, max., May 15, 1860.
Solar min., Mar. 15, 1867...	Kew, dec.-range, min., August 15, 1867.

I have thus examined the most trustworthy sun-spot values and declination-ranges, and it may, I think, be fairly concluded that there is an intimate relation between the two phenomena, but that the points of the sun-spot ranges precede those of the declination-ranges in respect of time.

Variations which seem to depend on Planetary Configurations.

6. The Prague proportional values herein given cannot be regarded as equally good for the purpose of investigating these periods with those derived from Kew or Trevandrum. In the Prague series we have only one value for each month, whereas in the Kew or Trevandrum series we have one value for each week. Inasmuch, however, as the Prague series is longer than either of the others we may, perhaps, regard it as of equal value for the purpose now in hand with the Kew series, while the Trevandrum series, on account of its comparatively short duration, can hardly be regarded as possessing more than half the weight of either of the others. If we treat the Prague observations in the manner in which the Kew observations were treated (Proc. Roy. Soc., March 22nd, 1877), we obtain the following result:—

TABLE VI.—Venus and Mercury together (0° denotes Conjunction, in all 94 sets).

Between	0	and	30°	+700
„	30	„	60	−171
„	60	„	90	−553
„	90	„	120	−100
„	120	„	150	+314
„	150	„	180	+ 92
„	180	„	210	− 31
„	210	„	240	+107
„	240	„	270	−301
„	270	„	300	−814
„	300	„	330	−218
„	330	„	360	+796

a result which is similar to that derived from Kew, in which we have manifest indications of a single, with some traces of a double, period.

7. If we next take the period of Mercury about the sun, we obtain the following result:—

TABLE VII.—Period of Mercury about the Sun (in all 153 sets, 0° denotes Perihelion).

				First half.	Second half.	Whole series.
Between	0°	and	30°	+ 565	−209	+ 356
„	30	„	60	+ 99	−384	− 285
„	60	„	90	− 441	−483	− 924
„	90	„	120	− 897	−442	−1339
„	120	„	150	−1079	− 64	−1143
„	150	„	180	− 981	+272	− 709
„	180	„	210	− 646	+389	− 257
„	210	„	240	− 176	+442	+ 266
„	240	„	270	+ 369	+407	+ 776
„	270	„	300	+ 667	+258	+ 925
„	300	„	330	+ 784	+165	+ 949
„	330	„	360	+ 841	+ 59	+ 900

We thus perceive a very considerable likeness between the results derived from the two halves, while the whole is very similar to the corresponding periods derived from the Kew or from the Trevandrum series.

8. Let us finally take the period of conjunction of Mercury and Jupiter, and we obtain the following result:—

TABLE VIII.—Period of Conjunction of Mercury and Jupiter (in all 150 sets, 0° denotes Conjunction).

				First half.	Second half.	Whole series.
Between	0°	and	30°	+ 47	+775	+ 822
„	30	„	60	−237	+217	− 20
„	60	„	90	−402	−358	− 760
„	90	„	120	−541	−738	−1279
„	120	„	150	−583	−740	−1323
„	150	„	180	−512	−402	− 914
„	180	„	210	−328	− 66	− 394
„	210	„	240	+ 21	+ 1	+ 22
„	240	„	270	+435	+ 90	+ 525
„	270	„	300	+636	+421	+1057
„	300	„	330	+496	+686	+1182
„	330	„	360	+300	+885	+1185

Here, as before, we have a very considerable likeness between the results derived from the two halves, while the whole is very similar to the corresponding period derived from the Kew series or from that of Trevandrum.

9. Let us now try to combine together the planetary periods derived from the three observatories—Kew, Trevandrum, and Prague—giving equal value to the results of Kew and Prague, and half value to those of Trevandrum.

We thus obtain the following values of a single period, the unit being as before, one thousandth of the whole mean range:—

TABLE IX.—Mean Magnetic Result from the various Observatories.

				Venus and Mercury.		
				Kew.	Prague.	Mean.
Between	0°	and	30°	+4·95	+7·45	+6·20
„	30	„	60	+0·59	−1·82	−0·61
„	60	„	90	−5·03	−5·88	−5·45
„	90	„	120	−5·31	−1·06	−3·18
„	120	„	150	−2·38	+3·34	+0·48
„	150	„	180	−1·51	+0·98	−0·26
„	180	„	210	−1·10	−0·33	−0·71
„	210	„	240	+0·33	+1·14	+0·73
„	240	„	270	+0·66	−3·20	−1·27
„	270	„	300	−1·33	−8·66	−4·99
„	300	„	330	−1·26	−2·32	−1·79
„	330	„	360	+3·05	+8·47	+5·76

Period of Mercury.

				Kew.	Trev.*	Prague.	Mean.
Between	0°	and	30°	+6·60	+ 3·02	+2·33	+4·18
„	30	„	60	+6·66	+ 3·91	−1·86	+2·70
„	60	„	90	+3·94	+ 6·16	−6·04	+0·39
„	90	„	120	+0·08	+ 8·77	−8·75	−1·71
„	120	„	150	−4·31	+ 6·21	−7·47	−3·47
„	150	„	180	−6·75	− 5·19	−4·64	−5·59
„	180	„	210	−6·35	−15·35	−1·67	−6·28
„	210	„	240	−4·29	−15·23	+1·74	−4·07
„	240	„	270	−2·15	− 8·14	+5·07	−0·46
„	270	„	300	+0·20	+ 2·39	+6·04	+2·97
„	300	„	330	+2·43	+ 9·02	+6·20	+5·26
„	330	„	360	+4·28	+ 6·26	+5·88	+5·32

Mercury and Jupiter.

				Kew.	Trev.	Prague.	Mean.
Between	0°	and	30°	+10·05	+10·53	+5·48	+8·32
„	30	„	60	+12·05	+ 6·28	−0·13	+6·02
„	60	„	90	+10·35	+ 3·00	−5·07	+2·71
„	90	„	120	+ 5·21	− 2·74	−8·53	−1·88
„	120	„	150	− 1·94	− 8·93	−8·82	−6·09
„	150	„	180	− 8·00	−10·86	−6·09	−7·81
„	180	„	210	−10·76	−11·33	−2·63	−7·62
„	210	„	240	−10·76	− 9·51	+0·15	−6·15
„	240	„	270	− 8·70	− 2·84	+3·50	−2·65
„	270	„	300	− 5·11	+ 5·16	+7·05	+1·81
„	300	„	330	− 0·16	+ 9·65	+7·88	+5·02
„	330	„	360	+ 5·44	+11·70	+7·90	+7·68

10. The mean sun-spot results corresponding to these three periods may be derived from the researches of Messrs. De La Rue, Stewart, and Loewy (Phil. Trans., 1870). For a single period they are as follows, the solar unit being one millionth of the sun's visible hemisphere :—

Table X.—Mean Solar Results.

				Venus and Mercury.	Period of Mercury.	Mercury and Jupiter.
Between	0°	and	30°	+18·61	−2·57	−1·56
„	30	„	60	− 1·54	−8·03	−2·17
„	60	„	90	−18·50	−8·69	−4·07
„	90	„	120	−26·17	−8·53	−4·89

* I take this opportunity of mentioning that a slight error has occurred in my determination of the inequality due to the period of Mercury, as shown by the Trevandrum observations (Proc. Roy. Soc., February 7, 1878), and that the above is the correct result.

				Venus and Mercury.	Period of Mercury.	Mercury and Jupiter.
Between	120°	and	150°	−25·76	−8·16	−3·48
„	150	„	180	−17·82	−6·94	−3·36
„	180	„	210	− 5·34	−3·50	−2·85
„	210	„	240	+ 6·08	+2·91	−1·29
„	240	„	270	+ 4·79	+7·31	−0·17
„	270	„	300	+ 2·53	+9·70	+1·05
„	300	„	330	+14·64	+8·66	+1·12
„	330	„	360	+25·37	+3·96	−0·31

These three planetary periods, as shown by the mean magnetic results of the three Observatories recorded in Table IX, are exhibited in Diagram II, in which fig. 2 denotes the period of Mercury, fig. 4 that of Mercury and Jupiter, and fig. 6 that of Mercury and Venus, while, as shown by the sun-spot results of Table X, they are exhibited in figs. 1, 3, and 5, fig. 1 denoting the period of Mercury, fig. 3 that of Mercury and Jupiter, and fig. 5 that of Mercury and Venus.

11. If we compare together the three sun-spot periods with the three magnetic periods, as exhibited in these diagrams, we shall remark a great similarity between them, while, however, as we might expect, the declination results lag behind the solar results in point of time.

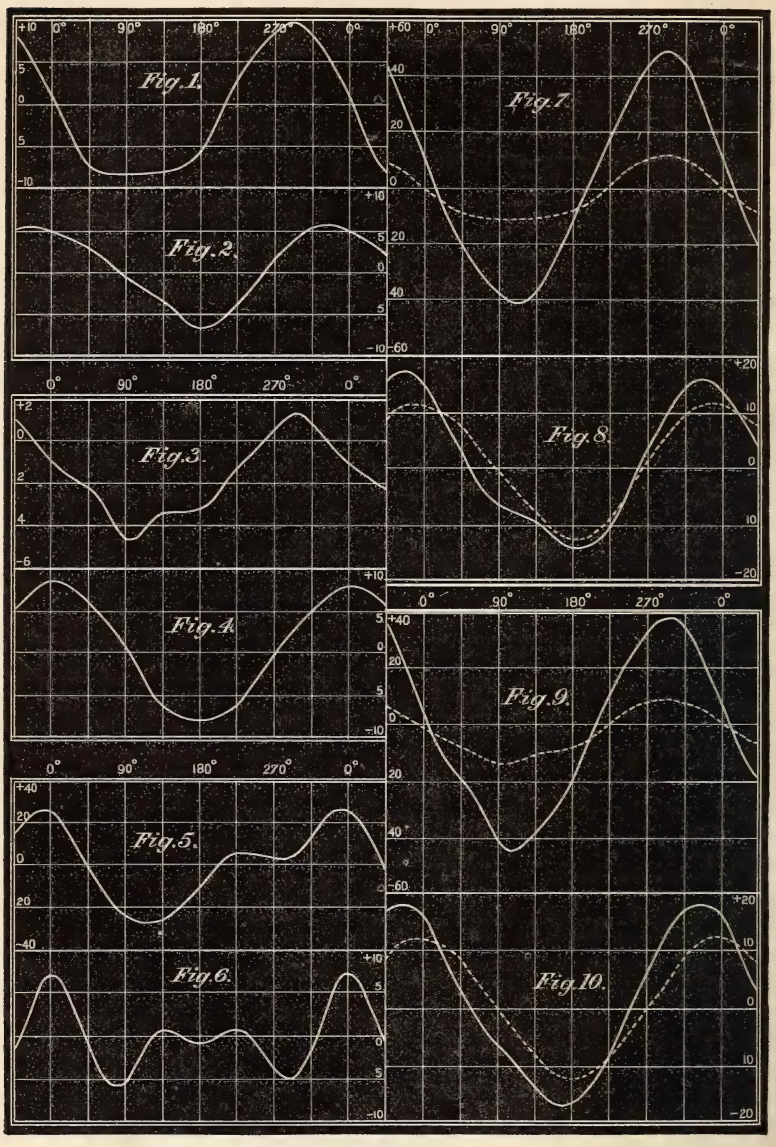
12. An inspection of the sun-spot records reveals the fact that at times of maximum spot frequency, not only are there most spots on the sun, but that the sun-spot inequalities or oscillations (however produced) are at such times much more prominent than during times of minimum sun-spot frequency. Now, if it be true that these spot periods are due in a great measure, if not entirely, to planetary configurations, we might expect that (possibly from an increase in the susceptibility of the sun) the planetary periods herein investigated should at times of maximum sun-spots be found to be greater than their average value.

13. I have endeavoured to test this in the following manner:—

The two most available periods are that of the orbit of Mercury round the sun, and that of the synodic revolution of Mercury and Jupiter. The average sun-spot inequalities for a single period of each of these have already been given in Table X. Now, it might be supposed that we have only for the present purpose to take these periods and find whether their values, during times of maximum sun-spots, are greater than their mean values. It is, however, a curious and interesting fact that (as far back as accurate observations extend) times of many sun-spots correspond well with times when Jupiter is at the perihelion of Mercury. Now, it is easy to see that on this account alone, and apart from any increased susceptibility of the sun, we should have sun-spot inequalities greater than the average at the times when the two planets are in this position with respect to each other. For

taking the average sun-spot inequality due to the period of Mercury (see Table X), we find a spot maximum somewhat before the time

Diagram II.



when Mercury comes to its perihelion, and again taking the sun-spot inequality due to the period of Mercury and Jupiter (see Table X), we have likewise a maximum somewhat before the time when Mercury

and Jupiter come together. If then Jupiter be at the same ecliptical longitude as that of the perihelion of Mercury, we should expect prominent oscillations from the effect of superposition of the two periods alone, apart from any increased susceptibility of the sun.

14. Suppose we now take a group of fifteen periods, embracing nearly four years, around each of the epochs when Jupiter is in this position with regard to Mercury. These epochs will be as follows (as far as available sun-spot observations are concerned): March, 1835, January, 1847, November, 1858. Let us make use of these selected periods to determine the inequality due to the period of Mercury, and also that due to the synodic revolution of Mercury and Jupiter. Even if there be no increase on these occasions of the susceptibility of the sun, we shall have results greater than the mean for each of the inequalities so determined from the effect of superposition alone. In other words, the apparent Mercury inequality is mixed up with and exaggerated by the superposition of the Mercury and Jupiter inequality, while the apparent Mercury and Jupiter inequality is, in its turn, mixed up with and exaggerated by the Mercury inequality. Now, on the supposition that the susceptibility of the sun does not alter, we can calculate from knowing the mean inequalities what these apparent inequalities ought to be, because we can calculate the effect of mere superposition of the one upon the other. These we may call the calculated inequalities. Now, if there be an exaltation due, let us imagine to an increased susceptibility of the sun on these occasions (which are also those of numerous sun-spots) the observed planetary inequalities should be greater than the calculated. It will be seen from the following table that this is really the case.

Table XI. Comparing together the observed and calculated sun-spot inequalities for selected periods.

				Period of Mercury (one revolution).	
				Observed.	Calculated.
Between	0°	and	30°	− 2·31	− 3·95
,,	30	,,	60	−20·85	−10·63
,,	60	,,	90	−33·07	−12·10
,,	90	,,	120	−40·37	−12·33
,,	120	,,	150	−37·50	−11·96
,,	150	,,	180	−21·30	−10·13
,,	180	,,	210	− 2·04	− 5·83
,,	210	,,	240	+18·29	+ 1·54
,,	240	,,	270	+37·09	+ 6·99
,,	270	,,	300	+47·73	+10·00
,,	300	,,	330	+43·55	+ 8·91
,,	330	,,	360	+22·22	+ 3·63

Mercury and Jupiter together (one revolution).

				Observed.	Calculated.
Between	0°	and	30°	− 5·76	− 3·22
,,	30	,,	60	−18·95	− 7·56
,,	60	,,	90	−33·26	−11·72
,,	90	,,	120	−43·66	−13·11
,,	120	,,	150	−37·91	−10·91
,,	150	,,	180	−27·22	− 8·71
,,	180	,,	210	−11·24	− 4·84
,,	210	,,	240	+10·78	+ 0·73
,,	240	,,	270	+27·76	+ 5·45
,,	270	,,	300	+37·35	+ 8·33
,,	300	,,	330	+35·33	+ 7·29
,,	330	,,	360	+16·62	+ 2·41

The results of Table XI are exhibited in Diagram II, in which fig. 7 gives the observed and the calculated sun-spot inequalities for the period of Mercury, and fig. 9 the same for the period of Mercury and Jupiter.

15. If we now turn to declination-ranges we shall find that there are greater oscillations or sub-periods in the value of these ranges during times of maximum than during times of minimum sun-spots. But on the other hand the increased value of such oscillations is by no means so striking as in the case of sun-spots. Mr. Broun has already made the remark that while there is an increase in the whole declination-range during times of maximum sun-spots, yet this increase is not so marked as in the case of the spots themselves, inasmuch as we have a considerable declination-range when there are no spots on the sun. From what has now been said it would seem that a similar remark applies to the oscillations or sub-periods of declination-range, which, while increasing from times of minimum to times of maximum sun-spots, do not yet increase so strikingly as the oscillations or sub-periods of the spots themselves.

16. If we now treat the inequalities of magnetic declination that appear to depend on the two most available planetary configurations in the manner in which we have just treated sun-spot inequalities, we might expect the observed magnetic inequalities corresponding to times of maximum sun-spots to be greater than the calculated inequalities, but not to the same extent as in the case of sun-spots.

Let us make use for this purpose of the records of the three Observatories, Kew, Prague, and Trevandrum. We cannot, however, take absolutely the same epochs that we have taken for the sun, inasmuch as for the first of these, March, 1835, there are no magnetic observations. We may, however, take the other two epochs, January, 1847, November, 1858, and an additional one at October, 1870. Thus we shall

have three epochs in each case, while only two of these are common to both solar and magnetic observations. This comparison is made in the following table.

Table XII. In which observed and calculated declination-range inequalities are compared together for selected periods.

				Period of Mercury (one revolution).	
				Observed.	Calculated.
Between	0°	and	30°	+11·48	+10·42
,,	30	,,	60	+ 3·62	+ 7·25
,,	60	,,	90	− 3·50	+ 2·25
,,	90	,,	120	− 6·91	− 3·25
,,	120	,,	150	− 9·13	− 8·16
,,	150	,,	180	−12·37	−11·67
,,	180	,,	210	−13·72	−12·12
,,	210	,,	240	−10·44	− 8·68
,,	240	,,	270	− 2·45	− 2·62
,,	270	,,	300	+ 7·73	+ 4·10
,,	300	,,	330	+15·14	+ 9·26
,,	330	,,	360	+16·20	+11·27

				Mercury and Jupiter together (one revolution).	
				Observed.	Calculated.
Between	0°	and	30°	+11·87	+11·61
,,	30	,,	60	+ 2·56	+ 8·07
,,	60	,,	90	− 4·26	+ 2·75
,,	90	,,	120	− 8·72	− 2·45
,,	120	,,	150	−13·85	− 7·93
,,	150	,,	180	−16·24	−11·97
,,	180	,,	210	−13·44	−11·80
,,	210	,,	240	− 8·32	− 8·71
,,	240	,,	270	+ 0·51	− 3·11
,,	270	,,	300	+11·39	+ 3·44
,,	300	,,	330	+16·91	+ 8·74
,,	330	,,	360	+17·06	+11·89

The results of Table XII are exhibited in Diagram II, in which fig. 8 exhibits the observed and calculated magnetic inequalities for the period of Mercury, and fig. 10 the same for the period of Mercury and Jupiter.

17. It thus appears that in the case of the magnetic declination periods there is (as in those of sun-spots) an exaltation of the observed over the calculated values during times of maximum sun-spot frequency, but this exaltation is not so marked as in the case of sun-spots. Now, without pretending to know in what way the sun influences

the magnetism of the earth, we may imagine that the increased values not only of the average declination-range but also of the sub-periods of these during times of maximum sun-spots may be due to one of two causes, or to both of these together. Thus we may imagine that the sun has an increased magnetic influence during such periods, or we may imagine that there is an increase in the magnetic susceptibility of the earth; or, finally, we may imagine that both of these causes operate together. I cannot help thinking that we have some evidence of an increase of the magnetic susceptibility of the earth on such occasions derived from two facts discovered by Mr. Broun. The one is that the magnetic influence of the moon on the earth shows traces of following the solar period, this influence being greater during times of maximum than during times of minimum sun-spots. The other is that at Trevandrum the lunar magnetic influence, without changing its type, exhibits an increase of value when the sun is above the horizon at that place, as if on such occasions there were an increase of susceptibility to the lunar influence. These, however, are points which can only be determined by a further discussion of observations.

In conclusion, I beg to record my thanks to Mr. W. Dodgson and to Mr. Morisabro Hiraoka, who have kindly assisted me in the work of this paper.

Presents, April 4, 1878.

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May 23, 1878.

Sir JOSEPH HOOKER, K.C.S.I., in the Chair.

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

The following Papers were read:—

I. "Researches in Spectrum Analysis in connexion with the Spectrum of the Sun." No. V. By J. Norman Lockyer, F.R.S. Received April 29, 1878.

(Abstract.)

The author submits the first strip of a new normal map of the Solar Spectrum, on four times the scale of Ängström's. It embraces W. L. 3900—4000, and has been produced by means of photography.

II. "Experimental Results relating to the Rhythmical and Excitatory Motions of the Ventricle of the Heart of the Frog, and of the Electrical Phenomena which accompany them." By J. Burdon Sanderson, M.D., LL.D., F.R.S., and F. J. M. Page, B.Sc., F.C.S. Received May 6, 1878.

The results which are shortly stated in the following paragraphs relate to (1) the order and duration of the rhythmical and excitatory motions of the heart of the frog; (2) the normal electrical condition of the surface of the heart and the influence thereon of mechanical, chemical, and thermal injuries; and (3) the characters of the normal and of the excitatory electrical variation, and the modifications of those characters which are induced by injuries of the surface, and under the temporary influence of radiant heat.

Section I.—*Order and Duration of the Motions of the Heart.*

1. *Duration of the Ventricular Systole.*—In the rhythmically contracting excised heart, *i.e.*, in the heart removed by cutting across the sinus, which will in this paper be called "the entire heart," the frequency of the contractions is usually, in winter frogs, a little over 30 per minute. The systole of the ventricle lasts about one second. The contraction attains its maximum in a little more than half a second after its commencement, declining at first gradually, afterwards more suddenly. The sudden relaxation occurs between 0″·7 and 0″·9 after the commencement. At the close of this period the ventricle does not become entirely flaccid, for a lever resting upon it continues to descend for about a third of a second.

In the rhythmically contracting heart, removed by cutting through the auricles, to be called hereafter "the ventricle preparation," the duration of the ventricular systole is about 1″·35. It attains its maximum at 0″·8 after its commencement, and relaxes rapidly between 1″·1 and 1″·3.

In these statements the word commencement is understood to mean the beginning of the hardening of the ventricle. It is preceded by a period of partial contraction of which the duration is about 0″·15, and which we propose to call the period of præ-contraction.

2. *Relation between the Periods of Contraction of the Auricles and Ventricles.*—In the entire heart, contracting rhythmically, the contraction of the auricle begins about 1″·4 before that of the ventricle, and the maximum of the auricular systole is attained 1″·2 before that of the ventricle. There is an interval of not less than half a second between the end of the auricular contraction as indicated by the movement of a light lever resting upon it and the beginning of that of the ventricle. It follows from this statement that when the heart is acting at its normal frequency, the beginning of the auricular contraction nearly

coincides with the maximum of the preceding ventricular systole, and that their endings are synchronous.

3. *The Stannius' Heart.*—In a heart of which the rhythmical action has been arrested by the application of a tight ligature round the line of junction between the wall of the sinus and of the auricles ("Stannius' ligature"), the duration of the ventricular contraction, determined by electrical or mechanical excitation, is much longer than in the beating heart. The contraction attains its maximum from 1″·4 to 1″·8 after the commencement as above defined, the ventricle remaining in full contraction for from 0″·4 to 0″·5. Accordingly the moment at which the sudden relaxation takes place varies between 1″·8 and 2″·0.

4. *Time interval between Excitation and Contraction.*—When a contraction is produced in a heart ligatured as above described (which may be referred to as a "Stannius' heart") by the passage through any part of its substance of a single induction shock, a sensible delay intervenes between the excitation and the commencement of the contraction. If the electrodes are in contact with two points of the surface of the ventricle at a short distance (one millim.) from each other, and currents of small intensity, *i.e.*, such as are just sufficient to produce an effect, are used, the delay may amount to three-tenths of a second. In this case contraction of the auricle commences after that of the ventricle, the time interval between them being from 0″·4 to 0″·5. If the seat of excitation be the posterior surface of the left auricle near the ligature, the auricle contracts after a delay of about one-sixth of a second, but the ventricular contraction does not commence until from 0″·4 to 0″·6 later. It frequently happens that the ventricular systole produced by excitation of the auricle is followed after an interval of inaction of about 1″·3 by a second contraction of the auricles.

Section II.—*Electrical Condition of the Surface of the Uninjured and of the Injured Heart.*

5. *The Uninjured Heart.*—It was shown by Engelmann in 1873, that the surface of the uninjured resting heart is isoelectric, and further, that the slightest mechanical or chemical injury renders the injured part negative. These facts we have confirmed in numerous observations relating both to the heart of the toad and of the frog.

Nature of the Injuries Investigated.—In order to investigate the influence of a local injury on the condition of the surface of the heart, its rhythmical motion must be suspended. This may be done either by the passage of a rapid succession of induction shocks in opposite directions through the inhibitory tract, by applying to the surface a trace of a solution of muscarin, or by Stannius' ligature. A localized injury of the surface may be produced either by the scratch of a needle, by touching it with hot wire (preferably a platinum wire heated elec-

trically), by creosote, or by a fine wire covered with a thin layer of fused nitrate of silver.

6. *Comparison of Sound with Uninjured Spots on the Surface of the Resting Heart.*—When the resting heart is connected by its base and apex with a galvanoscopic circuit, and one of the two isoelectrical spots on the surface of the organ by which it is led off is injured in any of the ways above mentioned, the injured spot is found to have become negative to the sound surface. The electrical difference between them may amount to 0·03 volt, but, as Engelmann found, that difference usually diminishes very rapidly.

This effect is for the most part not dependent on the distance of the sound spot from the injured spot. Thus, if an injured spot near the apex is compared (*a*) with a sound spot 2 millims. from it, and (*b*) with another near the base, *i.e.*, about 5 millims. distant, it is often found that the differences between the two results are very inconsiderable. This experiment shows that the electromotive forces developed by injury have their seat in the immediate neighbourhood of the injured surfaces.

7. *Influence of Radiant Heat.*—When two isoelectrical points, (*e.g.*, at apex and base) on the surface of the heart are led off, and one of them is warmed by the approach of a platinum coil (about 2 millims. in diameter) heated by a voltaic current and brought into its neighbourhood (3 millims. distant) for a period of one second, the warmed spot becomes *positive* to the other. The amount of difference has not been observed to exceed one-thousandth of a Daniell. No movement of the needle occurs until about a second after the approach of the coil. This effect is transitory, its duration being less than five seconds. If the distance at which the coil is placed is diminished, the warmed surface becomes for a moment positive, then permanently negative. A similar effect is observed when the warming is prolonged without diminishing the distance.

Section III.—*Characters and Modifications of the Electrical Variation.*

As was first observed by Kölliker and H. Müller in 1854, an electrical disturbance or variation precedes and accompanies each contraction of the ventricle of the heart of the frog. In the uninjured heart the extent of this variation is extremely small, but is at once increased by injury.

8. *The Normal Variation.*—In the uninjured pulsating heart the variation consists of two phases, viz., of an initial disturbance of short duration, in which the apex becomes positive, and of a much longer second phase, in which the apex tends to negativity. This statement, which relates to the heart cut off through the sinus, is also applicable to the organ while it still forms part of the body. In the entire heart the initial phase begins a quarter of a second before the

commencement of the hardening of the ventricle, *i.e.*, 0″·1 before the beginning of the præ-contraction. (Sec. I.) The second phase ends about 0″·9 after the beginning of the first, of which the duration is less than a tenth of a second. Consequently the first phase of the variation is completed 0″·15 before the commencement of the contraction of the ventricle, its completion being immediately followed by the beginning of the præ-contraction. The second phase lasts through the period of active contraction.

9. *Time Interval between the Excitation and the Variation.*—In the Stannius' heart when the posterior surface of the auricle near the septum is excited, the resulting contraction (as stated in 4) follows the excitation at an interval of about two-thirds of a second. In this case the first phase of the variation precedes the contraction by a very constant interval of a quarter of a second. When the heart is excited from the ventricle half way between the base and apex, left side, the delay is diminished to about a third of a second, but there is no diminution of the interval between the initial phase of the variation and the contraction.

10. *Effect of Injury on the Variation.*—If in a normal entire heart, led off at base and at apex, the surface of the ventricle near the base is injured, the character of the variation is changed, the second phase ("apex-negative") being intensified. If the apex is then similarly injured, the second phase is abolished, the variation being indicated by a single large excursion in the direction of the first phase ("apex-positive").

In the ventrical preparation, the variation, provided that the surface of the organ be uninjured, exhibits two phases, of which the first is identical with the first phase in the entire heart, the second is represented, when investigated with the aid of the capillary electrometer, by a larger excursion in the direction "apex-negative," of which the amplitude is not so great as that of the single excursion observed after injury of the apex. After a time the excursion diminishes in extent and duration, so that, finally, the characters of the normal variation reappear. In a similar manner, if now the surface of the ventricle is injured, similar phenomena to those already described, with reference to the entire heart, present themselves, according to the seat of the injury. If it is at the apex, whether it be mechanical or chemical, the whole variation is represented by a single large excursion (apex-positive); if it is at the base there is a præ-excursion, in which the apex becomes momentarily positive, followed by a more prolonged excursion in the opposite direction, corresponding in direction to the second phase.

It has been already shown, in 6, that the electrical difference between a sound and an injured surface is not materially affected by distance. In like manner, the excursions which present themselves

when two spots, 2 millims. apart, of which one has been injured, are led off, nearly equal in amplitude those which are observed when the contacts are respectively at apex and base.

11. *Decline of Effect after Injury.*—The electrical difference between an injured and a sound surface begins to decline from the moment of the infliction of the injury which produces it. This decline is not accompanied by a corresponding diminution of the variation, the eventual decline of which is often preceded by a temporary increase.

12. *Modifications observed under the Influence of Radiant Heat.*—By warming the surface of the ventricle in the manner described in 7, the following changes are produced in the character of the variation. In the uninjured rhythmically contracting heart, in which the variation has the normal character, the approach of the wire for five seconds to a distance of 3 millims. from the surface of the auriculo-ventricular groove intensifies the second phase and increases its duration. This effect lasts for two minutes at most. It subsides at first rapidly—so that each succeeding variation differs sensibly from the preceding one —afterwards more gradually.

If the apex be similarly warmed, the second phase is either obliterated or reversed, according to the time of exposure and to the distance of the coil. At first the variation assumes the characters witnessed after injury of the apex, as described in 10; but here, as when the base of the ventricle is warmed, the effects subside rapidly during the first five seconds, more gradually afterwards. In both cases the variation has, in about two minutes, resumed its normal characters.

In the above experiments it is seen that, although the warming influences the potential of the surface acted on in a direction which is opposite to that produced by a permanent injury, its effect on the variation is precisely the same, with this important difference, that it is transitory. These effects can be observed whether the entire heart or the ventricle alone is used.

13. *Modifications dependent on the Seat of Excitation.*—If in the entire heart, of which the motions have been arrested, either by faradization of the inhibitory tract or by the Stannius' ligature, a single contraction is induced either by a mechanical or electrical excitation of any part of the auricles or of the auriculo-ventricular groove, the excursions present the same characters as in the rhythmically contracting heart. If the excitation, whether electrical or mechanical, be at the apex, the first phase of the variation is *reversed*, *i.e.*, the apex becomes initially *negative*. In other respects the excursion is unaltered. Consequently, if at the moment of excitation the apex is warmed in the manner previously described in 12, the large single excursion (apex-positive) is seen to be preceded by a momentary præ-excursion in the opposite direction.

III. "Contributions to the Anatomy of the Central Nervous System in Vertebrate Animals. Part I. Ichthyopsida. Section 1. Pisces. Subsection 1. Teleostei." By Alfred Sanders, M.R.C.S. Communicated by Professor Huxley, Sec. R.S. Received May 7, 1878.

(Abstract.)

The brain of *Mugil cephalus* consists of three pairs and one unpaired tuberosity above, and two below.

The most anterior pair are the olfactory lobes. From the anterior to posterior end they present four layers; first, olfactory nerve fibres with cell-like swellings upon them; second, coarsely granular neuroglia, with incipient glomeruli olfactorii, and large tripolar nerve cells; third, small usually unipolar cells each in its own space in the neuroglia; the whole collected into a rounded mass; fourth, nerve fibres proceeding from this mass to the second pair of tuberosities, the cerebral lobes, which consist of finely granular neuroglia, in which small cells are situated towards the circumference, and larger cells towards the centre, each of the latter contained in a lymph space.

The third pair of tuberosities, the optic lobes, corresponding with the corpora quadrigemina, are formed of a thin layer of nervous substance, enclosing a ventricle; this layer, the *tectum*, consists of seven strata: first, granular neuroglia; second, oblique fibres with fusiform cells; third, radiating fibres; fourth, oblique fibres; fifth, transverse fibres; sixth, cells of a small size; seventh, connective tissue with an epithelial lining to the ventricle of this lobe. Within this ventricle, on the floor, are two tuberosities, tori semicirculares, faced with connective tissue, and containing small unipolar cells arranged along the margin. Behind, the ventricle in question is closed by a process from the unpaired tuberosity (cerebellum), which occupies the position of the valve of Vieussens; this is the valvula cerebelli; it forms a fold in the ventricle, and joins a longitudinal ridge situated along the contiguous inner margins of the tecta; on the side of the valvula are processes more or less developed in different fishes.

The unpaired tuberosity is the cerebellum; it has a structure comparable to that of the cerebellum of the newly-born human infant.

The tuberosities below are the hypoaria, which receive fibres from the ventral surface of the medulla oblongata behind, and from the cerebral lobes in front. Between the two hypoaria is the pituitary body, and also a saccus vasculosus.

The medulla oblongata behind the cerebellum has two tuberosities, constructed in the same way as the tori semicirculares; they are the vagal tuberosities.

Ventricles :—

The fourth ventricle consists of two parts, one part beneath the cerebellum, the other part, a deep fissure between the vagal tuberosities; they communicate with each other by means of a narrow quadrangular passage.

The ventricle of optic lobe is simply the remains of the ventricle of the corpora quadrigemina in fœtus, and now part of the aqueduct of Sylvius.

The third ventricle, between the cerebral lobes and the optic lobe, communicates behind with the last, and below with pituitary body by means of the infundibulum; above, it is closed in by the stalk of the pineal gland.

Origins of nerves :—

The olfactory nerve arises from the olfactory lobe.

The optic nerve arises by three roots, first from the tectum lobi opici, second from the torus semicircularis, third from the hypoarium.

The motor oculi arises from a ganglion beneath the floor of the aquæductus sylvii; it partially decussates.

The trochlearis arises from anterior part of the base of the cerebellum.

The trifacial arises by three roots from beneath the anterior end of the crura cerebelli; its deep origin is from three distinct points; first, from cells beneath the floor of anterior part of the fourth ventricle; second, from the external lateral part of the spinal cord; third, from the central part of the vagal tuberosity.

The abducens arises from two small ganglia in the ventral horn of grey matter beneath the narrow part of the fourth ventricle.

The acusticus arises from the lateral part of the medulla oblongata in the region of the ganglion of the vagus.

The glossopharyngeal has a separate origin in mugil, in other fishes it is a branch of the acusticus, in others again it is a branch of the vagus; here it arises from the grey matter covering the narrow part of the fourth ventricle.

The vagus arises by two roots, one from a ganglion beneath the floor of posterior end of the fourth ventricle; the other from the cerebellum, and from the grey matter covering the narrow part of the fourth ventricle.

The first spinal nerve, although passing through a foramen in the exoccipal, takes the place of the hypoglossal, rises by dorsal and ventral roots, forms with the second spinal nerve the brachial plexus, and supplies the region of the tongue.

The spinal nerves have a ganglion on the dorsal root.

The commissura ansulata, placed at the exit of the trochleares nerves, is partly a longitudinal and partly a transverse commissure; it first appears on the external and lateral part of the medulla, opposite the

posterior part of the fourth ventricle, passes forward, and at the point above-named ascends on the external edge of the medulla, and forms the commissure of the tecta lobi optici, at the base of their longitudinal ridges above described; this commissure also forms a decussation of fibres beneath the ganglion of the trochlearis.

The anterior commissure of the brain is situated between the cerebral lobes.

The posterior commissure is situated on the anterior part of the floor of the ventricle of the optic lobe behind the third ventricle.

A deeper commissure connects the region on each side of the third ventricle passing through the hypoaria in front of the infundibulum.

Two transverse commissures exist in the spinal cord, one ventral, one dorsal.

The ventral longitudinal columns of the cord contain two fibres of gigantic size, one on each side; these decussate opposite the origin of the trifacial on the floor of anterior part of the fourth ventricle; the longitudinal columns pass to the internal part of the floor of ventricle of the optic lobe, about the posterior end of the fourth ventricle.

The lateral columns of the cord pass forward, and are lost outside the last.

IV. "On the Equations of Circles." (Second Memoir.) By JOHN CASEY, LL.D., F.R.S., M.R.I.A., Professor of Mathematics in the Catholic University of Ireland. Received May 10, 1878.

(Abstract.)

In the year 1866 was published in the "Proceedings of the Royal Irish Academy" a paper "On the Equations of Circles," which contained extension of many known theorems. Thus it was proved in it that the same forms of equation which are true for a circle inscribed in a plane or spherical triangle hold also when the right lines in the one case, or the great circles in the other, are replaced by any three circles in the plane or sphere, and it was shown that the transformed equations represented the pairs of circles which touch the three given circles. The results for circles on the sphere were still further extended, namely, to conics having double contact with a given conic. The paper contained, in addition to these fundamental investigations, many collateral ones on allied subjects.

The memoir, of which I now give an abstract, extends the results of the foregoing paper to a polygon of any number of sides inscribed or circumscribed to a given circle. It is proved for the case of circumscribed figures, that the sides of the polygon may be replaced both on the plane and sphere, by circles touching the given circle; and

again, these results may be still further extended to conics having double contact with a given conic. A very large amount of geometry is embraced in the paper, and many subjects of much interest are discussed, showing the great fertility of the methods of investigation employed.

The following is an outline of the extensions of known theorems which the paper contains.

If a polygon of n sides, whose equations are α, β, γ, δ, &c., and whose lengths are a, b, c, d, &c., be inscribed in a circle, the equation of the circle is a factor in the equation $\frac{\alpha}{a}+\frac{b}{\beta}+\frac{c}{\gamma}+\frac{d}{\delta}+$ &c., $=0$, or say in

$$\Sigma\left(\frac{\alpha}{a}\right)=0,$$

and if it be on the sphere in the equation,

$$\Sigma\left(\frac{\tan\frac{1}{2}\alpha}{a}\right)=0.$$

The equation $\Sigma\left(\frac{\alpha}{a}\right)=0$, when the polygon consists of n sides, will denote besides the circle a residual curve of the degree $(n-3)$. A considerable portion of the paper is occupied with the discussion of the properties of this curve. More especially in the particular case $n=6$, where the residual curve is a cubic. The following theorem may be given as an instance. "When the polygon is a hexagon the line $\frac{\alpha}{a}+\frac{\beta}{b}+\frac{\gamma}{c}+\frac{\delta}{d}+\frac{\epsilon}{e}+\frac{\theta}{f}=0$, which is called its axis, is the satellite with respect to the cubic of each side of the hexagon, and also of its Pascal's line."

The properties of the inscribed polygon derived from the equations

$$\Sigma\left(\frac{\alpha}{a}\right)=0,$$

$$\Sigma\left(\frac{\tan\frac{1}{2}\alpha}{a}\right)=0,$$

may be reciprocated, and we get tangential equations of the form

$$\Sigma\left(\frac{\cot\frac{1}{2}\mathrm{A}}{\lambda}\right)=0,$$

giving properties of circumscribed polygons. In this last equatio A, B, C, &c., denote the angles of the polygon, and λ, μ, ν, &c., perpendiculars from its angular points on any tangent to the circle. The same tangential equation holds both for the plane and sphere.

If a polygon whose sides are α, β, γ, δ, &c. . . . ω, be circumscribed to a circle, the equation of the circle is a factor in the polyzomal curve

$$\frac{\cos\frac{1}{2}(a\beta)}{\sqrt{a\beta}}+\frac{\cos\frac{1}{2}(\beta\gamma)}{\sqrt{\beta\gamma}}+\frac{\cos\frac{1}{2}(\gamma\delta)}{\sqrt{\gamma\delta}}+\text{ \&c.,}$$

$$+\frac{\cos\frac{1}{2}(\omega a)}{\sqrt{\omega a}}=0.\text{"}$$

I have examined this equation at length in the special case $n=4$. The tetrazomal curve, which is of the eighth degree, breaks up into two factors. One factor represents the circle and the square of the line gaining the points (γ), $(\beta\delta)$, the other denotes an unicursal quartic whose three double points are $(a\gamma)$ $(\beta\delta)$, and the pole with respect to the circle of the line joining these points.

These results hold for the plane and sphere, and a, β, γ, δ, &c., may denote circles as well as lines. They are also true for conics having double contact with a given conic.

Besides the foregoing, which are manifestly extensions of known theorems, the paper contains some original theorems which are also extended to conics having double contact with a given conic. Thus, "if a circle Σ be touched by any number of circles S_1 S_2 S_3, &c., and if we denote by $P(i)$ the product of the common tangents drawn from any circle S_i of the system to all the remaining circles, then the equations of Σ will be a factor in the polyzomal curve,

$$\frac{\sqrt{S_1{}^{n-2}}}{P(1)}-\frac{\sqrt{S_2{}^{n-2}}}{P(2)}+\frac{\sqrt{S_3{}^{n-2}}}{P(3)}-\text{ \&c. }=0.$$

This theorem is, I believe, one of the most fertile in geometry. The paper contains a large number of deductions from it, but I am certain it is far from exhausting them.

V. "On the Bodily Tides of Viscous and Semi-Elastic Spheroids, and on the Ocean Tides on a yielding nucleus." By George H. Darwin, M.A., Fellow of Trinity College, Cambridge. Communicated by J. W. L. Glaisher, M.A., F.R.S. Received May 14, 1878.

(Abstract.)

Sir W. Thomson's investigation of the bodily tides of an elastic sphere* has gone far to overthrow the idea of a semi-fluid interior to the earth, yet geologists are so strongly impressed by the fact that enormous masses of rock have been poured out of volcanic vents in

* Phil. Trans., 1863, p. 573, and Thomson and Tait's Natural Philosophy, edit. of 1867, §§ 733–737 and 834–846.

the earth's surface, that the belief is not yet extinct that we live on a thin shell over a sea of molten lava. It appeared to me, therefore, to be of interest to investigate the consequences which would arise from the supposition that the matter constituting the earth is of a viscous or imperfectly elastic nature. In this paper I follow out these hypotheses, and it will be seen that the results are fully as hostile to the idea of any great mobility of the interior of the earth as are those of Sir W. Thomson.

I begin by showing that the equations of flow of an incompressible viscous fluid have precisely the same form as those of strain of an incompressible elastic solid, at least when inertia is neglected. Hence, every problem about the strains of the latter has its analogue touching the flow of the former. This being so, the solution of Sir W. Thomson's problem of the bodily tides of an elastic sphere may be adapted to give the bodily tides of a viscous spheroid. Sir W. Thomson, however, introduces the effects of the mutual gravitation of the parts of the sphere, by a synthetical method, after he has found the state of internal strain of an elastic sphere devoid of gravitational power The parallel synthetical method becomes, in the case of the viscous spheroid, somewhat complex, and I have preferred to adapt the solution analytically so as to include gravitation.

The solution is only applicable when the disturbing potential is capable of expansion as a series of solid harmonics, and it appears that each harmonic term in the potential acts independently of all others; it is thus only necessary to consider a typical term in the potential.

It is shown finally that if ρ, ϖ, ν, be the velocities of the fluid (at a point whose polar co-ordinates in the sphere are r, θ, ϕ,) radially, and along and perpendicular to the meridian; μ the coefficient of viscosity; $wr^i\mathrm{S}_i$ the disturbing potential, a solid harmonic of degree i; a the mean radius, w the density of the homogeneous spheroid, and g gravity; $r = a + \sigma_i$ the equation to the bounding surface of the spheroid; then

$$\rho = \frac{i^2(i+2)a^2 - i(i^2-1)r^2}{2(i-1)[2(i+1)^2+1]\mu} r^{i-1}\mathrm{T}_i,$$

$$\varpi = \frac{i(i+2)a^2 - (i-1)(i+3)r^2}{2(i-1)[2(i+1)^2+1]\mu} \times r^{i-1}\frac{d\mathrm{T}i}{d\theta},$$

$$\nu = \qquad \text{the same} \qquad \times \frac{r^{i-1}}{\sin\theta}\frac{d\mathrm{T}_i}{d\phi},$$

where $\mathrm{T}_i = w\left(\mathrm{S}_i - 2g\frac{i-1}{2i+1}\frac{\sigma_i}{a^i}\right)$, a surface harmonic of order i. A differential equation is then found, which gives the form of the free surface at any time under the action of any disturbing potential

which satisfies the condition of expansibility as a series of solid harmonics.

When the disturbing potential $wr^i S_i$ is zero, and when $r=a+s_i$ is the equation to the free surface initially, then the equation to the surface at the time t is given by $r=a+\sigma_i$, where

$$\sigma_i = s_i \exp.\left(-\frac{gwai}{2(i+1)^2+1}t\right).*$$

This gives the law of the subsidence of inequalities on the surface of a viscous globe under the influence of simple gravitation; and it is suggested that some light may possibly be thrown thereby on the laws of geological subsidence and upheaval. It appears from this formula that inequalities of wide extent will subside much more quickly than wrinkles.

The rate is found at which a rotating spheroid would adjust itself to a new form of equilibrium, when its axis of figure is not coincident with that of rotation; and the law is established which was assumed in a former paper.†

The case is next considered where S_i is a surface harmonic of the second order, multiplied by a simple time harmonic—that is to say, $S_i = S\cos(vt+\eta)$. This is the assumption appropriate for the tidal problem. The forces in this case do not form a rigorously equilibrating system; but there is a couple of the second order of small quantities called into existence, the consideration of which is deferred to a future paper.

It is then shown that if $wr^2 S\cos(vt+\eta)$ be a term in the tide-generating potential, and if $\tan\epsilon = \frac{19\mu v}{2gwa}$, the tide of the viscous spheroid is equal in height to the equilibrium tide of a perfectly fluid spheroid multiplied by $\cos\epsilon$, and the tide is retarded by $\epsilon \div v$. It is next proved that the equilibrium tide of a shallow ocean overlying the nucleus is equal to the like tide on a rigid nucleus multiplied by $\sin\epsilon$, and that there is an acceleration of the time of high water equal to $\frac{\pi}{2v} - \frac{\epsilon}{v}$.

This theory is then applied to the lunar semi-diurnal and fortnightly tides, and tables are given from which the following is extracted—the coefficient of viscosity being expressed in gramme-weights, centimeters, and seconds.

* I write "exp." for "e to the power of."

† "On the Influence of Geological Changes on the Earth's Axis of Rotation." Phil. Trans., vol. clxvii, Pt. I, p. 282. I take this opportunity of correcting a slight mistake in that paper; the formula in the fourth line from the bottom of p. 301, should run $\frac{D}{2d\rho hc^4} = \frac{E}{2e\rho hc^4} = -\frac{1}{qc}\cot qc$ + &c. The mistake arose in copying out the formula, and does not affect the subsequent arithmetical results.

Lunar Semi-diurnal Tide.

Coefficient of viscosity $\times 10^{-10}$.	Retardation of bodily tide.	Height of bodily tide is tide of fluid spheroid multiplied by	Height of ocean tide is tide on rigid nucleus multiplied by	Acceleration of high water of ocean tide.
Fluidity 0	0	1·000	000	3 hrs. 6 min.
96	41 min.	·940	·342	2 hrs. 25 min.
721	2 hr. 25 min.	·342	·940	41 min.
Rigidity ∞	3 hr. 6 min.	·000	1·000	0

Fortnightly Tide.

1,200	9 hrs.	·985	·174	3 days 1 hr.
12,000	2 days 6 hrs.	·500	·866	1 day 3 hrs.
Rigidity ∞	3 days 10 hrs.	·000	1·000	0

A comparison of the numbers in the first column with the viscosity of pitch at near the freezing temperature (when I found by rough experiments that its viscosity was about $1{\cdot}3 \times 10^8$), shows how enormously stiff the earth must be to resist the tidally distorting influence of the moon. It may be remarked that pitch at this temperature is hard, apparently solid and brittle; and if the earth was not very far stiffer than pitch, it would comport itself sensibly like a perfect fluid, and there would be no ocean tides at all. It follows, therefore, that no very considerable portion of the interior of the earth can even distantly approach the fluid condition.

This does not, however, seem conclusive against the existence of bodily tides in the earth of the kind here considered; for, under the enormous pressures which must exist in the interior of the earth, even the solidest substances might be induced to flow to some extent like a fluid of great viscosity.

The theory of the bodily tides of an "elastico-viscous" spheroid is next developed. The kind of imperfection of elasticity considered is where the forces requisite to maintain the body in any strained configuration diminish in geometrical progression, as the time increases in arithmetical progression. There are two constants which define the mechanical nature of this sort of solid: first, the coefficient of rigidity n, at the instant immediately after the body has been strained; and second, "the modulus of the time of relaxation of rigidity" t, which is the time in which the force requisite to maintain the body in its strained position has diminished to e^{-1} or ·368 of its initial value. I am not aware that there is any experimental justification for the assumption of such a law; but after considering the various physical

objections which may be raised to it, I came to the conclusion that the investigation was still of some value.

The equations of flow of such an ideal solid have been given (with some assistance from Professor Maxwell) by Mr. Butcher* and they are such that, if the body be incompressible, and if inertia be neglected, they may be written in exactly the same form as the equations of flow of a purely viscous fluid, the coefficient $n\left(\frac{1}{\mathbf{t}}+\frac{d}{dt}\right)^{-1}$ merely replacing the coefficient of viscosity. Hence it follows that the solution previously found may be at once adapted to the new hypothesis.

In the application to the tidal problem, if the tide-generating potential be as before, $wr^2\mathrm{S}\cos(vt+\eta)$, and if $\tan\psi=v\mathbf{t}$, $\tan\chi=v\mathbf{t}\left(1+\frac{19n}{2gwa}\right)$, and $\tan\epsilon=\tan\chi-\tan\psi$, it appears that the bodily tide raised by this potential is equal to the corresponding tide of a perfectly fluid spheroid multiplied by $\frac{\cos\chi}{\cos\psi}$, and the tide is retarded by a time $\frac{\chi-\psi}{v}$. Also the equilibrium tide of a shallow ocean overlying the elastico-viscous nucleus is equal to the corresponding tide on a rigid nucleus multiplied by $\cos\chi\tan\epsilon$, and there is an acceleration of the time of high water equal to $\frac{\pi}{2v}-\frac{\chi}{v}$.

If $\mathbf{t}$ be taken as zero, whilst n is infinite, but $n\mathbf{t}$ (the coefficient of viscosity) finite, the solution becomes that already found for a purely viscous spheroid. If, on the other hand, $\mathbf{t}$ be infinite, the solution is that of Sir W. Thomson's problem of the purely elastic sphere. This hypothesis is therefore intermediate between those of pure viscosity and pure elasticity.

Sir William Thomson worked out numerically the bodily tides of elastic spheres with the rigidities of glass and of iron; and tables of results are here given for those rigidities, with various times of relaxation of rigidity, for the semi-diurnal and fortnightly tides.

It appears that if the time of relaxation of rigidity is about one quarter of the tidal period, then the reduction of ocean tide does not differ much from what it would be if the spheroid were perfectly elastic. The acceleration of high tide, however, still remains considerable; and a like observation may be made in the case of pure viscosity approaching rigidity. This leads me to think that one of the most promising ways of detecting such tides in the earth, would be by the determination of the periods of maximum and minimum in a tide of long period in a high latitude. But I am unfortunately unacquainted with practical tidal observation, and therefore cannot tell how far it would be possible to carry out this suggestion.

* Proc. Lond. Math. Soc., Dec. 14, 1876, pp. 107–9.

It is then shown that the effects of inertia, which had been neglected in finding the laws of the tidal movements, cannot be such as to materially affect the accuracy of the results.

In the first part of this paper I followed Sir W. Thomson in using the equilibrium theory for the determination of the amount of reduction of ocean tides. But that theory is acknowledged on all hands to be very faulty in its explanation of tides of short period; hence a dynamical investigation of the effects of a bodily yielding of the earth on a tide of short period in a shallow equatorial canal appeared likely to be interesting. This investigation is carried out in the second part of the paper. The problem is simplified by supposing the circular canal developed into a straight canal, whose bottom is constrained to execute a simple harmonic wave motion.

The result shows that the height of the ocean tide relatively to the nucleus bears the same relation to the height of tide on a rigid nucleus as in the equilibrium theory, and that the alteration of phase is the same. This seems to increase the force of Sir W. Thomson's argument as to the rigidity of the earth.

The chief practical result of this paper may be summed up by saying, that it is strongly confirmatory of the view that the earth has a very great effective rigidity; but its chief value is, that it forms a necessary first chapter to the investigation of the precession of viscous and imperfectly elastic spheroids—an investigation which I hope to complete very shortly.

VI. "On the Formation of Chlor-iodide and Brom-iodide of Ethylidene." By Dr. Maxwell Simpson, F.R.S., Professor of Chemistry, Queen's College, Cork. Received May 7, 1878.

(Preliminary Notice.)

Chlor-iodide of ethylidene $\begin{matrix} CH_3 \\ | \\ CHClI \end{matrix}$. This body I have succeeded in preparing by two processes.

First Process.—A quantity of iodide of ethylidene, which had been prepared by Gustavson's method, and heated to 160° C. but not distilled, was vigorously agitated for some time with a weak solution of chloride of iodine without the application of heat. The excess of chloride was then poured off, and the product well washed with dilute potash and distilled. Almost the entire quantity passed over between 110 and 150° C. This yielded, on fractioning, a large quantity of fluid boiling between 116 and 120° C., most between 117 and 119.° This was the body in question. The chloride of iodine used in this process was

prepared by passing washed chlorine into 4 oz. of water, holding 400 grains of iodine in suspension, till *almost* all the iodine was dissolved. The vessel containing the iodine must be surrounded with cold water, and repeatedly shaken during the passage of the gas.

This method is quite analogous to that by which I obtained its isomer, the chlorio-dide of ethylene.*

Second Process.—One molecule of iodide of aluminium (Al_2I_6) was dissolved in three times its weight of dry carbon disulphide, and added drop by drop without exposure to air to six molecules of chloride of ethylidene ($C_2H_4Cl_2$) diluted with an equal volume of the disulphide. The chloride of ethylidene must be surrounded with ice, and kept in a state of continual agitation during the addition of the aluminium iodide solution. By mixing the reacting bodies in this way, the chloride is always in excess, and only one atom of chlorine in each molecule is supplanted by one of iodine. After the addition of the aluminium iodide, the product was filtered through asbestos, washed with water, and heated in a water-bath to drive off the carbon disulphide.† The residue, which had been previously washed with dilute potash, on being heated above 100°, commenced to distil at 110°, and between that temperature and 155° C. about two-thirds of it passed over. (The liquid distilling above 155° was iodide of ethylidene). This yielded, on fractioning, a large quantity of an oil boiling at the same temperature, and having the same properties as that prepared by the first process. On submitting this body to analysis I obtained the following numbers:

	Theory C_2H_4ClI.	Experiment.
Carbon	12·60	12·92
Hydrogen	2·10	2·23

Chlor-iodide of ethylidene has a sweet taste, and is almost colourless when freshly prepared. Its specific gravity at 19° C. is = 2·054. It distils without decomposition between 117 and 119° C. It will be observed that its boiling point is 20 degrees lower than that of its isomer, the chlor-iodide of ethylene (137° C.), and is intermediate between those of the iodide (177° C.) and chloride of ethylidene (58° C.).

Of these processes, the second is easier of execution and yields a larger product.

Brom-iodide of ethylidene $\begin{matrix} CH_3 \\ | \\ CHBrI \end{matrix}$. This body I obtained also from the iodide of ethylidene, and by a process almost identical with the

* Proc. Roy. Soc., xi, 590.

† It is advisable to use Würtz's tube with two bulbs in distilling the carbon disulphide, as a large quantity of the chlor-iodide passes over with the disulphide when the distillation is conducted in the usual way.

first used for the preparation of chlor-iodide of ethylidene. The iodide was agitated in the cold for some time with a weak solution of bromide of iodine,* the excess of bromide was separated, and the product was washed with dilute potash. On subjecting this to distillation almost the entire quantity passed over between 130 and 165° C. On fractioning I obtained a large quantity of fluid distilling between 140 and 148°, most between 142 and 144°. The following equation explains the formation of this compound:

$$\begin{array}{l} CH_3 \\ | \\ CHI_2 \end{array} + BrI = \begin{array}{l} CH_3 \\ | \\ CHBrI \end{array} + H.$$

On analysing this body I obtained the following results:

	Theory C_2H_4BrI.	Experiment.
Carbon	10·21	10·24
Hydrogen	1·70	1·82

Brom-iodide of ethylidene is nearly colourless when freshly prepared. It has a sweet taste, and distils without decomposition between 142 and 144° C. This is about 20 degrees lower than the boiling point of its isomer, the brom-iodide of ethylene (163° C.). It refuses to become solid even when surrounded with a mixture of ice and salt, differing in this respect also from its isomer. Heated with alcoholic potash it yielded a volatile vapour containing bromine, probably bromide of vinyl, and iodide of potassium.

This body is probably identical with those obtained by Pfaundler† and by Reboul‡ by exposing bromide of vinyl to the action of hydriodic acid. The boiling points of their compounds agree pretty well with each other and also with mine.

I am at present engaged in studyiug the behaviour of these compounds towards several reagents.

I have to thank my young pupil, Mr. Harrington, for his valuable assistance during the progress of this research.

VII. "Note on the Specific Gravity of the Vapours of the Chlorides of Thallium and Lead." By Henry E. Roscoe, F.R.S., Professor of Chemistry in Owens College, Manchester. Received May 8, 1878.

Experimental difficulties of so serious a nature surround the attempt

* For the preparation of the bromide of iodine, see Proc. Roy. Soc., No. 149, 1874. It was made a little weaker for this process.

† "Jahresbericht," 1865, p. 483.

‡ *Ibid.*, 1870, p. 439.

to ascertain the specific gravity of vapours at a high temperature that, in spite of the interest which attaches to this subject, but few additions have been made in our knowledge in this direction since the researches of Deville and Troost.

The present experiments, of which this notice contains the first results, have been made with the object of so simplifying the process as to render it easy to determine the specific gravity of the vapours of bodies possessing high boiling-points with a degree of accuracy sufficient for the purpose of controlling their molecular weights.

The method consists in vaporizing the substance under examination in long-necked glazed porcelain globes of known capacity placed in a muffle raised to bright redness. The temperature of the globe is ascertained by a calorimetric determination made with heavy platinum weights placed in the muffle, this determination being checked by the simultaneous insertion in the muffle of a second globe containing mercury.

The porcelain globes having a capacity of about 300 cub. centims., and containing from 3 to 9 grams of substance, are closed by loosely fitting stoppers of baked clay, and then gradually introduced in the muffle. After remaining there until no further escape of vapour is observed, and until the temperature has become constant, the globes are quickly withdrawn from the muffle and their contents removed and analysed, the temperature being in each case ascertained by the calorimetric method at the time of withdrawal of the globe. The following determinations of the specific gravity of mercury vapour serve to show the reliability of the method:

	Temperature determined calorimetrically.	Specific gravity of mercury vapour.
Experiment I.....	1019°	6·92
„ II.....	894°	6·75
„ III.....	815°	6·91
„ IV.....	972°	5·77
„ V.....	1047°	7·05

the calculated specific gravity (Hg=198·8) being 6·728.

Before determining the specific gravity of the vapour of thallium chloride it was ascertained that this compound does not give off free chlorine when volatilized at a red-heat, and that the sublimate contains thallium and chlorine in the atomic ratio of equality.

In each experiment the total amount of thallium and of chlorine remaining in the globe was determined by analysis, and the specific gravity calculated from their sum.

	Temperature determined calorimetrically.	Specific gravity of the vapour of thallium chloride.
Experiment I.	859°	8·15
„ II.	828°	8·28
„ III.	1015°	8·06
„ IV.	859°	7·43
„ V.	1026°	8·75
„ VI.	852°	8·60
„ VII.	837°	7·84

The specific gravity of thallium chloride vapour calculated upon the supposition that the molecular weight of the compound is 238·07, and its formula TlCl, is 8·49.

Four determinations of the specific gravity of mercury vapour made simultaneously with four of the above experiments gave as a mean the number 6·0 instead of 6·728.

The specific gravity of the vapour of lead chloride was made in a similar way, but the temperature required for complete volatilization is much higher than that needed in the case of the last compound. The residue left in the globes was completely soluble in hot water, and contained lead and chlorine in the proportion of one atom of the former to 2·08 of the latter.

	Temperature determined calorimetrically.	Specific gravity of the vapour of lead chloride.
Experiment I.	1046°	9·12
„ II.	1089°	9·72
„ III.	1077°	9·51
„ IV.	1070°	9·64

The specific gravity calculated from the formula $PbCl_2$=277·14 is 9·62.

I hope before long to be able to lay before the Society the results of specific gravity determinations of the vapours of other compound and elementary bodies, together with the whole of the experimental details.

VIII. "Extract from Report to Professor Sir Wyville Thomson, F.R.S., Director of the Civilian Scientific Staff, on the Brachiopoda dredged by H.M.S. 'Challenger.'" By Thomas Davidson, F.R.S. Received May 8, 1878.

At the request of Professor Sir Wyville Thomson, I have undertaken the examination, description, and illustration of the Brachiopoda dredged by the 'Challenger' Expedition.

Very little seems to have been known with respect to recent Brachi-

opoda before the middle of last century; and even during the period extending from 1750 to 1800, the information relating to the recent species was, with some exceptions, meagre and often unsatisfactory.

A Brachiopod, until within the last fifty years, was considered a great rarity in all collections, and no one could boast of possessing more than a very limited number of species and specimens. Much uncertainty was also felt as to their nature, and the position they should occupy among the Invertebrata. They were generally supposed to be referable to the genus *Anomia*, and were very quaintly described by some of the earlier naturalists. Several more serious and better informed observers appeared soon after 1753, such as Linné, Pennant, Müller, Lamanon, Pallas, Grundler, and a few others, who were able, in a measure, to prepare the way for the important discoveries reserved for the more favoured naturalists of the present century.

The animal of the Brachiopod had attracted the attention of Pennant in 1766, Grundler in 1774, Müller in 1776, Poli in 1791, Lamanon and Cuvier in 1797; but no regular anatomical dissections had been executed, and their observations seemed, in a great measure, to be limited to the labial appendages, mantle, and some other minor details.

This most important inquiry was, however, subsequently admirably followed out by such excellent zoologists and anatomists as Cuvier, Owen, Huxley, Vogt, Macdonald, Hancock, Gratiolet, Lacaze-Duthiers, King, E. Deslongchamps, and others, to whose works the reader is referred, as it would not be possible in the limited space devoted to the description of the species dredged in the 'Challenger' Expedition, to write a treatise on the history of the class, nor to refer to the fossil genera and species, which vastly outnumber those inhabiting the present seas.

Restricting ourselves, therefore, to the recent species, we may observe that the correct knowledge we now possess with respect to their geographical and bathymetrical distribution is mainly due to the numerous Governmental and private dredging expeditions carried out during the last forty years. Before that period, very few reliable data were in our possession; and as these dredging expeditions proceed, the more will our knowledge become extended.

The admirable report of Professor E. Forbes, "On the Mollusca and Radiata of the Ægean Sea," published by the British Association for the Advancement of Science, for 1843, shadowed forth the important results that might be obtained by well conducted and equipped Expeditions; but even now we are wanting in information with respect to the bathymetrical distribution of some twenty-six of the known living species.

In his excellent memoir, "Uber die Wohnsitze der Brachiopoden,"

1859, Professor E. Suess recapitulates all the then known data respecting the geographical distribution and depths at which Brachiopoda live; since then, our knowledge has been very considerably extended; and it has become evident from direct observation that the Brachiopoda are widely but sparingly distributed over the depths of the sea, though of course they are more numerous both in species and individuals at depths of less than 500 fathoms. They are much localised, and prefer rocky, stony, and coralline sea bottoms, to soft or muddy ones.

The entire collection of Brachiopoda brought home by the 'Challenger' Expedition, numbering several hundred specimens, was placed in my hands by Sir Wyville Thomson, on the 11th of August, 1877. The specimens were in an excellent state of preservation, and had been put into bottles of spirits, with a correct indication of the stations, latitude, longitude, depth, bottom-temperature, and the nature of the sea-bed whence they had been obtained. Thus, reliable and invaluable data accompanied each specimen, which I have in every instance reproduced in the pages relating to this Expedition. Sometimes but one specimen had been dredged at a station, while at other times two or more species or specimens were brought to the surface. It must, however, be noted that, in addition to the 361 dredging stations included in the printed instructions, the naturalists of the 'Challenger' dredged very often in shallow water.

We learn from the "list of observing stations," recorded during the voyage of 68,890 miles, that there were 354 of such stations, at most of which "a fair sample of the bottom fauna was collected by means of the dredge or trawl." But Brachiopoda do not appear to have been obtained more than thirty times. Although the number of specimens was large, they represent 28 or 29 species only. The greatest depth at which any living Brachiopod was obtained was 2,600 fathoms, the greatest depth dredged being, on one occasion, 4,575 fathoms.

The ranges of depth at which the 'Challenger' species of Brachiopoda occurred, were as follows :—

Shore or low water to 10 fathoms.	Waldheimia flavescens. *Lamark.*
„	Kraussina Lamarckiana. *Davidson.*
„	Lingula anatina. *Lam.*
„	Megerlia sanguinea. *Chemnitz.*
5 to 15 fms.	Magasella flexuosa. *King.*
25 „ 30 „	Terebratella dorsata. *Gmelin.*
38 „ 49 „	Terebratulina cancellata. *Koch.*
49 „	Discina stella. *Gould.*
51 „ 150 „	Terebratulina caput-serpentis, var. septentrionalis. *Couthouy.*
70 „ 75 „	Megerlia truncata. *Linné.*
70 „ 75 „	Argiope decollata. *Chemn.*
82 „	Terebratulina sp. (?) Philippines.

82	to 102	fms.	Terebratella sp. (?) perhaps T. Frieli. *Dav.*
100	150	„	Platydia anomioïdes. *Scacchi.*
100	„ 150	„	Waldheimia Kerguelensis. *Dav.*
120		„	Megerlia Willemoesi. *Dav.*
120	„ 600	„	Terebratula uva. *Brod.*
150		„	Terebratula vitrea, var. minor. *Philippi.*
150		„	Kraussina pisum. *Lam.*
150		„	Rhynchonella nigricans, var. pixidata. Willemoes Suhm M.S.
210		„	Terebratula Moseleyi. *Dav.*
350		„	Terebratulina Cailleti. *Crosse.*
390		„	Magasella, sp.
390		„	Terebratulina Wyvilli. *Dav.*
420		„	Terebratula Cubensis. *Pourtales.*
600		„	Terebratulina Murrayi. *Dav.*
1,340		„	Terebratella Frieli. *Dav.*
1,875		„	Terebratula or Terebratulina (?) Dalli. *Dav.*
1,850		„	Megerlia (?) incerta. *Dav.*
1,850	to 2,180	„	Discina Atlantica. *King.*
1,035	„ 2,600	„	Terebratula Wyvilli. *Dav.*
1,900		„	Terebratula ? (undeterminable fragments).
2,160		„	Waldheimia Wyvilli. *Dav.*

The following table shows approximately how many times Brachiopoda were dredged at certain depths:—

99	dredgings in depths of from		1	to 500	fms.	21 times.
30	„	„	501	„ 1,000	„	4
47	„	„	1,001	„ 1,500	„	3
47	„	„	1,501	„ 2,000	„	4
92	„	„	2,001	„ 2,500	„	1
83	„	„	2,501	„ 3,000	„	1 (at 2,600 fms.)
5	„	„	3,001	„ 3,500	„	none
5	„	„	3,501	„ 4,000	„	none
none	„	„	4,001	„ 4,500	„	none
1	„	„	4,501	„ 4,575	„	none

Thus, it becomes apparent that Brachiopoda do not generally abound in depths exceeding 500 or 600 fathoms, or less; but out of 125 dredgings, in depths of from 1 to 600 fathoms, Brachiopoda were brought up twenty-one times only; while in depths varying from 600 to 2,600 fathoms, Brachiopoda were obtained ten or eleven times. The depths for some of the species, however, varied in localities not visited by the 'Challenger,' as recorded in the general list.

In order to obtain some approximate data as to the depths Brachiopoda are at present known to inhabit, it will be desirable to append a list of all the known recent species, with an indication of all their respective depths.

A point of interrogation has been placed before uncertain species, or not sufficiently determined species or varieties; and an asterisk before those dredged in the 'Challenger' Expedition.

CLISTENTERATA. *King.*

Depths in fathoms hitherto ascertained.†			
5 to 1,456	*Terebratula* vitrea. *Born.*		
292 „ 994	„ „ var. sphenoïdea. *Phil.*		
40 „ 1,000*	„ „ „ minor. *Philippi.*		
55 „ ?	„ „ „ Davidsoni. *Adams.*		
(?) „ ?	„ „ „ Cernica. *Crosse.*		
100 „ 420*	„ Cubensis. *Pourtales.*		
210 „ *	„ Moseleyi. *Dav.*		
500 „ 600	„ subquadrata. *Jeffreys.*		
10 „ 600	„ uva. *Broderip.*		
1,035 „ 2,600*	„ Wyvilli. *Dav.*		
1,875 „ *	„ Dalli. *Dav.*		
(?) „ ?	? Malvinæ. *D'Orb.*		
390 „ *	*Terebratulina* Wyvilli. *Dav.*		
30 „ 40*	„ cancellata. *Koch.*		
(?)	„ radiata. *Reeve.*		
46 „ 50	„ (Agulhasia) Davidsoni. *King.*		
0 „ 1,180	„ caput-serpentis. *Linné.*		
51 „ 130*	„ „ „ „ var. septentrionalis. *Couthouy.*		
50 „ 130	„ „ „ „ var. Mediterranea. *Jeffr.*		
82 „ *	„ undetermined (Philippine Islands).		
55 „ 58	„ Japonica. *Sowerby.*		
26 „ 63?	„ Cumingi. *Dav.*		
500 „	„ trigona. *Jeffr.*		
340 „ 795	„ tuberata. „		
70 „ 471*	„ Cailleti. *Crosse.*		
Low water „ 100	„ unguiculata. *Carpenter.*		
600 *	„ Murrayi. *Dav.*		
8 „ 25	*Gwynia* capsula. *Jeffr.*		
Shore „ 10†	*Waldheimia* flavescens. *Valenciennes.*		
5 „ 50	„ venosa. *Solander.*		
5 „ 690	„ cranium. *Müller.*		
1,450	„ tenera. *Jeffr.*		
2,160 „ ?*	„ Wyvilli. *Dav.*		
100 „ 150*	„ Kerguelensis. *Dav.*		
110 „ 200	„ Floridana. *Pourtales.*		
15	„ lenticularis. *Deshayes.*		
75 „ 725	„ septigera. *Loven.*		
(?) ?	„ Raphaelis. *Dall.*		
7 „ 50	„ Grayi. *Dav.*		
25 „ 90*	*Terebratella* dorsata. *Gm.*		
From low water to 45 fms.	„ frontalis. *Middendorf.*		
21 to 55	„ Mariæ. *Adams.*		
7 „ 50	„ coreanica. *Adams.*		
(?) ?	„ Bouchardi. *Dav.*		
15 ?	„ cruenta. *Dillwyn.*		

† The range of depth of the European and North Atlantic species has been given me by Dr. Gwyn Jeffreys.

Depths in fathoms hitherto ascertained.		
Near low water mark to 50 fathoms.	*Terebratella* occidentalis.	*Dall.*
5 to 50?	„ pulvinata.	*Gould.*
82 „ 1,340*	„ Frieli.	*Dav.*
15	„ rubicunda.	*Sol.*
20 „ 600	„ Spitzbergensis.	*Dav.*
(?)	„ rubiginosa.	*Dall.*
Low water to about 30 fms.	„ transversa.	*Sow.*
(?) ?	? Labradorensis.	*Sow.*
(?) ?	? Algoensis.	*Sow.*
(?) ?	„ Lamenoni.	*Schrenk.*
5 to 15*	*Magasella* flexuosa.	*King.*
60 ?	„ Gouldi.	*Dall.*
26 ?	„ Adamsi.	*Dav.*
Low water to 10 fms.	„ Aleutica.	*Dall.*
(?)	„ crenulata.	*Sow.*
15 ?	„ Evansi.	*Dav.*
15 ?	„ inconspicua.	*Sow.*
5 to 50?	„ lævis.	*Dall.*
5 „ 50	„ Patagonica.	*Gould.*
Lowest spring tides ?	„ radiata.	*Dall.*
(?) ?	„ suffusa.	*Reeve.*
(?)	? Cumingi.	*Dav.*
15 to 120	*Laqueus* Californica.	*Koch.*
40 „ 85	„ picta.	*Ch.*
35	„ rubella.	*Sow.*
10 „ 292*	*Megerlia* truncata.	*Linné.*
20 „ 292	„ var. monstruosa.	*Scacchi.*
1,850 *	? incerta.	*Dav.*
15 „ 345?	„ Jeffreysi.	*Dall.*
10 „ 63*	„ sanguinea.	*Ch.*
150 *	„ Willemoesi.	*Dav.*
(?)	*Kraussina* rubra.	*Pallas.*
150 *	„ pisum.	*Lam.*
(?) ?	„ cognata.	*Ch.*
Shore to 10 fms. *	„ Lamarckiana.	*Dav.*
Shore to tide mark	„ Davidsoni.	*Vélain.*
(?)	„ capensis.	*Adams* and *Reeve.*
10 to 15	*Bouchardia* rosea.	*Mawe.*
(?) ?	? fibula.	*Reeve.*
40 „ 600*	*Platydia* anomioïdes.	*Sc.*
90 „ 100?	? Davidsoni.	*E. Desl.*
59 „ 95?	? lunifera.	*Phil.*
18 „ 364*	*Argiope* decollata.	*Chem.*
20 „ 45	*Cistella* cistellula.	*S. V. Wood.*
40 „ 130	„ Neapolitana.	*Sc.*
150 ?	„ biplicata.	*Sequenza.*
17 „ 150	„ Barrettiana.	*Dav.*
(?) ?	„ var. lutea.	*Dall.*
60	„ Woodwardiana.	*Dav.*
28 „ 200	„ cuneata.	*Risso.*

Depths in fathoms hitherto ascertained.	
200 to 250?	*Cistella* Schrammi. *Crosse.*
30 „ 43	„ „ var. rubrotincta. *Dall.*
30 „ 300	*Thecidium* Mediterraneum. *Risso.*
60	„ Barretti. *Woodward.*
650 „ 1,443	*Atretia* gnomon. *Jeffr.*
10 „ 690	*Rhynchonella* psittacea. *Gmelin.*
690	„ Sicula. *Seq.*
15 „ 150	„ nigricans. *Sow.*
150 *	„ „ var. pixidata. *Willemoes Suhm* M.S.
(?)	„ Grayi. *Woodward.*
48 „ 100	„ lucida. *Gould.*
	TRETENTERATA. *King.*
3 to 808	*Crania* anomala. *Müll.*
71	„ Japonica. *Ad.*
Low water	„ Suessi. *Reeve.*
6	*Discina* striata. *Schumacher.*
5 to 9	„ (*Discinisca*, Dall) lamellosa. *Brod.*
6	„ „ lævis. *Low.*
6	„ „ Cumingi. *Brod.*
(?)	„ „ Antillarum. *D'Orb.*
17 „ 49*	„ „ stella. *Gould.*
690 „ 2,400*	„ „ Atlantica. *King.*
(?) ?	„ tenuis. *Sow.*
Shore, low water*	*Lingula* anatina. *Lam.*
Shore to 7 fms.	„ exusta. *Reeve.*
(?)	„ hians. *Swainson.*
(?)	„ hirundo. *Reeve.*
7 ?	„ jaspidea. *Ad.*
10	„ lepidula. *Ad.*
(?)	„ Reevi *Dav.*=L. ovalis. *Reeve.*
10	„ smaragdina. *Ad.*
7	„ Adamsi. *Dall.*
(?) ?	„ affinis. *Hancock.*
Shore to 7 fms.	„ tumidula. *Reeve.*
Shore to 7 fms. ?	„ Murphiana. *King.*
Shore	*Glottidia* Antillarum. *Reeve.*
(?) ?	„ Audebarti. *Brod.*
Tidal mud flats at lowish water.	„ Palmieri. *Dall.*
7 to 60	„ albida. *Hinds.*
Low flats and lowest water	„ pyramidata. *Stimpson.*
17 ?	„ semen. *Brod.*

In 1852 I published in the 'Annals and Magazine of Natural History' a sketch of a classification of recent Brachiopoda based upon internal organisation, giving a list of all the recent then known species. Similar and revised lists were subsequently published in 1859 by Professor E. Suess, in 1861 by Lovell Reeve and myself, and in

1870 and 1873 by Mr. W. H. Dall. I have in the above list given all the ranges of depth at present known to me; but of twenty-six species no information is given by the original authors, and later research has not revealed it. It is to be hoped that future dredging expeditions will supply the desiderata. It may also be stated that some twenty of the recent species have also been found in the upper tertiary formations. No permanent list of the recent species can at present be tabulated; but in order that the desired result may ultimately be attainable, it is necessary from time to time to lay before the public the progress that has been achieved in the right direction, pointing out at the same time the unavoidable deficiencies in our knowledge. The ranges in depth recorded in our list are even now sufficient to warrant us in arriving at certain general inductions. Thus, for the sake of argument we will put down the number of recorded species and named varieties at nominally 135, viz , 125 so termed species and 11 named varieties, a number which will certainly have to be hereafter reduced. As nothing is known respecting some 25 or 26 so called species, and which are given in the list, the number upon which we may venture to generalize would be about 107. In approximate numbers we find,

From shore to 500 fathoms, some.....	97	species.
Or named varieties, 12 of these range up to 1,000 fathoms or less.		
From 501 to 1,000 fathoms	16	„
Of these only one, *Discina Atlantica*, would range from 690 to 2,400 fathoms.		
From 1,001 to 1,500 fathoms	6	„
Of these *Ter. Wyvilli* ranges from 1,035 to 2,600 fathoms, the greatest depth at which any species has been found.		
From 2,501 to 2,000 fathoms	3	4
From 2,001 to 2,600 fathoms	3	„

Thus, out of 107 species or named varieties, some 57, or about half the known species, were dredged at depths of about 100 fathoms; 20 to 25 at low water mark, or from 5 to 10 fathoms; the larger number up to 50 or 60 fathoms. These facts indicate that the greater bulk of known species preferred to live at comparatively small or moderate depths, but few in depths ranging up to 500 fathoms, and that Brachiopoda are specifically rare at depths varying from 500 to 2,600 fathoms.

It must, however, in fairness be noted that the number of deep sea dredgings is small when compared with those made in seas of moderate depths; and, consequently, that a proportionably larger number of species may be hereafter expected when a larger area of oceanic abysses have been explored. I do not, however, anticipate that the general results will much alter the conclusions formulated in

the preceding pages. It is also evident that some species were capable of existing at a great variety of depth, for instance—

Platydia anomioides is recorded from	40 to	600	fathoms.
Rhynchonella psittacea from	10 „	690	„
Terebratula vitrea „	5 „	1,456	„
Discina Atlantica „	600 „	2,400	„
Terebratula Wyvilli „	1,035 „	2,600	„

The animal of the same species of Brachiopod is, moreover, capable of existing at different depths without any observable modification in shape and character. It has also been clearly ascertained that the Brachiopoda, although widely distributed, are very much localised, and usually occur in great numbers in their respective haunts.

If we examine the nature of the sea bottom, from which the 'Challenger' specimens were obtained, we find that they were dredged nine times from sea bottoms composed of rock and clay, once from stones and gravel, three times from sand, and eleven times from soft bottoms composed of mud, globigerina, or grey ooze; but as previously stated, as a rule they prefer rocky bottoms and coral reefs.

Out of the 30 or 31 species of Brachiopoda dredged during the 'Challenger' Expedition, ten or eleven appear to be undescribed.

1. *Terebratula Wyvilli, n. sp.*

This is a very interesting species of about 18 millimètres in length, by 17 in width, and 9 in depth, is nearly as broad as long, semi-transparent, smooth, and thin. It has a wide concave depression in the dorsal valve, and a fold in the ventral one with a small simple loop in the interior of the dorsal valve. It bears much external resemblance to several species of Waldheimia of the *W. carinata* group, but differs from the forms of that group by the shape of its loop. It was dredged at four stations, lat. 42° 41′ S., long. 134° 10′ E., depth 2,600 fathoms; in lat. 33° 31′ S., long. 74° 43′ W., depth 2,160 fathoms; lat. 42° 43′ S., long. 82° 11′ W., depth 1,450 fathoms; a small example was also got not far from Falkland Island, depth 1,035 fathoms.

2. *Terebratula Moseleyi, n. sp.*

The shell is broadly oval, semi-globose, and rather longer than wide, white and smooth, loop short, simple; length 23, breadth 21, depth 14 millims.

Five examples were obtained all about the same size. It seems to be a smaller species than *T. vitrea* and *T. Cubensis*, its nearest allies, is less elongated and not as convex. It was dredged west of Kerguelen Island, lat. 46° 47′ S., long. 51° 37′ E.; depth 210 fathoms.

3. *Terebratulina Wyvilli, n. sp.*

This is the most remarkable species of Brachiopod brought home

by the expedition. The shell is large, trigonal, broadest anteriorly, tapering posteriorly; valves moderately convex, somewhat flattened along the middle, and abruptly bent inwards close to the margin. The loop is short, simple, and rendered annular by the union of the oral processes. The surface of valves is marked by fine radiating lines; length 63, width 50, depth 35 millims.

A unique specimen of this fine species was dredged off Culebra Island, to the north-west of St. Thomas, in the West Indies, at a depth of 390 fathoms.

Terebratulina Wyvilli greatly exceeds in dimensions the largest known species of the sub-genus both recent and fossil.

4. *Terebratula or Terebratulina (?) Dalli, n. sp.*

Of this small shell only one dead specimen was dredged, measuring 8 millims. in length by $5\frac{1}{2}$ in width, and 4 in depth. The loop was incomplete, but seems to have been short as in Terebratula and Terebratulina. In shape the shell is longitudinally oval, thin, globose, glassy, and semi-transparent, slightly depressed anteriorly. Exteriorly the surface of valves is covered with fine radiating striæ, with shorter ones interpolated between the larger ones. It was dredged in lat. 34° 37′ W., long. 140° 32′ E.; depth 1,875 fathoms, together with *Discina Atlantica.*

5. *Terebratulina (?) Murrayi, n. sp.*

Of this species some eleven examples were dredged. It is a small shell not exceeding 4 millims. in length by $3\frac{1}{4}$ in width, and 2 in depth. In external shape it is obscurely trigonal, about as broad as long; broadest anteriorly, tapering posteriorly, white, surface marked by a small number of strong radiating ribs. Loop short, simple. Dredged, lat. 28° 33′ S., long. 177° 50′ W., near Kermadoc Isle, south of Fejee Isles, in a depth of 600 fathoms.

This is a remarkable and puzzling species, for while its shell and loop partake of the character of Terebratulina, its labial appendages seem to differ very materially from those of the sub-genus to which it is provisionally referred.

6. *Waldheimia Kerguelensis, n. sp.*

Of this fine species a great number of specimens were dredged by the 'Challenger' Expedition off Marion Island, west of Kerguelen Isle, in a depth of 100 fathoms, and also in lat. 50° 4′ S., long. 71° 22′ E., at a depth of 150 fathoms.

The shell is elongated oval, ventricose, smooth, yellowish-white; ventral valve deeper than the dorsal one, and more or less prominently keeled by the presence of a wide, slightly convex fold, and in the dorsal valve by a slight mesial depression or sinus commencing about

the middle of the valve and extending to the front, and more or less distinctly margined on either side by a faint raised line or ridge: beak incurved and truncated by a small circular foramen, loop long and reflected; length 44, width 34, depth 29 millims.

7. *Waldheimia Wyvilli, n. sp.*

Only one example of this shell was dredged off Valparaiso; lat. 33° 31′ S., long. 74° 43′ W., at a depth of 2,160 fathoms.

The shell is longitudinally oval, very thin, semi-transparent, smooth, light brownish-yellow; valves moderately convex, and rather flattened along the middle in dorsal valve. Loop long, reflected; length 19, width 14, depth 10 millims.

In external shape it somewhat approaches to *Waldheimia cranium*, but differs from it in several respects.

8. *Terebratella Frieli, n. sp.*

Shell small ovate, smooth, white; valves moderately convex, loop doubly attached; length 10, width 9, depth 5 millims. Two examples were dredged off Halifax, lat. 41° 15′ N., long. 65° 45′ W., in 1,340 fathoms; and two other specimens, which appear to be referable to the same species, near the Philippine Islands in from 82 to 102 fathoms.

I have felt much uncertainty with respect to the identification of this species. Dr. Gwyn Jeffreys considers it to be new, and to be referable to the genus *Terebratella*.

9. *Megerlia (?) incerta, n. sp.*

This is a new and very remarkable species. I did not like to sacrifice one of the two or three specimens in the attempt to develop the loop, and therefore am unable to pronounce any decided opinion as to the genus among the Terebratulidæ, to which it should be referred. The strong general resemblance it bears to *Megerlia truncata* induces me to provisionally leaving it in that genus. In external shape it is semi-circular, small, somewhat broader than long, hinge-line, straight, with obtuse cardinal angles, semi-transparent, whitish; valves very moderately convex, ventral valve a little deeper than the opposite one, and slightly longitudinally depressed along the middle; beak small and truncated by an incomplete circular foramen laterally margined by small deltidial plates; beak margin very sharply defined, leaving between them and the hinge-line a sharply defined narrow area; surface of both valves marked by numerous rounded radiating ribs; length 8, breadth 9, depth 4 millims. It was dredged in lat. 1° 47′ N., long. 24° 26′ W.

10. *Megerlia Willemoesi, n. sp.*

Shell oval or longitudinally oval, broadest anteriorly, tapering

posteriorly; valves moderately convex; surface smooth, white; length 10, width 9, depth 5 millims.

Five examples of this interesting species were dredged in lat. 36° 56′ S., long. 150° 30′ E., off Twofold Bay, South Australia, in 120 fathoms.

The other species of Brachiopoda, dredged in the 'Challenger' Expedition, have been already named along with the depths at which they were found. It is, however, somewhat remarkable that the 'Challenger' Expedition did not bring back any of those red-coloured species which are so abundant near New Zealand, Japan, and other southern places.

IX. "Electrodynamic Qualities of Metals.* Part VII. Effects of Stress on the Magnetization of Iron, Nickel, and Cobalt." By Sir William Thomson, F.R.S., Professor of Natural Philosophy in the University of Glasgow. Received May 22, 1878.

(Abstract.)

This paper commences with a detailed description of a series of experiments on the effects of stress on the magnetism of soft iron, of which some first results were described in a preliminary notice, communicated to the Royal Society on the 10th of June, 1875, and published in the "Proceedings." A few months later, the author found that he had been anticipated by Villari† in the most remarkable of those results—that showing increase or diminution of magnetization by longitudinal pull, according as the magnetizing force is less than, or greater than, a certain critical value.

In the first series of experiments described in this paper, the amount of the magnetizing force is varied through a range of values from zero to 900, on a scale on which about $12\frac{1}{2}$ is the value of the vertical component of the terrestrial magnetic force at Glasgow, and the effects of hanging on and taking off weights of 7 lbs., 14 lbs., and 21 lbs.,‡ in changing the induced magnetism, are observed. The experiments were made at ordinary atmospheric temperatures, and at temperature 100° C. The results are shown in curves, of which the abscissas represent the magnetizing forces and other ordinates, the change of magnetism produced by "ons" and "offs" of the weight while the magnetizing force is kept constant. The Villari critical value was

* Phil. Trans., 1875. † Poggendorf's "Annalen," 1868.

‡ The wire was of about 22 Birmingham gauge, weighing therefore about 14 lbs. per nautical mile. It was so soft that it had experienced a considerable permanent stretch by 21 lbs.; it would probably break with 30 or 40 lbs. Steel pianoforte wire of same gauge bears about 230 lbs.

found to differ for the two temperatures, and for different weights: thus approximately:—

Amount of weight "on" and off."	Magnetizing force for which the "on" and "off" produce no change of magnetism.	
	At atmospheric temperature (being about 15° C.)	At temperature 100° C.
7 lbs.	266	280 or 290
14 „	281	286
21 „	288	310

The maximum effect of the "on" and "off" was found in each case with magnetizing force of from 50 to 60 of the arbitrary scale divisions (or about four times the Glasgow vertical force). Its amount differed notably, though not greatly, with the temperature, and, as was to be expected, greatly with the different amounts of pull; but it was not nearly three times as much with 21 lbs. as with 7 lbs.; thus approximately:—

Amount of weight "on" and "off."	Maximum effect in the way of augmentation of magnetism by "on" and diminution by "off."	
	Temperature about 15° C.	Temperature 100° C.
7 lbs.	31 scale divisions of ballistic galvanometer.	25 scale divisions.
14 „	35 do. do.	32·4 do. do.
21 „	54 do. do.	50·3 do. do.

The curves all tend to asymptotes parallel to the line of abscissas on its negative side for infinite magnetizing forces; and they indicate the following ultimate values for the two temperatures, and the different amounts of pull:—

Amount of weight "on" and "off."	Effect in the way of diminution of magnetism by "on" and augmentation by "off" when the magnetizing force is very great.	
	Temperature 15° C.	Temperature 100° C.
7 lbs.	6 scale divisions of ballistic galvanometer.	3 scale divisions.
14 „	13·5 do. do.	9·2 do. do.
21 „	21 do. do.	15·2 do. do.

For other features the curves themselves as given in the paper may be looked to.

Later experiments on the effects of pull transverse to the direction of magnetization showed correspondingly *opposite* effects to those of longitudinal pull, but with a "critical value" of magnetizing force nearly twice as great. That for longitudinal pull, according to the preceding figures, was about 23 times the Glasgow vertical force; for the transverse pull the critical value found was about 60 times the Glasgow vertical force. The transverse pull was produced by water pressure in the interior of a gun-barrel applied by a piston and lever at one end. Thus a pressure of about 1,000 lbs. per square inch, applied and removed at pleasure, gave effects on the magnetism induced in the vertical gun-barrel by the vertical component of the terrestrial magnetic force, and, again, by an electric current through a coil of insulated copper wire round the gun-barrel. When the force magnetizing the gun-barrel was anything less than about 60 times the Glasgow vertical force, the magnetization was found to be *less* with the pressure on than off. When the magnetizing force exceeded that critical value, the magnetization was *greater* with the pressure on than off. The residual (retained) magnetism was always less with the pressure on than off (after ten or a dozen "ons" and "offs" of the pressure to shake out as much of the magnetization as was so loosely held as to be shaken out by this agitation).

The vertical component of the terrestrial magnetic force at Glasgow is about ·43 c.-g.-s. units. Hence the critical values of the magnetizing force for longitudinal and transverse pull are approximately 10 and 25 c.-g.-s. units. With any magnetizing force between these limits the effect of pull whether transverse or longitudinal must be to diminish the magnetization. Hence it is to be inferred that equal pull in all directions would diminish, and equal positive pressure in all directions would increase, the magnetization under the influence of force between these critical values, and through some range above and below them; and not improbably for all amounts, however large or small, of the magnetizing force (?); but further experiment is necessary to answer this question.

The opposite effects of longitudinal and transverse pull, for magnetizing forces not between the critical range of from 10 to 25 c.-g.-s. units, show an aeolotropic magnetic susceptibility in iron under aeolotropic stress [that is, any stress other than pressure (whether positive or negative) equal in all directions.] Consideration of the relation of this result to Wiedemann's remarkable discovery of the induction of longitudinal magnetization by twisting an iron wire through which an electric current is maintained, is important and suggestive. In the present paper a counter-influence is pointed out, in the aeolotropic change of electric conductivity probably produced in the iron by

stress.* This influence illustrated by experiments made a few days ago for the author, by Mr. Macfarlane, in Glasgow, and Mr. Bottomley, in the Physical Laboratory of King's College, London, by kind permission of Professor Adams, which show in two very different ways that twisting a brass tube through which a current of electricity is maintained gives to the electric stream lines a spirality of opposite name to that which the twist gives to longitudinal filaments of the substance, and so proves that in aeolotropically stressed brass the electric conductivity is greatest and least in the directions of greatest and least pressure. The some law probably holds for iron. Wiedemann's result is that the end of the iron wire by which the current enters, becomes a true north or a true south pole, according as the twist is that of a right handed or of a left handed screw. This is the same direction of effect as would result from the aeolotropy of the magnetic susceptibility produced by the stress if the tangential magnetizing force in the outer part of the wire is less than the critical value, for which the effect of the stress is isotropic; but it is opposite to the effect due to the aeolotropy of the electric conductivity. Yet the author in repeating Wiedemann's experiments has found his result—the same in direction, and greatest in amount—with the strongest currents he has hitherto applied—currents strong enough to heat the wire seriously (but not yet measured or estimated in absolute measure). The reconciliation of the Wiedemann result with the conflicting influence of conductive aeolotropy, and with the influence of aeolotropy of magnetic susceptibility, which also is conflicting when the magnetizing force is great enough, is a difficulty which calls for investigation.

The paper includes a series of experiments on the effects of twist on magnetization of iron wire under longitudinal magnetizing force (the Glasgow vertical force alone in this first series). It confirms results of previous experimenters, Matteucci, Wertheim, and Edmond Becquerel, according to which twist in either direction diminishes the magnetization; and extends them to wires under different amounts of longitudinal pull. When the pull was great—approaching the limit of elasticity of the wire, the twist, even when well within the limits of elasticity, had much less effect in diminishing the magnetism than when the pull was small. The results are recorded in curves which show a very remarkable lagging of effect, or residue of influence of previous conditions.

The paper concludes with a description of experiments, showing in bars of nickel and cobalt effects of longitudinal pull opposite to those found by Villari for iron with magnetizing force below the critical

* See § 161 of Part V of this paper ("Electrodynamic Qualities of Metals," "Trans. Roy. Soc.," vol. cxlvi, Feb., 1856. Also Tomlinson, "Proc. Roy. Soc.," vol. xxvi, p. 401, 1877.

value—that is to say, the magnetization of the nickel and cobalt was diminished by pull. But this effect came to a maximum, and began to diminish markedly as if towards zero, when the magnetizing force was diminished. Hitherto the critical value, if there is one, has not been reached; but the experiments are being continued to find it, if it is to be found, with attainable degrees of magnetizing force.

(Addition, May 23, 1878.)

It had been reached, for nickel, in Glasgow, about the day on which this abstract was written; advantage having been taken of a kind loan, by Professor Tait, of a much smaller bar of nickel than those which had been specially made for the investigation, and which alone had been previously available. Mr. Thomas Gray, by whom the experiments were made, in the Physical Laboratory of the University of Glasgow, in the author's absence, found the critical value of the magnetizing force for Professor Tait's thin nickel bar to be about 600 times the Glasgow vertical force.

[The author is indebted to the celebrated metallurgical chemist, Mr. Joseph Wharton, of Philadelphia, for a splendid and unique set of bars, globes, and disks, of pure nickel and cobalt, which he kindly made, at his request, for this and other proposed investigations of electro-dynamic qualities of those metals.]

X. "On the Existence of a Rudimentary Head-Kidney in the Embryo Chick." By F. M. Balfour, M.A., Fellow of Trinity College, Cambridge, and Adam Sedgwick, B.A., Scholar of Trinity College, Cambridge. Communicated by Dr. M. Foster, F.R.S., Prælector of Physiology in Trinity College, Cambridge. Received May 20, 1878.

We have been for some time engaged in an investigation on the mode of growth of the developing Müllerian duct in the chick, and its possible derivation from the Wolffian duct; and, while carrying on our investigations on this point, were struck by some remarkable features of the abdominal opening of the Müllerian duct in its very early condition. We did not for some time pay much attention to these features, but finally devoted ourselves to their interpretation, and have been led to the conclusion that they form the rudiment of a head-kidney, "Vorniere" or "Kopfniere," identical with that present in Amphibia, Marsipobranchii, and Teleostei. We purpose first to give a short account of our observations, and then to proceed to state the grounds on which we have been led to compare the structures we have found with the head-kidney of the Ichthyopsida

The first trace* of the Müllerian duct we have met with is a very shallow groove in the germinal epithelium some little way behind the front end of the Wolffian body, and nearly overlying, though slightly external to, the Wolffian duct. This stage corresponds with the earliest stage described by Dr. Gasser.† In the next stage, which follows very closely upon the first one, remarkable changes have taken place in the groove, which can best be explained by describing the appearance of a series of successive sections from before backwards through the groove and its continuation.

Anteriorly there appears in section a simple groove, which, after a short distance, is suddenly replaced by an apparently solid thickening of the germinal epithelium. An open groove again appears in this, which in its turn is succeeded by a thickening, which is next converted into a definite ridge, projecting inwards towards the Wolffian duct, in which, we believe, we have observed a rudimentary lumen. After one or two sections, a slit-like opening appears, placing the lumen in the ridge in communication with the body cavity, and, in fact, a groove somewhat deeper but otherwise exactly similar to that visible in the anterior sections has reappeared. This groove after being continued for two or three sections again becomes closed, and its walls are prolonged as a nearly solid rod which cannot be traced beyond one or two sections. In general terms, the change which has taken place is, that the opening of the primitive simple groove has been divided into three, and the three separate grooves are connected by a rod-like structure developed from part of the wall of the original groove, in which a lumen either exists from the first, or very soon appears.

In the next stage, the anterior part of the Müllerian duct is formed of a tube communicating by, at the least, three separate apertures with the body cavity, and the tube connecting these is perhaps slightly convoluted. The hindermost opening of the duct is continuous with a rod-like body which may be traced backwards for some little distance. It is hollow in front, and terminates by a solid point, in a manner which we propose describing in detail in a fuller paper, with illustrations.

This peculiar condition of the abdominal opening of the Müllerian duct does not last for long, and before the Müllerian duct has nearly grown back to the cloaca, it opens in front by a single elongated groove. At this period, its hind end, or growing point, instead of being conical, is rounded, and the lumen of the duct is continued into the rounded extremity.

So far as we know, none of the very divergent accounts which have

* We do not give the number of hours of incubation, as we find these are too inconstant to be of any scientific value.

† "Entwicklungsgeschichte d. Allantois d. Müller'schen Gänge u. d. Afters." Frankfurt, 1874.

been given of the development of the Müllerian duct in the least resemble what we have described, though the well known and often repeated figure of Waldeyer* (copied in the "Elements of Embryology," fig. 51) very possibly represents a section of the anterior extremity of the duct in an interval between two openings.

In the above description we have given the more important results of our investigation, and it only remains for us to prove that the peculiarly modified anterior extremity of the Müllerian duct is in reality a rudimentary head-kidney. For this purpose it is necessary for us to show, first, that it is in the situation where we might expect to find a head-kidney; and secondly, that it resembles a head-kidney in structure.

It will be convenient to reverse the logical order, and to attempt to prove, in the first instance, the second of these points. We will use as our type the head-kidney in the Amphibia.

The head-kidney of Amphibian larvæ was discovered by Johannes Müller,† and is often spoken of as the Müllerian body. Its development has, in recent times, been worked out in a satisfactory manner by W. Müller,‡ Götte,§ Spengel, ‖and Fürbringer.¶ It developes from the anterior extremity of what becomes afterwards its duct. This duct is called by Fürbringer the duct of the head-kidney, and by one of us** the *segmental duct.*

This duct, for which we shall retain the name segmental duct, developes as a groove-like invagination of the epithelium of the body cavity, which soon becomes constricted into a duct, blind behind, but ending in front by a groove in free communication with the body cavity. *The open groove at first deepens, still remaining in communication with the body cavity. It next elongates, and by process of unequal constriction becomes converted into a horizontal canal, retaining its communication with the body cavity by a number of openings, varying according to the species from two to four.*

The part of the duct following on the horizontal canal next assumes an S-shaped curvature, continuous, however, with the segmental duct behind, which somewhat later acquires an opening into the cloaca. We need not follow the further development and final atrophy of the head-

* "Eierstock u. Ei." Leipzig, 1870.

† "Ueber die Wolff'schen Körper bei d. Embryonen d. Frösche u. Kröten." "Meckels Archiv," 1829.

‡ "Ueber d. Urogenital System der Amphioxus u. d. Cyclostomen." "Jenaische Zeitschrift," Bd. ix.

§ "Entwicklungsgeschichte d. Unke."

‖ "Urogenitalsystem d. Amphibien. Arbeiten a. d. Zool-zoot. Institut zu Würzburg," Bd. iv.

¶ "Zur. Entwicklung der Amphibienniere." Heidelberg, 1877. "Zur verg. Anat. u. Entwick. d. Excretionsorgane d. Vertebraten." "Morphologische Jahrbuch," Bd. vi.

** Balfour, "Origin and History of Urinogenital Organs of Vertebrates." "Journal of Anat. and Phys.," vd. x, and "Monograph on Elasmobranch Fishes."

kidney in Amphibia, and consider it only necessary to call attention to the similarity, amounting almost to identity, between our account of the metamorphosis of the anterior part of the Müllerian duct in the bird and the italicised part of our description (taken from Fürbringer) of the changes in the open extremity of the segmental duct in the Amphibia, which results in the formation of the head-kidney.

In this connexion, there is only one point we desire to call attention to, and that is the presence in Amphibia of a peculiar body, usually spoken of as the glomerulus of the head-kidney, which is developed at about the same time as the head-kidney. We believe, though we have not fully satisfied ourselves on the point, that we have found an homologous body in the chick.

In reference to the identity in the position between our head-kidney in the bird and that in Amphibia, we have only to say that one of us* has already, on other grounds, attempted to show that the abdominal opening of the Müllerian duct in the bird is the homologue of the abdominal opening of the segmental duct in Amphibia, Elasmobranchii, &c., and that we believe that this homology will be admitted by most anatomists. Should the interpretation we have given of the peculiarities of the abdominal opening of the Müllerian duct in birds be accepted, a further proof of this homology will be afforded.

We may say, in conclusion, that we trust soon to be in a position to publish a fuller account of our observations, with illustrations.

XI. "Observations on Arctic Sea-water and Ice." By Surgeon-Major E. L. Moss, M.D. Communicated by Captain Sir George Nares, R.N., K.C.B., F.R.S. Received May 3, 1878.

[Publication deferred.]

"Note to Mr. Sedley Taylor's 'Experiments on the Colours shown by thin liquid Films under the Action of Sonorous Vibrations.'" ("Proceedings," vol. xxvii, 1878, pp. 71, seq.) Communicated by J. W. L. Glaisher, F.R.S. Received May 20, 1878.

The failure to obtain steady resultant-forms for two component sounds, mentioned at p. 74, proved, on further inquiry, to be due to the imperfect mode of experimenting there described. I stated this fact in a note, written upon the final revise of my paper; but, by some inadvertence for which I am not responsible, the paper appeared without it.

The Society adjourned over Ascension Day and the Whitsuntide Recess to Thursday, June 20.

* Balfour, *loc. cit.*

June 6, 1878.

The Annual Meeting for the election of Fellows was held this day.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Major-General H. Clerk and Dr. G. Johnson were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following candidates were declared duly elected into the Society.

John Gilbert Baker, F.L.S.
Francis Maitland Balfour, M.A.
Rev. Thomas George Bonney, M.A.
Prof. James Henry Cotterill, M.A.
Sir Walter Elliot, K.C.S.I.
Rev. Canon W. Greenwell, M.A.
Thomas Hawksley, C.E.
John Hopkinson, M.A. D.Sc.
John Hughlings Jackson, M.D.
Lord Lindsay, P.R.A.S.
Samuel Roberts, M.A.
Edward Albert Schäfer, M.R.C.S.
Hermann Sprengel, Ph.D.
George James Symons.
Charles Sissmore Tomes, M.A.

Thanks were given to the Scrutators.

June 20, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

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

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

The Right Hon. William Henry Smith, Mr. John Gilbert Baker, Mr. Francis Maitland Balfour, Prof. James Henry Cotterill, Sir Walter Elliot, Rev. Canon W. Greenwell, Mr. Thomas Hawksley, Dr. John Hopkinson, Mr. Samuel Roberts, Mr. George James Symons, and Mr. Charles Sissmore Tomes, were admitted into the Society.

The following Papers were read:—

I. "Notes on Physical Geology. No. V. Mr. George H. Darwin's Comments on Note No. III." By the Rev. SAMUEL HAUGHTON, M.D. Dubl., D.C.L. Oxon, F.R.S., Professor of Geology in the University of Dublin. Received May 20, 1878.

In the "Proceedings of the Royal Society," 14th March, 1878, p. 179, Mr. George H. Darwin has published a criticism on my proposed method of finding a limit to the duration of certain geological periods, published in the "Proceedings of the Royal Society," 20th December, 1877, p. 534.

My paper, of 20th December, consisted of two parts:—1st. A discussion of the rate at which a "wabble" of the earth's axis of rotation, caused by a want of coincidence between the axis of figure and the axis of rotation, would be destroyed by the friction of the ocean against its bed. 2nd. Speculative inferences from the solution of this problem, as to the duration of geological periods, depending on hypothetical geological assumptions as to the method in which Europasia was manufactured.

These assumptions were three in number, and none of them very probable, viz. :—

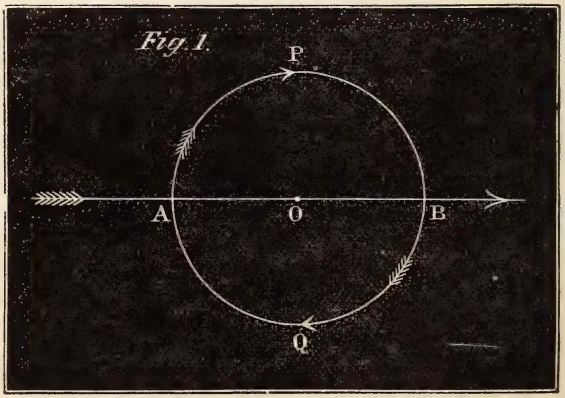
(*a.*) An instantaneous formation of the continent;

(*b.*) Its formation by means of 69 equal convulsions, with an interval of time between each, sufficient to reduce the radius of the wabble from one mile to five feet;

(*c.*) Its formation, by a number of small shocks, each displacing the axis of figure by such an amount that tidal friction would be able to render the axes of figure and rotation again coincident in the period of a single wabble.

In discussing the two latter hypotheses, I naturally timed my shocks so as to produce the maximum wabble. For this assumption I have, of course, no authority; and I readily admit that any conclusions drawn from the earth's wabble can have no more value than the probability of the hypothesis we may make as to the method of formation of Europasia.

In general, let AOB, fig. 1, be a portion of the path described by the axis of the figure, let AO be the displacement produced by any single shock, and let APBQ be the circle described by the axis of rotation in the first wabble after the occurrence of the shock.



If the next shock which moves the axis of figure from O to B occurs when the axis of rotation is at A, the wabble will be doubled, and have the radius AB; but if the second shock be so timed as to

occur when the axis of rotation is passing through B, the wabble will immediately cease, for the axes of rotation and of figure will coincide.*

If $\tau = 304{\cdot}75$ days, the wabble will be doubled if the next shock occurs at an interval denoted by $n\tau$, and will be destroyed if the interval is $(n + \frac{1}{2})\tau$.

If the shocks occur at irregular intervals, at the moment of shock the axis of rotation may be anywhere on the circle APBQ, and the mean effect will be found when the axis is at P or Q, which would be a more probable assumption than that made by me, when I placed the axis always at A. Let us now calculate the mean effect when shocks occurring at unknown intervals take place when the axis of rotation is at P or Q.

Let r denote the radius AO, then we have—

Radius of 1st wabble $= r$.

„ of 2nd „ $= r\sqrt{2}$.

„ of 3rd „ $= r\sqrt{3}$.

„ of *nth* wabble $= r\sqrt{n}$.

If $r = 5$ feet (the least observable wabble), n, the number of equal shocks required to displace the axis of figure through 69 miles, will be

$$n = \frac{69 \times 5280}{5} = 72{,}864,$$

and

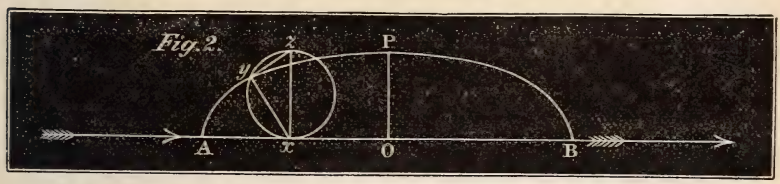
$$\sqrt{n} = 270.$$

This number should be substituted for A′ in equation 12 (Note iii, p. 543), when we obtain, for the number of years required by tidal friction to destroy the final wabble,

320,380 years.

This, as might be expected, is half the time required to destroy the final wabble, when all the shocks were additive at a maximum and occurred when the axis of rotation was passing through A.

Mr. Darwin has discussed at length the case of the axis of figure moving uniformly, and finds that the axis of rotation will move on a cycloid. I here give an easy geometrical proof of this theorem :—



Let AOB be a portion of the path uniformly described by the axis

* If AB be 69 miles, Europasia might have been manufactured in 152½ days, by two equal shocks.

of figure; and let x be the position of the axis of figure at any moment, and y the corresponding position of the axis of rotation, then by the conditions of the problem, fig. 2,

Ax varies as the time,

The angle, $Axy = yzx$ varies as the time.

But the angle yzx is proportional to the arc xy: therefore

Ax varies as arc yx,

but they are supposed to start together from zero, at the point A. Therefore Ax = arc xy, and the locus of y is the common cycloid, whose generating circle is xyz with diameter equal to the axis of the cycloid PO.

If the axis of figure therefore travels uniformly, the axis of rotation will describe a cycloid, and the two axes will coincide every $152\frac{1}{2}$ days, at the cusps, and the maximum radius of wabble will be OP, the diameter of the generating circle.

Mr. Darwin, finally, discusses the following problem:*

"I will now suppose that the geological changes begin suddenly from rest and proceed at such a rate that the variations in the position of the principal axis are imperceptible to astronomical observation. I will suppose, therefore, that the extremity of the instantaneous axis is never more than 5 feet distant from the extremity of the principal axis."

Mr. Darwin deduces from this that a displacement in the axis of figure, amounting to 69 miles, might be produced in 19,200 years, without ever producing an observable wabble.

My calculation of this problem is as follows:—

Since OP is 5 feet, the base of the cycloid, AB will be 5π, and as this length is described by the axis of figure in half a wabble, we have, if x be the number of years required to describe 69 miles,

$$5\pi : 69 \times 5280 :: \frac{304{\cdot}75}{2 \times 365{\cdot}25} : x.$$

From this I find—

$$x = \frac{69 \times 5280 \times 304{\cdot}75}{2 \times 365{\cdot}25 \times 5\pi} = 9675{\cdot}7 \text{ years.}$$

This result is half that obtained by Mr. Darwin.

My object in publishing the latter part of my paper, was to show that if geologists shall ever be in a position to give us exact information as to the mode of formation of a continent, under certain circumstances, conclusions of value as to geological time might be deduced from observations made on the wabble of the axis of rotation; but it is clear that, in the present state of our knowledge, such conclusions must be regarded as hypothetical.

* *Ante*, p. 182.

II. "On the Acceleration of Oxidation by the Least Refrangible End of the Spectrum." Note II. By Captain Abney, R.E., F.R.S. Received June 8, 1878.

In my first note on this subject it was stated that further experiments would be undertaken, in which sensitive films would be exposed to the action of the spectrum in atmospheres free from oxygen. These have been carried out by means of apparatus specially designed for the purpose, hydrogen and nitrogen being the atmospheres employed, and in some cases hydrogen vacua. In every case the experiments were confirmatory of what was previously surmised, the image showing no signs of oxidation, and there is evidence to show that the limit of sensibility of the compounds used is lowered towards the least refrangible end of the spectrum.

Exposure of films in solutions which readily combine with oxygen, and at the same time with the halogens, have given most remarkable results. For instance, silver bromide which by its colour should have proved sensitive to the red end, yet when exposed in the usual manner was insensitive below B in the spectrum, proved sensitive when exposed in sodium sulphite (Na_2SO_3), and arrived at M the lowest limit (about W.L. 12,000) which I have as yet photographed. In the same solution, to quote another experiment, silver iodide proved sensitive to a point between *a* and A. The experiments were carried out in duplicate. In one the plate was immersed in the solution, and in another the salt was dissolved when possible in glycerine and applied to the film. Both methods answered equally well, but for some purposes the latter is more convenient.

My experiments also prove, that what is technically known as solarization is due to the oxidation of the image, accelerated by light generally, be it the more or less refrangible end of the spectrum. This oxidation causes the formation of a compound which is undevelopable, as already has been pointed out. It has thus been found impossible to produce solarization in solutions which have oxygen absorbents. We may therefore conclude that the whole spectrum exercises a reducing action on the sensitive salt, and that this reduced compound is again capable of being oxidized by it.

The relative power of the two actions seems to vary according to the part of the spectrum. This subject is still under consideration.

In my first note I also mentioned that photography in natural colours probably depended on the same action. My surmise is confirmed to a great extent. If silver sub-chloride or silver sub-bromide be produced chemically, we have a dark compound formed which, if exposed to the action of the spectrum whilst in an oxidizing solution (such as hydrogen peroxide), rapidly takes the colour of the rays acting upon it,

the yellow being the least marked. The red, green, and blue are however particularly well rendered by reflected light, and the plate shows the colours as seen when a dull light is thrown on the slit of the spectroscope, a simile which was suggested to me by Mr. Norman Lockyer.

From the evidence obtained by these experiments it appears that two or three molecular groupings are sufficient to give the necessary colours, a subject which I only allude to, since the more general question of molecular groupings is being considered by others.

III. "A Tenth Memoir on Quantics." By A. Cayley, Sadlerian Professor of Pure Mathematics in the University of Cambridge. Received June 12, 1878.

(Abstract.)

The present memoir, which relates to the binary quintic $(*)(x, y)^5$, has been in hand for a considerable time; the chief subject-matter was intended to be the theory of a canonical form discovered by myself, and which is briefly noticed in "Salmon's Higher Algebra," 3rd Ed (1876), pp. 217, 218; writing $a, b, c, d, e, f, g \ldots u, v, w$, to denote the 23 *covariants* of the quintic, then a, b, c, d, f are connected by the relation $f^2 = -a^3d + a^2bc - 4c^3$; and the form contains these covariants thus connected together, and also e; it in fact is ... $(1, 0, c, f, a^2b - 3c^2, a^2e - 2cf)(x, y)^5$.

But the whole plan of the memoir was changed by Sylvester's discovery of what I term the Numerical Generating Function (N.G.F.) of the covariants of the quintic, and my own subsequent establishment of the Real Generating Function (R.G.F.) of the same covariants. The effect of this was to enable me to establish for any given degree in the coefficients and order in the variables, or, as it is convenient to express it, for any given deg-order whatever, a selected system of powers and products of the covariants, say a system of "segregates;" these are asyzygetic, that is, not connected together by any linear equation with numerical coefficients; and they are also such that every other combination of covariants of the same deg-order, say, every "congregate" of the same deg-order, can be expressed (and that, obviously, in one way only) as a linear function, with numerical coefficients, of the segregates of that deg-order. The number of congregates of a given deg-order is precisely equal to the number of the independent syzygies of the same deg-order, so that these syzygies give in effect the congregates in terms of the segregates: and the proper form in which to exhibit the syzygies is then to make each of them give a single congregate in terms of the segregates, viz., the left hand side can always be taken to be a monomial congregate $a^\alpha b^\beta \ldots$, or, to avoid fractions, a numerical multiple of such form, and the right hand

side will then be a linear function, with numerical coefficients, of the segregates of the same deg-order. Supposing such a system of syzygies obtained for a given deg-order, any covariant function (rational and integral function of covariants) is at once expressible as a linear function of the segregates of that deg-order; it is in fact only necessary to substitute therein, for every monomial congregate, its value as a linear function of the segregates. Using the word covariant in its most general sense, the general conclusion thus is that every covariant can be expressed, and that in one way only, as a linear function of segregates, or, say, in the segregate form.

Reverting to the theory of the canonical form, and attending to the relation $f^2 = -a^3d + a^2bc - 4c^3$, it thereby appears that every covariant multiplied by a power of the quintic itself, a, can be expressed, and that in one way only, as a rational and integral function of the covariants, a, b, c, d, e, f, linear as regards f; say, every covariant, multiplied by a power of a, can be expressed, and that in one way only, in the "standard" form. As an illustration, we may take $a^2h = 6acd + 4bc^2 + ef$. Conversely, an expression of the standard form, that is, a rational and integral function of a, b, c, d, e, f, linear as regards f, not explicitly divisible by a, may very well be really divisible by a power of a (the expression of the quotient, of course containing one or more of the higher covariants, g, h, &c.), and we say that, in this case, the expression is "divisible," and has for its "divided" form the quotient expressed as a rational and integral function of covariants. Observe that, in general, the divided form is not perfectly definite, only becoming so when expressed in the before-mentioned segregate form, and that this further reduction ought to be made. There is occasion, however, to consider these divided forms, whether or not thus further reduced, and moreover it sometimes happens that the form presents itself or can be obtained with integer numerical coefficients, while the coefficients of the corresponding segregate form are fractional.

The canonical form is peculiarly convenient for obtaining the expressions of the several derivatives (Gordan's "Uebereinanderschiebungen"), $(a, b)^1$, $(a, b)^2$, &c. (or, as I propose to write them, $ab1$, $ab2$, &c.), which can be formed with two covariants the same or different, as rational and integral functions of the several covariants. It will be recollected that, in Gordan's theory, these derivatives are used in order to establish the system of the 23 covariants, but it seems preferable to have the system of covariants and, by means of them, to obtain the theory of the derivatives.

I mention, at the end of the memoir, two expressions (one or both of them due to Sylvester) for the N.G.F. of a binary sextic.

The several points above adverted to are considered in the memoir; the paragraphs are numbered consecutively with those of the former memoirs upon quantics.

IV. "Osteology of Polyodon folium." By T. W. Bridge, B.A., Scholar of Trinity College, and Demonstrator of Comparative Anatomy in the University of Cambridge. Communicated by Professor Newton, F.R.S. Received May 29, 1878.

(Abstract.)

On a previous occasion* the author described the cranial osteology of one of the more specialized genera of existing Ganoids, and the paper now laid before the Society is an attempt to add to the relatively scanty literature of this department of comparative anatomy, by giving in detail an account of the skeletal structures of one of the most generalized members of the group. The following are among the more important of the conclusions which the facts elucidated appear to warrant.

A comparison of *Polyodon* with its nearest living ally, the Sturgeon, demonstrates the close relationship that exists between the two forms as regards their skeletal anatomy. In so far as *Acipenser* differs from *Polyodon*, it approaches the Teleostean type, nor is the latter genus without indications of having undergone a degree of specialization in the same direction. But not the least important characteristic of *Polyodon* is its possession of a remarkable combination of structures, usually regarded as being specially distinctive of the Plagiostome Elasmobranchii. The existence of such primitive characters, which are also present in *Acipenser*, though to a less extent, suggests the desirability of giving to such facts adequate expression in any scheme that may be proposed for the classification of the Ganoids. After giving a *resumé* of the views held by various zoologists as to the position of these genera in their order, it is suggested that J. Müller's two sub-orders (Chondrostei and Holostei) should be retained, though it seems advantageous to remodel the definitions on which they were based. The various families of recent Ganoidei may be arranged in two sub-orders of approximately equal morphological value, which are mainly differentiated by the structure of their upper jaws.

a. G. Selachoidei.—Pterygoid processes united in a medio-ventral symphysis beneath the basis cranii; notochord persistent and unsegmented; spiracles and mandibular branchiæ present.

Gen. *Polyodon*, *Acipenser*, *Scaphirhynchus*, *Chondrosteus*.

β. G. Teleosteoidei.—Pterygoid processes not united with each other but are connected directly, or indirectly by the intervention of a palatine bone, with the prefrontal region of the skull; vertebral column generally ossified; mandibular gills abortive.

Gen. *Amia*, *Lepidosteus*, *Polypterus*.

* "Journal of Anatomy and Physiology," vol. xi.

If tested by the structure of their upper jaws, the Crossopterygidæ, Lepidosteidæ, and the Palæoniscidæ should be referred to the Teleosteoidei, while the Acanthodidæ would probably belong to the other sub-order. Nevertheless, the distinctness of the two groups is materially lessened by the existence of many annectent fossil forms. *Acipenser* and *Chondrosteus* in the one division and Palæoniscus in the other, partially bridge over the gap which exists between the two when the recent forms only are considered. The Placodermi and the Cephalaspidæ must still be referred to as being "incertæ sedis."

A comparison of the skull of *Polyodon* with the Amphibian skull leads to interesting results. Perhaps the most remarkable feature in which *Polyodon* resembles the Anura, is in the possession of a forwardly directed "orbitar process" associated with a suspensorium so much inclined backwards that the gape is extended even beyond the posterior limits of the skull.

The condition of the "orbitar process," as an apparently functionless rudiment in *Polyodon*, does not throw any light on its primitive origin, but its position and relations in the adult Lamprey, and its transitory condition in the embryo Anura, suggest that originally it may have acted as an anterior suspensor to the much inclined mandibular pier of animals possessing a suctorial mouth, prior to the adaptation of the pterygo-quadrate arcade to that purpose. These facts, together with the rotation of the quadrate cartilage, which we may infer to have taken place from the direction and relations of the "orbitar process," are indications of the existence of a close parallelism between the developmental history of the cranium in the embryo Polyodon, and in such otherwise dissimilar Anurous Amphibia as *Dactylethra capensis*, *Bufo ornatus*, and *Rana temporaria*. The fenestration of the roof of the periotic capsule which exists in Polyodon seems to correspond to the primitive auditory involution which persists in Siren lacertina, and, as in the latter, it is situated to the outer side of the arch of the posterior vertical semicircular canal, and not to its inner or mesial side as is the case with the Selachians.

The co-existence in *Polyodon* of so remarkable a combination of Amphibian and Selachian features suggests an enquiry into the phylogenetic relationship of the Ganoids, the Amphibia, and the Elasmobranchs, or, in other words, suggests the question, is the ancestral stem of the Ganoidei more closely related to that of the Amphibia, or to that of the Elasmobranchs? An analysis of the structural features common to any two of these primary groups seems to affirm the monophyletic origin of the two first-mentioned. On this assumption the relation of the three groups may be roughly and tentatively expressed as follows:—

It seems not improbable that a primitive ancestral stock (x) very

early differentiated into the two groups of Apneumatocœla and Pneumatocœla, the former being the root-stock of the modern Elasmobranchii, while the latter, by acquiring rudimentary and more or less functional lungs, became the primitive double-breathers from which have been derived the Ganoidei and the Amphibia. From the primitive Ganoidei were derived the Teleosteoid Ganoids and eventually the Teleostei also, their originally complex swim-bladders becoming gradually devoted to other functions, while the Selachoidei may be regarded as the but little modified descendants of the original progenitors of the order. The close correspondence that exists between *Polyodon* and the Selachii is not incompatible with these views, but may be the result of the persistence in both of structures originally possessed by their primitive ancestor. Two facts in the cranial anatomy of *Polyodon* are not easy to explain, viz., the formation of the upper jaw and the existence of the "orbitar process." The union of the pterygoid processes in a median symphysis may have been the primitive condition of the jaws in the ancestral form (*x*), but that while persistent in *Polyodon* and in the Selachii, it was superseded by a different arrangement, viz., the union of the pterygoid processes with retral palatine outgrowths in most Ganoidei, and in all Teleostei and Amphibia. Neither is it easy to account for the retention of the "orbitar process." It may have been an adaptive modification correlated with a suctorial mouth in the larval or adult forms of those Ganoids that were first differentiated from the Amphibian stem, and independently developed; or it may have been possessed by, and similarly functioned in, the primitive Pneumatocœla, but has become obsolete in all their descendants, except *Polyodon* and the Anura. Thus it would appear that the Polyodontidæ constitute a remarkably central group. They retain not a few of the characters which we may assume to have belonged to the primitive stock out of which were evolved the three most important groups of Ichthyopsida, combined, however, with a certain amount of specialization; nor are they altogether without indications of retrogression.

V. "On *Astrophiura permira*, an Echinoderm-form intermediate between Ophiuroidea and Asteroidea." By W. Percy Sladen, F.L.S., F.G.S. Communicated by Professor Duncan, F.R.S. Received June 18, 1878.

(Abstract.)

The following peculiarities of structure presented by the anatomy of the echinoderm above described are noteworthy:—

1. The combination of ophiuroid disk- and arm-structure within a pentagonal asteroid form of body.

2. The asteroid character of the ambulacral system: the divisional plates not only being homologous with, but resembling in the manner of their disposition the ambulacral plates of Asteroidea; at the same time furnishing a highly suggestive representation of their phylogenetic development.

3. The rudimentary structure of the mouth-armature, more asteroid than ophiuroid in general facies. Absence of teeth, jaw-plates, and jaws.

4. Extension of the peritoneal cavity to the extremity of the functional portion of the rays, that is to say, to the margin of the pentagonal body.

5. The extremely rudimentary condition and aborted character of that portion of the brachial series which is prolonged beyond the body-disk.

6. The continuity of the tentacular pore-system limited to the disk only.

The above characters are clearly sufficient to stamp the peculiarity of this extraordinary echinoderm, and, whilst excluding it from any known group of genera by their remarkable nature and by the aberrant departure they present from all previous types, are such as would seem to necessitate the relegation of the form to a family apart by itself.

To speak definitely as to the exact position of intermediacy which the organism holds between the Asteroidea and Ophiuroidea would obviously be premature, without a more detailed examination of the internal anatomy than the present specimen in its dry condition will permit, as well as some knowledge of the life-history of the form. It may however be safely affirmed without overstepping the bounds of due caution, that *Astrophiura* bridges further over, from the ophiuroid side, the differences which have separated the two orders, than any previously described starfish or brittle-star.

VI. "Experimental Researches on the Temperature of the Head." Part II, III, IV. By J. S. Lombard, M.D., formerly Assistant Professor of Physiology in Harvard University, U.S. Communicated by H. Charlton Bastian, M.D., F.R.S., Professor of Pathological Anatomy in University College, London. Received June 18, 1878.

(Abstract.)

Part II.—*Examination of the Middle Region of the Head.*

This region is divided on each side into 7 tiers by 6 equidistant lines drawn parallel to the longitudinal median line. The tiers are

numbered from 1 to 7 from below upwards. There are 5 districts in each lateral half, formed by 4 equidistant lines drawn parallel to the anterior and posterior boundaries.* The districts are numbered from 1 to 5 from the anterior boundary backward. Each tier measures vertically on the anterior boundary line 22·78 mm. (0·89 inch), and each district measures horizontally on the median line 16·6 mm. (0·65 inch). The ear cuts off the greater portion of the 3rd district, the whole of the 4th district, and a little of the 5th district—all in the 1st tier. The following are the principal results of the examination of the middle region:—

1st. Comparison of symmetrically situated spaces of the two sides of the head, 100 observations on each pair of spaces.

As in the case of the anterior region, every space may be higher in temperature on the right side or on the left side in turn. The following is the distribution of temperature in the *majority of cases*:—

	In favour of	
	Left side. Districts.	*Right side.* Districts.
1st Tier—	1st,	2nd, 3rd, 5th.
2nd ,, —	0,	1st, 2nd, 3rd, 4th, 5th.
3rd ,, —	0,	1st, 2nd, 3rd, 4th, 5th.
4th ,, —	2nd, 3rd, 4th, 5th,	1st.
5th ,, —	2nd, 3rd, 4th, 5th,	1st.
6th ,, —	2nd, 3rd, 4th, 5th,	1st.
7th ,, —	2nd, 3rd, 4th, 5th,	1st.

Thus in 34 spaces a side compared, the temperature is higher on each side in 17 spaces. Taking the total number of observations, 3,400, and deducting 107 cases of equality of temperature, the following are the percentages of times of occurrence of relative superiority of temperature for the right and left sides respectively:—

Right side,—49·711. Left side,—50·289.

The following are the spaces in which equality of temperature is most generally found:—

1st Tier.	4th Tier.	5th Tier.
Districts—1st, 2nd, 3rd.	2nd, 3rd, 4th, 5th.	2nd, 3rd, 4th, 5th.

6th Tier.	7th Tier.
Districts—2nd, 4th.	1st, 2nd.

The following are the percentages of superiority of temperature on the right side, on the left side, and of equality of temperature,—in the total number of observations:—

Right side—48·147 per cent. Left side—48·706 per cent.
Equality—3·147 per cent.

* For the boundaries of the different regions, see Abstract, Part I.

Quantitative comparisons of the two sides.

The following is a summary of the mean results of 100 comparisons of each pair of symmetrically situated spaces.

The mean difference of temperature is very unequal on the two sides of the head; thus the mean difference for the 17 spaces, which are, on an average, higher in temperature on the right side than on the left, is 0·0589° C. (0·106° F.); while the mean difference for the 17 spaces which are of higher temperature on the left side than on the right, is 0·1103° C. (0·198° F.) The smallest differences noted are in the 4th district, 2nd tier, and 5th district, 2nd and 3rd tiers, all right side, and 5th district, 5th tier, left side; the difference in each case being 0·016° C. (0·028° F.) The greatest difference noted is in the 3rd district, 5th tier, left side, namely, 0·264° C. (0·475° F.) The extreme range of difference of temperature is therefore 0·248° C. (0·446° F.) The mean difference of temperature of all the observations taken together, irrespective of sides, is 0·087° C. (0·152° F.)

2nd.—*Comparison of spaces situated on one and the same side of the Head.*

(*a.*) Comparison of spaces situated in the same district of two adjoining tiers—50 observations on each pair of spaces.

Left side.	*Right side.*
1st tier, higher in temperature than 2nd tier, by 0·1206° C. (0·217° F.)	0·1093° C. (0·196° F.)
2nd tier, higher in temperature than 1st tier, in 5th district, by 0·264° C. (0·475° F.) ..	0·082° C. (0·147° F.)
2nd tier, higher in temperature than 3rd tier, by 0·158° C. (0·284° F.)	0·144° C. (0·259° F.)
3rd tier, higher in temperature than 4th tier, by 0·107° C. (0·192 F.)	0·176° C. (0·316° F.)
4th tier, higher in temperature than 5th tier, by 0·255° C. (0·459° F.)	0·22° C. (0·396° F.)
5th tier, higher in temperature than 4th tier, in 2nd district, both sides, and in 3rd district, left side, by 0·135° C. (0·244° F.) ..	0·074° C. (0·133° F.)
5th tier, higher in temperature than 6th tier, by 0·244° C. (0·439° F.)	0·227° C. (0·408 F.)
6th tier, higher in temperature than 5th tier, in 4th and 5th districts, by 0·193° C. (0·347° F.)	0·115 C. (0·207° F.)
6th tier, higher in temperature than 7th tier, by 0·106° C. (0·19° F.)	0·103° C. (0·185° F.)

(*b.*) Comparison of spaces situated in two adjoining districts of the same tier—50 observations on each pair of spaces.

Left side.	*Right side.*
1st district, higher in temperature than 2nd district, by 0·107° C. (0·192° F.)	0·127° C. (0·228° F.)
2nd district, higher in temperature than 1st district, in 5th tier, by 0·148° C. (0·266° F.)	0·
2nd district, higher in temperature than 3rd district, by 0·147° C. (0·264° F.)	0·216° C. (0·388° F.)
3rd district, higher in temperature than 4th district, by 0·124° C. (0·223° F.)	0·115° C. (0·207° F.)
4th district, higher in temperature than 5th district, by 0·119° C. (0·214° F.)	0·079° C. (0·142° F.)

Part III.—*Examination of the Posterior Region of the Head.*

The posterior region is divided, on each side, into 6 tiers by 5 equidistant lines, drawn horizontally from the median line to the lateral limit. The tiers are numbered from 1 to 6 from below upward. There are 5 districts in each lateral half, formed by 4 equidistant lines drawn parallel to the median line. The districts are numbered from 1 to 5 from the median line outward. On the median line each tier measures, vertically, 21·33 mm. (0·83 inch). On a horizontal line, passing through the occipital protuberance, each district measures, horizontally, 21 mm. (0·82 inch). As in the case of the anterior region, the 5th tier wants the 5th district, and both the 4th and 5th districts are absent in the 6th tier. The following are the principal results of the examination of the posterior region :—

1st. Comparison of symmetrically situated spaces of the two sides of the head—100 observations on each pair of spaces.

As in the case of the two preceding regions, every space in the posterior region may, in turn, be of higher temperature on the right side or on the left side. The following is the distribution of temperature in the *majority of cases* :—

In favour of

Left side. Districts.	*Right side.* Districts.
1st Tier—1st.	1st Tier—2nd, 3rd, 4th, 5th.
2nd ,, —1st.	2nd ,, —2nd, 3rd, 4th, 5th.
3rd ,, —1st, 2nd.	3rd ,, —3rd, 4th, 5th.
4th ,, —1st, 2nd, 3rd, 4th 5th.	
5th ,, —1st, 2nd, 3rd, 4th.	
6th ,, —1st, 2nd, 3rd.	

Thus, in 27 spaces a side compared, the temperature is higher on the left side in 16 spaces, and on the right side in 11 spaces. Taking the total number of observations, 2,700, and deducting 80 cases of equality of temperature, the following are the percentages of times of

occurrence of relative superiority of temperature for the right and left sides, respectively: Right side, 45·458. Left side, 54·542. The following are the spaces in which equality of temperature is most commonly found:—

	1st Tier.	2nd Tier.	3rd Tier.	4th Tier.
Districts—	1st, 2nd.	1st, 2nd.	2nd, 3rd, 4th.	2nd, 3rd, 4th, 5th.

The following are the percentages of superiority of temperature on the right side, on the left side, and of equality of temperature in the total number of observations:—

Right side—44·112 per cent. Left side—52·926 per cent.
Equality—2·962.

Quantitative comparisons of the two sides.

The following is a summary of the mean results of 100 comparisons of each pair of symmetrically situated spaces.

There is a marked difference in the values representing the mean differences of temperature in favour of the right and left sides respectively; thus, the mean difference of temperature for the 11 spaces which are, as a rule, in favour of the right side, is 0·186° C. (0·334° F.); while the mean difference of temperature of the 16 spaces which are generally in favour of the left side, is 0·066° C. (0·118° F.). The greatest difference noted is in the 4th district, 3rd tier, right side, namely, 0·386° C. (0·694° F.) The smallest difference noted is in the 2nd district, 4th tier, left side, namely, 0·008° C. (0·0144° F.) The extreme range of difference of temperature is 0·378° C. (0·68° F.) The mean difference of temperature of all the observations taken together, irrespective of sides, is 0·115° C. (0·207° F.)

2nd.—*Comparison of spaces situated on one and the same side of the Head.*

(*a.*) Comparison of spaces situated in the same district of adjoining tiers—50 observations on each pair of spaces.

Left side.	*Right side.*
1st tier, higher in temperature than 2nd tier, by 0·112° C. (0·201° F.)	0·103° C. (0·185° F.)
2nd tier, higher in temperature than 1st tier, in 4th and 5th districts, by 0·062° C. (0·111° F.)	0·0725° C. (0·13° F.)
2nd tier, higher in temperature than 3rd tier, by 0·204° C. (0·368° F.)	0·157° C. (0·282° F.)
4th tier, higher in temperature than 3rd tier, by 0·302° C. (0·543° F.)	0·345° C. (0·621° F.)
3rd tier, higher in temperature than 4th tier, in 3rd and 5th districts, by 0· ..	0·085° C. (0·153° F.)

Left side.	*Right side.*
4th tier, higher in temperature than 5th tier, by 0·261° C. (0·47° F.)	0·284° C. (0·512° F.)
5th tier, higher in temperature than 6th tier, by 0·086° C. (0·155° F.)	0·135° C. (0·243° F.)

(*b.*) Comparison of spaces situated in two adjoining districts of the same tier—50 observations on each pair of spaces.

Left side.	*Right side.*
1st district, higher in temperature than 2nd district, in 1st, 2nd, and 3rd tiers, left side, and in 3rd and 6th tiers, right side, by 0·072° C. (0·129° F.)	0·065° C. (0·117° F.)
Remainder of 2nd district, higher in temperature than remainder of 1st district, by 0·065° C. (0·117° F.)	0·076° C. (0·136° F.)
2nd district, higher in temperature than 3rd district, in 3rd tier, left side, and in 4th, 5th, and 6th tiers, both sides, by 0·162° C. (0·291° F.)	0·2° C. (0·36° F.)
Remainder of 3rd district, higher in temperature than remainder of 2nd district, by 0·035° C. (0·063° F.)	0·141° C. (0·243° F.)
3rd district, higher in temperature than 4th district, in 1st and 3rd tiers, both sides, and in 2nd and 5th tiers, left side, by 0·112° C. (0·201° F.)	0·043° C. (0·077° F.)
Remainder of 4th district higher in temperature than remainder of 3rd district, by 0·084° C. (0·151° F.)	0·047° C. (0·084° F.)
4th district, higher in temperature than 5th district, by 0·113° C. (0·203° F.)	0·14° C. (0·252° F.)
5th district, higher in temperature than 4th district, in 3rd tier, left side, by 0·125° C. (0·225° F.)	0·

Part IV.—*On the effect of intellectual and emotional activity on the temperature of the Head.*

(*a.*) *Intellectual work.*

The following is a summary of the principal results:—

1st. Intellectual work causes a rise of temperature in all three regions of the head.

2nd. The rapidity and degree of this rise is different in the different regions.

3rd. Different kinds of work in one and the same person, produce

elevations of temperature differing from each other both in rapidity of appearance and degree.

4th. The kind of work which affects one region in the greatest degree affects all three regions most.

The anterior region shows, as a rule, both the most rapid and the highest rise of temperature. The middle region comes next and the posterior last. A notable exception to the above order is found, however, in the 1st district (and sometimes the 2nd district) of the 3rd, 4th, and 5th tiers of the middle region. In these spaces, the middle region often shows a greater rise of temperature than that exhibited by a considerable part of the anterior region. The average rise of temperature for each region is as follows:—Anterior region, 0·02625° C. (0·04725° F.) Middle region, 0·02062° C. (0·03711° F.) Posterior region, 0·01743° C. (0·03137° F.)

5th. Intellectual work, as a rule, causes a greater rise of temperature on the left side of the head than on the right side: but in a certain number of cases the rise is either greater on the right side or is equal on the two sides. The following are the average percentages of times of occurrence of greater rise of temperature on the left side and on the right side, and of equal rise on the two sides, together with the mean difference of rise in favour of each side:—

Anterior Region.

	Left side.	*Right side.*	*Equality.*
Average percentage of cases in favour of..	75	15·625	9·375
Average difference of rise of temperature	0·00606° C. (0·0109° F.)	0·00295° C. (0·00531° F.)	

Middle Region.

	Left side.	*Right side.*	*Equality.*
Average percentage of cases in favour of..	62·5	21·875	15·625
Average difference of rise of temperature	0·00496° C. (0·00892° F.)	0·00225° C. (0·0045° F.)	

Posterior Region.

	Left side.	*Right side.*	*Equality.*
Average percentage of cases in favour of..	56·25	15·625	28·125
Average difference of rise of temperature	0·00231° C. (0·00415° F.)	0·00162° C. (0·00291° F.)	

In some individuals, after severe and prolonged work, or after work of an exciting character, not very prolonged, the rise of temperature on the left side of the head may be sufficient to cause almost the whole of the anterior region to be of higher temperature on this side.

Alterations in the same direction, but less common and less marked, may also occur in the middle and posterior regions.

(b.) Emotional Activity.

The following results have been obtained by the recitation of poetry or prose of an emotional character:—

1st. Emotional activity, of the kind specified, causes a rise of temperature in all three regions; this rise is, moreover, more rapid and of greater degree than that seen in intellectual work.

2nd. Less difference exists between different regions in the rapidity and degree of rise of temperature in emotional activity than in intellectual work; but the order of the regions, as regards their comparative degrees of rise of temperature, is usually the same in the two cases. The average rise of temperature for each region is as follows:—Anterior region, 0·036° C. (0·0648° F.); middle region, 0·0345° C. (0·0621° F.); posterior region, 0·033° C. (0·0594° F.) Recitation to oneself produces, as a rule, a greater effect than recitation aloud. This result is in accordance with physical laws, a portion of the heat produced being, in recitation aloud, consumed in exterior work.

3rd. In emotional activity, as in intellectual work, the left side of the head is generally more affected than the right side. The average percentages of times of occurrence of greater rise of temperature on the left side and on the right side, and of equal rise on the two sides, together with the mean difference of rise in favour of each side, are as follow:—

Anterior Region.

	Left side.		*Right side.*		*Equality.*
Average percentage of cases in favour of..	77·5	..	17·5	..	5
Average difference of rise of temperature	0·0075° C. (0·0135° F.)	..	0·00573° C. (0·01035° F.)		

Middle Region.

	Left side.		*Right side.*		*Equality.*
Average percentage of cases in favour of..	62·5	..	20	..	17·5
Average difference of rise in temperature	0·00562° C. (0·0116° F.)	..	0·00462° C. (0·00831° F.)		

Posterior Region.

	Left side.		*Right side.*		*Equality.*
Average percentage of cases in favour of ..	55	..	20	..	25
Average difference of rise of temperature	0·00437° C. (0·00786° F.)	..	0·00387° C. (0·00696° F.)		

Emotional activity, of the kind we have been considering, often extends very decidedly the tract of superior temperature on the left side of all three regions.

The effect of anger, in a moderate degree, after the intensity of passion has subsided, has been usually to increase the extent of the tract of superior temperature on the left side, in all three regions. Vexation and mental irritability cause a rise of temperature, most marked in the anterior region and in the anterior spaces of the middle region; the left side is most affected.

VII. "Note on the Effect of various Substances in Destroying the Activity of Cobra Poison." By T. Lauder Brunton, M.D., F.R.S., and Sir Joseph Fayrer, K.C.S.I., M.D., F.R.S. Received June 20, 1878.

In a paper, read some time ago before this Society, by Mr. Pedler, he mentioned his discovery of the fact that the activity of cobra poison was completely destroyed by admixture with perchloride of platinum. This substance, however, could only be regarded as a chemical and not as a physiological antidote to the poison, inasmuch as it had no power to modify or prevent the action of the venom after its absorption into the blood. Mr. Pedler expressed his opinion that the proper method of pursuing the investigation was to investigate separately the action of platinum salts and of cobra poison upon the animal body. In the discussion which followed we stated that the method proposed by Mr. Pedler was in the present instance not likely to lead to any results, and that as the action of the substance employed by him was in all probability due to its simply forming an insoluble compound with the cobra poison and not to any action of the platinum *per se*, certain other metallic salts would have a similar action to the perchloride of platinum. Experiments have confirmed the opinion we then expressed,* and we find the action of chloride of gold is precisely similar to that of perchloride of platinum, the cobra venom being rendered entirely inert by admixture with the gold salt before its injection into the body. Chloride of gold, however, like perchloride of platinum, is merely a chemical antidote, and does not modify the action of the venom after its absorption into the circulation. Permanganate of potash, which has been recommended as an antidote, also destroys its activity completely. Chloride of zinc, chloride of mercury, nitrate of silver, and carbolic acid all diminish the activity of the poison, and prolong life when mixed with it before its injection; but they do not prevent death, nor do they prolong life to any great extent. Perchloride of iron has very much

* The Poison of the Cobra, by A. W. Blyth, M.R.C.S. "The Analyst," 28th February, 1877, p. 204.

less action upon the poison than one would expect, and it prolongs life to a very slight extent. Liquor potassæ impairs the activity of the poison very considerably, and prolongs life for several hours. When a large dose of cobra poison is injected, none of these substances prevent death even when applied immediately to the wound. The reason of this probably is that they do not come into such perfect contact with the poison as to destroy the whole of it, and the portion which escapes destruction is sufficient to kill. It is possible, however, that when minimum doses only are injected, the local application of one or other substance may turn the balance between life and death, but this point we must reserve for a future paper.

Our first experiment was made in order to compare the action of chloride of platinum alone with that of cobra poison alone, and of chloride of platinum injected after cobra poison.

Experiment I.

February 25th, 1878. A cat weighing 4 lbs. had about 1 cub. centim. of the chloride of platinum solution of the British Pharmacopœia injected into its flank.

3.44 P.M. Injection completed.

3.55 ,, No apparent effect. The cat well and playful.

No symptoms whatever were observed, but after some days a slough formed at the point of injection. Chloride of platinum thus appears to have no physiological action whatever when injected subcutaneously beyond its effect as a local irritant.

In Experiments II and III similar doses of cobra poison were subcutaneously injected into two cats; but in Experiment III the injection of the poison was followed immediately by the injection of a solution of chloride of platinum into the same spot, so as if possible to destroy the venom which had not yet been absorbed. In this case death was delayed, but not to a very great extent, as it occurred in an hour and fifty minutes after the injection of the venom and chloride of platinum, and in an hour and two minutes in the animal which received the poison alone.

Experiment II.

Black cat, weight 5 lbs.

25 mgms. of cobra poison dissolved in 1 cub. centim. of distilled water injected into skin of flank at 3.26 P.M. of 25th February.

3.28 P.M. Vomiting. It had taken chloroform to keep it quiet whilst being weighed, and was recovering from the chloroform. Micturated. Drooping head on one side.

3.32 P.M. Vomiting again. Looks much depressed. *Defecated.*

3.40 ,, Breathing slow. *Shallow.*

3.41 ,, Vomiting again.

3.45 ,, Twitching of muscles.

3.52 P.M. The same state.
3.58 „ Defecating. Micturating.
3.59 „ Retching. Vomiting. Moves about in a restless manner.
4.6 „ Moving *backwards* with staggering gait.
4.12 „ Staggers, and head droops.
4.13 „ Falls over on its side. The respiration is slow. Reflex from eye and ear almost gone.
4.14 P.M. Reflex from head and legs when irritated. None from tail.
4.15 „ Attempts to rise.
4.18 „ Got up, but fell over again. Tried to walk. No reflex from the head.
4.20 P.M. Head raised and fell over again. Touching the eye seems to rouse the cat, but no reflex of lids. Tries to get up, but cannot. Fell over on the opposite side.
4.24 P.M. Again a struggle to rise.
4.27 „ Touching the eye produces no reflex. Breathing very slow. Convulsive twitching of limbs.
4.28 P.M. Apparently dead. Heart still beating one hour and two minutes after injection of poison.

The blood, after death, formed a *firm* coagulum.

Intestines much congested. Patch of congestion in stomach. Red *serum effused into the peritoneal* cavity.

No *local* symptoms or changes.

A good deal of food in the stomach, notwithstanding the vomiting. Digestion was in full action.

In these and other experiments the dose of cobra poison was regulated according to the weight of the animal, the same proportion per pound weight being given in each case.

Experiment III.

A grey cat 4 lbs. weight had 20 mgms. of cobra poison, dissolved in one cub. centim. of distilled water, injected under skin of flank at 3.39 P.M. of 25th February.

At 3.40 P.M. a solution of chloride of platinum injected at the same spot.

3.42 P.M. Very restless.
3.43 „ Drinks water.
3.52 „ Vomiting.
3.55 „ Dull and depressed.
4.10 „ Same condition.
4.20 „ Sluggish.
4.30 „ Restless. Moving about.
4.45 „ Staggers. Getting very sluggish. Walks with difficulty. Head drooping.

4.54 P.M. Shivering. Head fallen over.

5 P.M. Fallen over. Slow paralysis creeping over limbs. Respiration slow. Gets up, rolls over again. On touching the eye the eyelid moves. Reflex not gone from the ear.

5.6 P.M. Fallen over. Paralysed. Reflex nearly gone, still slight from ear. Pupils dilated.

5.9 P.M. Convulsions. Pupils become normal again. Respiration very slow—thirteen per minute.

5.20 P.M. Tries to rise. Very feebly.

5.22 ,, Fallen quite over.

5.23 ,, In same condition. Makes feeble efforts to rise. Pupils dilated again.

5.27 P.M. Again tries to rise. Micturition.

5.29 ,, Convulsions.

5.30 ,, Dead.

No local symptoms, *i.e.*, no extravasation about the puncture. No congestion of stomach or bowels. Stomach empty. Blood coagulated *after death.*

Injected at 3.39 P.M.

Died at 5.30 P.M.

Death in one hour and fifty-one minutes.

The following experiments show the effect of chloride of gold in completely destroying the cobra poison.

Experiment IV.

March 7th, 1878. Three mgms. of cobra poison, mixed with 1 grain of chloride of gold, dissolved in 40 grain measures of water, injected into the hip of a white guinea-pig, weighing 18 oz., at 3.50 P.M.

4.10 P.M. Crouching quietly in corner of box. Tremor, perhaps fright.

4·15 P.M. Seems uneasy; crouching in corner. No other change. Recovered without any bad symptoms.

Experiment V.

March 14th. In this experiment a very large dose of poison was used.

30 mgms. of cobra poison, mixed with $1\frac{1}{3}$ cub. centims. of a 10 per cent. solution of chloride of gold, were injected into a guinea-pig weighing 20 oz., at 3.30 P.M.

75 cub. centims. of water was used to wash out glass, and then injected. The poison and the chloride form a yellow creamy precipitate.

3.30 P.M. Began to jerk and twitch immediately, excited, running about the box.

3.35 P.M. Crouching in corner, twitching, but not otherwise affected.

3.42 P.M. Not apparently affected.

3.52 „ Crouching; does not appear affected, but is weak in the hind legs when he runs.

4.10 P.M. Very little affected; hind legs weaker, but he is very active otherwise.

4.20 P.M. Much the same; active, except that hind legs seem rather weak.

4.55 P.M. Remains the same. Recovered perfectly without any further symptoms.

In order to make sure that the dose of cobra poison would certainly prove fatal if administered alone, the animal, after its recovery, was injected with a quantity of pure cobra poison, fifteen times less than that from which it had recovered, and, as will be seen from Experiment VI, death rapidly occurred.

Experiment VI.

March 14th. White guinea-pig that recovered from 30 mgms. of cobra poison, mixed with chloride of gold.

At 4.45 P.M., 2 mgms. of cobra poison were injected into the hip.

4.50 P.M. Very restless; scratching his skin.

4.52 „ Twitching; very restless.

4.59 „ Squeaking; very restless.

5.10 „ Injected leg weak; not so restless.

5.15 „ Trying to vomit; twitching movement of head, jerking upwards; violent efforts to vomit; a sort of cough; flows from nostrils and mouth; getting gradually paralysed, he still crawls; nearly violent efforts to vomit.

5.24 P.M. The animal creeps along, putting his head along the ground.

5.26 P.M. Apparently dead; heart still beats.

At 4.45 P.M. the injection was made, and at 5.26 P.M. the animal was dead. Death in 41 minutes.

Experiment VII shows that chloride of gold is a chemical, and not a physiological antidote, and does not prevent the action of the poison after its absorption.

Experiment VII.

March 14th. Guinea-pig, weight 16 oz.

5 mgms. of cobra poison dissolved in 1 cub. centim. of water, and injected into the right hip at 3.39 P.M.

In 3 minutes afterwards $1\frac{1}{3}$ cub. centims. of a 10 per cent. solution of chloride of gold were injected into another spot (the left hip, at 3·42).

3.43 P.M. Very restless.

3.45 „ Very restless; head twitching; drops the left leg.

3.53 P.M. Restless.

3.55 „ Weak, dropping both hind legs; left appears quite paralysed.

4.5 „ Getting weaker; paralysis creeping over him.

4.10 „ Barely moves; hind quarters completely paralysed.

4.12 „ Convulsions.

4.19 „ Heart still beats feebly.

4.20 „ Dead.

Experiment VIII shows that permanganate of potash destroys the action of the venom.

Experiment VIII.

5 mgms. of poison were dissolved in 1 cub. centim. of water, and mixed with 1 cub. centim. of liquor potassæ permanganatis of the British Pharmacopœia, and injected under the skin of a guinea-pig. No symptoms were produced, and the animal remained quite unaffected.

Experiment IX.

Two rabbits of the same litter, each weighing exactly 2 lbs. were taken. 5 centigrammes of cobra poison dissolved in 1 cub. centim. of distilled water, were mixed with 1 cub. centim. of liquor potassæ permanganatis (B.P.), and allowed to stand for about 8 minutes. The mixture was then injected under the skin of the flank of one rabbit. No symptoms whatever were produced, and the animal though kept under observation for some weeks remained quite unaffected by the poison. 5 mgms. of cobra poison, dissolved in 2 cub. centims. of water, were injected into the other rabbit at the same time. During the injection a little of the poison was lost, so that the animal did not receive the full dose, yet it died in 30 minutes.

Chloride of zinc delays the action of the cobra poison, but does not prevent it, as appears from Experiments X and XI, in which a guinea-pig that had received 3 mgms. of pure cobra poison (Experiment X) died in 45 minutes, whereas one that had received a similar dose, previously mixed with chloride of zinc, lived for about 3 hours. Experiment XI.

Experiment X.

March 1st, 1878. 3 mgms. of cobra poison, dissolved in 2 cub. centims. of distilled water, injected at 3.43 P.M. into a guinea-pig's hip. Weight of guinea-pig 20 oz.

3·46 P.M. Twitching.

3.50 „ Restless twitching.

3.53 „ The same. Irritable; squeals; quarrels with the other guinea-pigs; respiration jerky.

4 P.M. The same.

4.5 P.M. Drags the injected leg, which is nearly paralysed.

4.15 P.M. Much the same.

4.19 P.M. Paralysed, and crawls with difficulty; all hind quarters invaded by poison's influence.

4.21 P.M. Paralysis extending; struggles to rise; can only move the head. Reflex from eye diminished.

4.25 P.M. Convulsive movements.

4.28 ,, Dead in 45 minutes. Heart continued to beat after apparent death.

Experiment XI.

Red guinea-pig, weight 16 oz. At 3.46 P.M., 1st March, 3 mgms. of cobra poison, dissolved in 2 cub. centims. of distilled water and mixed with ·01067 chloride of zinc, were injected subcutaneously. The poison and the chloride were mixed 5 minutes before injection.

5.53 P.M. Guinea-pig restless; twitching; grunting; keeps licking the puncture; irritable with other guinea-pigs.

4 P.M. Very restless. Puncture seems irritable; leg partially paralysed.

4.15 P.M. Much the same.

4.29 ,, Much the same.

4.35 ,, Not quite so restless.

4.45 ,, Active; runs about.

4.45 ,, Restless; not worse.

5 ,, Seems pretty well now.

Died about 7 o'clock.

Liquor potassæ impairs the activity of the poison, but does not destroy it, as will be seen from Experiment XII, in which the dose of the poison, which had usually proved fatal considerably within an hour, did not cause death until 8 hours had elapsed.

Experiment XII.

March 14th. Guinea-pig, weight 16 oz.

5 mgms. of cobra poison dissolved in 1 cub. centim. water mixed with 1 cub. centim. of liquor potassæ injected into hip at 3.52 P.M.

3.53 P.M. Twitching.

4.0 ,, Leg paralysed.

4.20 ,, It seems much the same.

4.35 ,, Appears much the same.

4.55 ,, Appears much the same.

5.35 ,, Much the same.

At 11.30 it was lying with left hind leg paralysed, could walk when irritated, mouth opened, head twitching back frequently.

11.45 P.M. its respiration ceased, but when the skin of the belly was pinched the animal took a breath and respiration continued for about a minute afterwards. The heart continued to beat until 11.50.

Liquor ferri perchloridi fortior (B.P.), has much less action upon

the cobra poison than one would have expected, as will be seen from Experiment XIII, in which death occurred in an hour and a half.

Experiment XIII.

March 14th. Guinea-pig, weight 16 oz.

5 mgms. cobra poison dissolved in 1 cub. centim. water mixed with 1 cub. centim. of liquor ferri perchloridi fortior (B.P.) injected into the left hip of the guinea-pig at 4.4 P.M.

4.20 P.M. Dropped the hind leg, but otherwise seems active and well.
4.35 ,, Very active, but leg drops.
5.10 ,, Hind leg paralysed.
5.15 ,, Tries to crawl, cannot, struggles, convulsed.
5.16 ,, Jerking convulsions.
5.17 ,, Almost dead.
5.18 ,, Dead.

Carbolic acid likewise delays the action of the poison, but to a very much smaller extent than liquor potassæ, as is proved by Experiment XIV.

Experiment XIV.

March 14th. Carbolic acid, one-third of a cub. centim. mixed with 5 mgms. of cobra poison in 1 cub. centim. of water, injected into hip of a guinea-pig weighing 14 oz., at 4.55 P.M.

5.55 P.M. Much the same.
7.50 ,, Very slight convulsions.
7.55 ,, Much the same.
8.0 ,, Dead.

Experiment XV.

March 7th. 3 mgms. cobra poison mixed with 1 grain of nitrate of silver dissolved in 40 grains of water, injected into hip of black guinea-pig, weighing 14 oz., at 3 P.M.

4.10 P.M. Twitching; crouching in corner, crying out slightly, as guinea-pigs are wont to do when restless.

4.15 P.M. Restless, crouched in corner of box, twitching of muscles, cries as before.

4.25 P.M. Restless, crying fretfully.

Died in about one and a half hours afterwards, about two hours after the injection of the poison.

Experiment XVI.

March 7th. 3 mgms. of cobra poison mixed with 10 grain measures of a saturated solution of chloride of mercury (corrosive sublimate), injected into hip of guinea-pig (black and white), weighing 14 oz., at 3.56 P.M.

4.10 P.M. Twitching, uneasy, tremors.
4.15 ,, Quiet, crouching in the corner.

4.24 P.M. Restless, but does not seem to twitch; cries like the other guinea-pigs occasionally.

About 6 the animal lay quiet with occasional twitches, and about 6.30 it died.

In order to ascertain which substance would be most likely to save the life of the animal by local application to the point of injection, either by destroying the poison itself, or by preventing its absorption by the tissues, we applied chloride of gold, permanganate of potash, chloride of platinum, and carbolic acid locally, the method adopted being to inject the poison under the skin of the leg, immediately afterwards to apply a ligature tightly above that point, and then to make an incision and apply the substance in just the same manner as we would have done if the animal had actually been bitten. From the following experiments, however, it will be seen that the absorption of the poison is so rapid that all local applications were useless. It should be noted that the quantity of poison we employed was large, and it still remains to be seen whether these local applications may turn the balance between life and death when the quantity of the poison would be just sufficient to kill in case of no remedy being applied.

Experiment XVII.

Guinea-pig weighing 1 lb.

4.27½ P.M. Injected 2 grs. 9 centigrammes of cobra poison into thigh, ligature applied immediately.

4.29½ P.M. Solution of chloride of platinum applied.

4.31 „ Twitching violently.

4.33 „ Twitching violently; ligature remains on limb.

4.40 „ Not worse; the ligature is evidently delaying the action of the poison.

4.47 P.M. Getting weaker.

4.50 „ Convulsed.

4.52 „ Dead.

Death delayed in this instance.

Experiment XVIII.

April 4th, 1878. Guinea-pig weighing 1½ lb.

4.11′ 10″ P.M. Injected 3 centigrammes of cobra poison.

4.13 P.M. Chloride of gold solution, 1 in 10; ligature kept on until chloride of gold was applied.

4.15 P.M. Twitching.

4.16 „ Leg paralysed.

4.17 „ Animal nearly paralysed.

4.19 „ Do. dying.

4.20 „ Convulsions.

4.22 „ Dead.

Experiment XIX.

April 4th, 1878. Guinea-pig weighing 1½ lb. Injected 4 centigrammes of cobra poison into leg.

4.1 P.M. Ligature applied immediately. Permanganate of potash applied immediately.

4.5 P.M. Twitching.

4.10 „ Dying.

4.13 „ Convulsion.

4.14 „ Dead.

Experiment XX.

April 4th, 1878. Guinea-pig weighing 1 lb.

3.45′ 20″ P.M. Injected ¾ gr. = 4 centigrammes of cobra poison under skin of leg. A ligature was applied round the leg in one minute, and in five minutes permanganate of potash was rubbed into an incision made over the site of injection.

3.52 P.M. Ligature cut.

3.53 „ Twitching violently; leg paralysed.

3.55 „

3.57 „ Dying.

3.58 „ Dead—less than 13 minutes.

VIII. "The Life-History of Bacterium termo and Micrococcus, with further Observations on Bacillus." By J. COSSAR EWART, M.D. Edin., University College, London. Communicated by Professor HUXLEY, Sec. R.S. Received June 20, 1878.

[PLATE 10.]

While recently studying the phases through which the now familiar organism *Bacillus anthracis* passes, my attention was often directed to two still more familiar organisms, *Bacterium termo* and *Micrococcus*. Frequently from cultivations of *Bacillus* both rods, spores, and filaments disappeared, and in their place millions of *Micrococci* and the short-jointed rods of *B. termo* were found.* In the short rods of *B. termo*, which in the struggle for existence overcame the less active *Bacilli*, minute bright particles were often present. These exactly resembled the *Micrococci* in the field around and between them, and were evidently the remains of spores out of which the rods had just been developed. The presence of *Micrococcus*-like spores in the short rods

* This disappearance of the one and appearance of the others accounts for early investigators believing that there was a continuity of development between *Bacilli* and septic organisms.

led me to conclude not only that *B. termo* had a life-history similar to that of *Bacillus anthracis*, but also that Billroth was probably right in believing that *Micrococci* were the spores of ordinary *Bacteria*.

I have already proved * that *Bacillus anthracis*, like *B. subtilis*, and the *Bacillus* found by Dr. Klein on animals suffering from pneumo-enteritis (pig typhoid), is, though rarely, an active organism, the motile phase appearing at irregular intervals; that the protoplasm of the long filaments into which the rods lengthen divides into segments, which, either at once or after again dividing, condense into spores; that the spores before germinating may divide into sporules, and that the new rod is not formed, as stated by Dr. Koch,† out of the gelatinous-looking envelope, but out of the spore itself—the spore germinating, and pushing out a process from one of its ends, which carries along with it the thin capsule, converting it into the sheath of the rod.

Before attempting to compare the life-history of *Bacterium termo* with what we know of the life-history of *Bacillus anthracis*, it was necessary to have *B. termo* isolated from all other organisms. After many failures, I was fortunate enough to find a cultivation in which the rods of *B. termo* were alone visible. After keeping this cultivation under observation for some time, others were made by infecting fresh drops of *humor aqueus*, previously placed on absolutely clean covering-glasses, with as small a drop of the liquid as possible on the point of a needle. The covering-glasses were inverted over "cells" made by fixing glass rings to ordinary slides by means of Brunswick black, the cells having been carefully washed immediately before use with absolute alcohol. A thin layer of olive oil between the edge of the glass ring and the covering-glass prevented evaporation and the entrance of moisture and solid particles from the surrounding atmosphere.

In cultivations of *B. termo* prepared in this way, and kept at a temperature of 30° C., I made out that, under certain conditions, the rods, instead of undergoing fissiparous division, lengthened into filaments, in which in due time spores appeared. (Plate 10. Series V, *h*, *i*.) The filaments were shorter than those of *B. anthracis*, and they never showed any tendency to form a network or mycelium. The spores were extremely small, bright, almost spherical bodies. In from two to three days after their formation the spores escaped from the filaments, and either lay isolated near the centre or formed a zooglœa (Series VI, *h*) at the edge of the cultivation. Having been free for some time, they germinated into short slender rods. (Series V, *b*, *c*.) The young rods did not at once lengthen into another generation of spore-bearing filaments, but, becoming active, they multiplied by transverse fission. (Series V, *d*, *e*.) The filaments packed full of spores often resembled "*Micrococcus* chains," and only differed from filaments containing spores, which I

* "Quart. Journ. Micro. Science," April, 1878.

† "Beitrage zur Biologie der Pflanzen," II, 2, 1876.

lately found in scum from the surface of sea-water by being motionless. Those in the scum, notwithstanding their length and the number of spores in them, were moving freely about the field.

Having satisfied myself that, under certain conditions, the rods of *B. termo* lengthened into filaments, and that the spores formed in them when set free germinated into short rods, I next directed my attention to *Micrococcus*, in order, if possible, to make out whether it was a distinct organism, or whether it was simply a phase in the life-history of some common *Bacterium*, *e.g.*, *Bacterium termo*. After making numerous preparations, I at last succeeded in getting a cultivation in which only *Micrococci* were present. The cultivation was made by adding to a drop of *humor aqueus* from a fresh ox's eye a minimal quantity of pus from a newly opened abscess on the point of a calcined needle. For three days there was no indication of organisms, but on the fourth, small moving particles were visible, which, when examined with a No. XII immersion (Hartnack), were seen to be either round, oval, or dumbbell-shaped, and often in groups of two and four. (Series VI, *g*.)

A long and careful study proved that the different forms were all phases of the same organism ; the oval forms became dumbbell-shaped, and then divided into two round bodies similar to, but smaller than, the sporules of *Bacillus anthracis*. The two round bodies moved actively about till they separated from each other, when each became dumbbell-shaped and divided as before. Being apparently in a suitable medium and at a favourable temperature, they were extremely active and increased so rapidly that they soon formed a large milky spot in the centre of the cultivation liquid. This cloud, when examined with a low power, was seen to dip well down into the fluid, thus corresponding to the plug of *Bacteria* found by Professor Tyndall in several of his test-tubes.*

Though kept under observation for three weeks, not one of the round or oval organisms present ever germinated into a rod. During the fourth week three fresh drops of *humor aqueus* were infected with the smallest possible number of *Micrococci* from the above preparation ; also several flasks containing a sterilized turnip infusion. Both in the aqueous humour preparations and in the turnip infusion they rapidly increased, but none of them germinated into rods. This experiment was again repeated with a like result. Hence, having failed to find *Micrococcus* developing into Bacterial rods, it may, in the meantime, be inferred that it is a distinct form : or just as *Torula* may be an arrested phase of some *Penicillium*-like organism, so may *Micrococcus* be the spore of a *Bacterium* which has either altogether lost its power to germinate, or can only do so under very peculiar conditions. That *Micrococcus* closely resembles *Torula* will be at once apparent.

* " Phil. Trans.," 1877.

Effects of Desiccation and of different Temperatures on Bacterium termo and Micrococcus.

If the oil be removed by blotting paper from between the glass ring and the covering-glass of a preparation made as above described, or if the covering-glass be fractured without being displaced, the cultivation liquid rapidly evaporates, and the remains of what a few minutes before were active organisms are in great part left adhering to the under surface of the covering-glass. Preparations treated in this way may be either subjected to high or low temperatures; or, when protected by a glass cap, may be left in the ordinary atmosphere. The result of desiccation was ascertained by infecting flasks containing sterilized organic infusions. Such flasks infected with rods desiccated at 20° C. remain sterile, but flasks infected with desiccated spores soon teem with *Bacteria*, and flasks infected with desiccated *Micrococci* soon teem with round, oval, and dumbbell-shaped organisms, leading to the conclusion that desiccation destroys Bacterial rods; but that, though continued for weeks, it has no influence on spores or *Micrococci*. If *Micrococci* and the spores of *B. termo* are not destroyed by desiccation in a small protected atmosphere, it may be further inferred that they retain their viability when dried in the ordinary atmosphere, and, being extremely small and light, that after they are dry they will float about along with other solid particles in disturbed, and settle down in quiet, atmospheres, without undergoing any change until they find themselves in a medium which admits of their growth and development. In all probability desiccation destroys the oval and dumbbell-shaped forms of *Micrococcus*, the round spore-like forms only retaining their vitality. That spores exist in our ordinary atmosphere may be easily proved by placing sterile organic infusions in different parts of a building. In such a building as University College a considerable number of different organisms may be found. In cold rooms the infusions may remain sterile for a considerable time, but generally a scum soon appears on the surface, which, on examination, may contain, besides *Bacteria*, fungi, monads, and other low forms from both the animal and vegetable kingdom. However readily the unprotected rods of *Bacteria* are destroyed when desiccated at a temperature of 30° C., they are not destroyed in the substance of a spleen or kidney when the temperature is raised to 40° C. Those in the outer part of the dried portion of spleen or kidney are destroyed, but those in the centre are protected by the hard outer cake, not only because this hard shell tends to keep the heat from them, but especially because it tends to prevent complete desiccation.

Effects of Ebullition.

Along with Dr. Burdon Sanderson,* I have shown that the spores

* "Quart. Journ. Micro. Science," April, 1878.

of *B. anthracis* are destroyed when the fluid they are suspended in is kept for a few minutes at the point of ebullition. The same is true of *B. termo* and *Micrococcus*. On the other hand, when they are subjected to a temperature of 110° C. in a dry state they are not destroyed; they are rendered inactive, however, by a temperature of 120° C. The difference between the effects of moist and dry heat is probably owing to the gelatinous capsules of the spores and *Micrococci* giving way, and thus allowing the boiling fluid to come into direct contact with the unprotected central protoplasm.

Further Observations on Bacillus.

On the surface of sea water, containing the remains of seaweeds, Pedicellinæ and skeleton shrimps, brought from Roscoff by Mr. Geddes last April, a few patches of scum appeared which, on examination, were found to be made up of active and resting *Bacilli*, exactly resembling in size and form *Bacillus subtilis*. Day by day the small pellucid areas increased until the whole surface of the water (about 12 centimetres in diameter) was completely covered with a thick opaque scum, which, after remaining entire for seven days, gave way in the middle, and soon sank to the bottom, remaining there in a torn and broken condition.

The minute spots which first appeared were almost entirely made up of active *Bacilli*, rapidly multiplying by transverse fission. As the patches increased in size, the *Bacilli* either formed zooglœa, or lengthened into filaments. The process of lengthening was, as compared with *B. anthracis*, when cultivated on the warm stage, a remarkably slow one, and often the filaments which with the No. VIII Hartnack were apparently 3 or 4 centimetres in length, moved in a languid way amongst the active *Bacilli*. Having increased to about forty times the length of the original rod, the protoplasm divided and condensed into spores, the steps of the process being similar to those already described in *B. anthracis*.* The spores next escaped from the filaments, and either formed zooglœa, or germinated into another generation of rods. This increase of the rods by simple division, and by spore formation, continued until the whole of the surface of the water was covered by the scum.

On examination, six days after the scum sank to the bottom, an immense zooglœa was found made up of a relatively small number of quiescent rods, embedded in a thick transparent matrix (Series I, *j*). But of especial interest were a considerable number of large and small granular masses, made up of minute round particles, the smaller ones resembling the "cell families" of *Ascococcus*,† and the larger ones

* *Loc. cit.*

† Cohn, "Beit. zur Biol. der Pflanzen."

the peach-coloured granular disks figured by Lankester from old cultivations of *Bacterium rubescens*, in which "the nourishment had dwindled to its very smallest limit."

In the zooglœa all the *Bacilli* were quiescent, and it is important to observe that in none of them was there any appearance of division; but around the edge (Series I, *k*) some were in active motion, whether entering or leaving the motile stage I do not know. The close resemblance of these masses, both in form and in time of appearance to the "macroplasts" of Lankester,* led me to watch their development. First, the spore divided into four sporules quite as in *Bacillus anthracis*,† but these sporules again divided forming a granular mass (Series IV, *d*, *e*), division and growth going on simultaneously till a large very finely granular sphere was produced. (Series IV, *g*.)

When one of the large spheres was broken up, round particles (Series IV, *h*) spread far and wide over the field. These particles, when placed in a fresh drop of sea water enlarged (Series IV, *i*) and germinated into rods. (Series IV, *j*.)

If then a single minute spore is thus capable of producing innumerable still more minute germs, and if these, as all experiment tends to show, resist desiccation at ordinary temperatures, Professor Huxley's dictum may unhesitatingly be repeated and endorsed, that, considering the lightness of *Bacterium* germs, and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.

Morphological Considerations.

Various investigators, notably Huxley and Lankester, have long ago asserted the Protean nature of *Bacteria*, and the accompanying plate (exclusively compiled from actual observation), is an attempt to summarise and define what we at present know of the phases through which three of these forms, *Bacillus*, *Bacterium*, and *Micrococcus* may pass. Such a diagrammatic representation may be the more useful, seeing that at present our knowledge is scattered through many papers.

Series I represents the most common phases through which *Bacillus* may pass. The spore germinates into a rod, this divides, the portions, if at rest, either falling apart or forming a jointed filament (*f*). The motile stage may be assumed, during which division also goes on (*g*), *zigzag forms being produced which have often been mistaken for Vibriones*. While division is going on, the development of the cilia is often beautifully seen with a high immersion. First, the cellulose

* "Quart. Journ. Micro. Science," vol. xvii, New Series, p. 27, Plate III.

† *Loc. cit.*

wall gives way, and the segments as they separate slowly draw out a thin viscous thread of protoplasm. One segment generally fixes itself to the cover glass, while the other wriggles about in all directions, moving and resting by turns, until the almost invisible thread gives way in the middle so as to form two cilia. Thus the first formed motile rods should only have one cilium, and such are occasionally seen, but are probably soon supplanted by their more active biciliate progeny. After being alternately at rest and in motion for an indefinite period, they may rise to the surface of the fluid to form a zooglœa (*j*) and after some time again become motile.

In Series II the spore immediately after germinating develops into a long filament or unbranched hypha, which interweaves with others to form a mycelium. The protoplasm of the filaments soon contracts into rows of "chlamydospores" (*f*) which either escape through the cellulose walls, or are set free by the disintegration of the hypha (*i j k*). When once free they may either form a zooglœa (*m*) or germinate immediately.

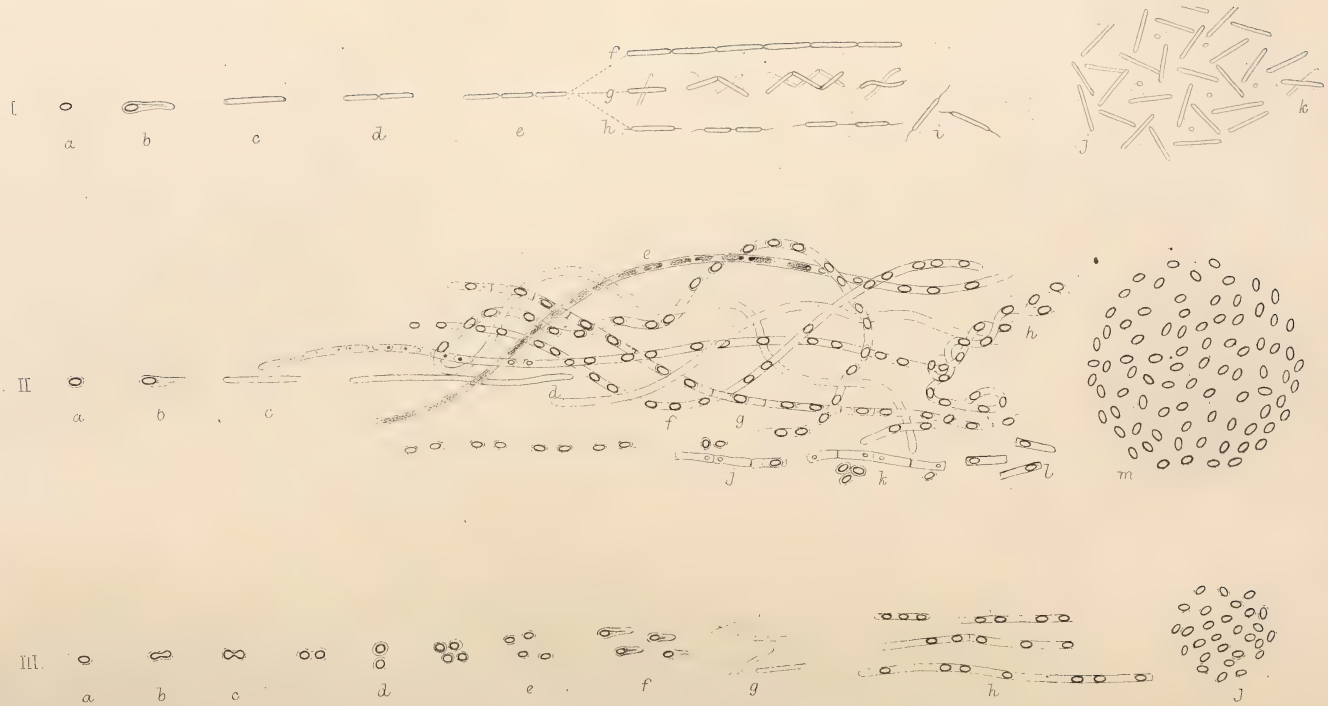
In Series III the spore divides into four sporules (*e*), which, while separating, move very energetically. This takes place when the nutritive fluid is becoming exhausted, the sporules not germinating until more nourishment is obtainable. If fresh pabulum be not added the sporules may continue dividing (Series IV) so as to form large finely granular spheres (*e f g*), compound masses resulting from the division of several adjacent spores. These disintegrate, setting free numerous small round particles (*h*) which in suitable media enlarge and germinate (*j*).

Leaving *Bacillus*, Series V shows the life-cycle of *Bacterium termo*. The round spore germinates into a short rod (*b*), which either divides into a chain (*e*), or into separate rods, with resting and motile stages alternating (*f g*), while the short rods may lengthen into delicate spore-bearing filaments (*h i*). The thorough correspondence of all this with Series I and II of *Bacillus* is very obvious and suggestive. Moreover, these spores (*j*) exactly resemble the *Micrococcus* represented below in Series VI.

Here the spore-like *Micrococcus* becomes dumbbell-shaped and divides into two (*c*), which again divide, and so on indefinitely, a preparation of *Micrococci* kept for three months having shown no tendency to germinate into rods. Resting and motile stages (*g*), chains (*f*), and zooglœa (*h*) are often observed.

EXPLANATION OF THE PLATE.

Series I—IV. Phases in Life-History of *Bacillus*.
„ V. „ „ *Bacterium termo*.
„ VI. „ „ *Micrococcus*.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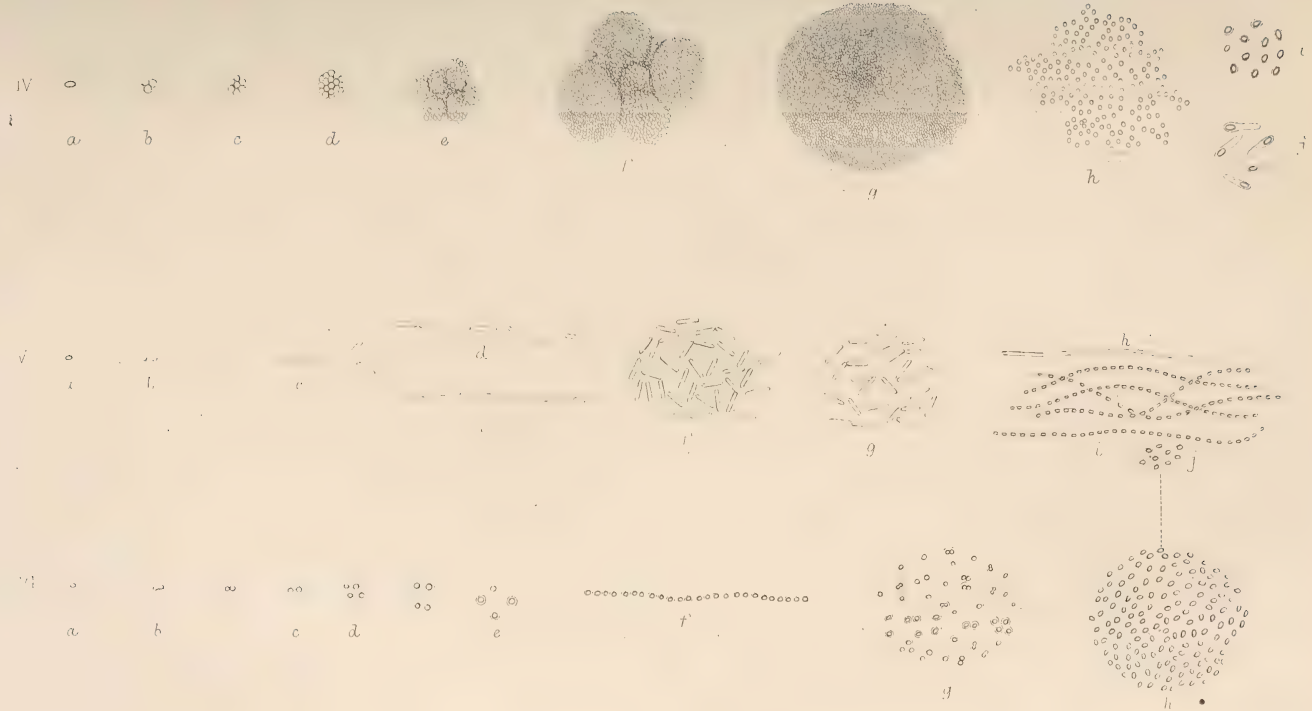

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IX. "On the Life-History of Spirillum." By Patrick Geddes and J. Cossar Ewart, M.D. Edin., University College, London. Communicated by Professor Huxley, Sec. R.S. Received June 20, 1878.

[Plates 11, 12.]

Notwithstanding the numerous and fruitful researches which have been recently made into the life-history of *Bacteria*, our knowledge of the common and interesting curved and spiral forms—the *Vibrio** and *Spirillum* of Ehrenberg—has made little or no advance since his time, neither embryonic nor reproductive forms having ever been observed; while even the zooglœa phase, so characteristic of *Bacterium* and *Bacillus*, has only once been mentioned,† and then in a different form.

A fresh-water aquarium, which has been stagnating since last summer in the Physiological Laboratory of University College, contained in winter vast numbers of ordinary motile *Spirillum*. On recently re-examining the water, one zooglœa film after another having in the meantime formed on the surface, thickened, broken, and sunk, we found that these motile forms had almost disappeared, while the films consisted almost entirely of resting *Spirillum* in a gelatinous-looking matrix, similar to that of *Bacterium* and *Bacillus*. Among these were two or three apparently distinct kinds of filaments, some resting and colourless, others motile, and filled with highly refracting bright yellowish-brown spheres. Such a field is represented in fig. 1.

The glœa had, even to the naked eye, a brownish tint, which under the microscope was distinctly traceable to the *Spirilla*, the matrix being also faintly tinged. When a fragment was mounted, water very rapidly revived the perfectly quiescent organisms at the torn edges. These, after some efforts, succeeded in disengaging themselves, and in a few seconds were darting to and fro, fully motile. The rapidity and extent of this change from the resting to the motile condition, which we have witnessed again and again, was of extraordinary interest and beauty. When crowded into a narrow space, the motile *Spirilla* showed their bright brown madder tint very intensely—a single one, unless very carefully examined, appearing colourless (fig. 1).

The resting *Spirilla* are of very various shapes, some singly bent, others slightly curved at one or both ends, and others coiled like corkscrews of two or more turns (fig. 1). We were much astonished

* We are very strongly of the opinion that the forms described by various authors as *Vibrio* are merely either—(1.) Zigzag dividing *Bacillus*; (2.) Slightly waved *Bacillus*; or (3.) Undeveloped *Spirillum*, and hence that *Vibrio* should no longer be used as a generic term.

† Lankester, "Quart. Journ. Micr. Sci.," vol. xiii, p. 424.

to note that amongst these lay short, comma-shaped forms (fig. 11), the head of the "comma" exactly resembling in size and colour the yellowish-brown spheres contained in the long motile filaments above mentioned. Every possible gradation in size and curvature existing between the smallest comma and the longest spiral, the explanation at once suggested itself that here we had the germination of spores into *Spirilla*. The adult resting forms might often easily be mistaken for germinating spores; but, by careful focussing, the apparent spore is resolved into a mere twist or upturned end, and we have therefore specially studied and carefully figured such possible sources of fallacy (fig. 12).

The spore-bearing hyphæ, to the study of which we were thus led, may be next described. They were of enormous length and constantly in motion, the shorter progressing with a strange, unsteady, forward movement; the longer, like intertwisting snakes, convoluting into loops, knots, and spirals, to form a motile mycelium (fig. 1). After remaining entire for a considerable time they break up into longer and shorter, still motile, segments which may again divide (fig. 7). We have repeatedly observed a filament, as it crept along, sowing its own spores, which either escaped at the end or through the walls, leaving slightly smaller clear vacuoles in the cellulose, to show where they had lain (fig. 7*a*). In all other cases in the vegetable kingdom, so far as we know, save two other Bacterial forms,* the spores or seeds may have the means of active or passive locomotion, but the parent organism, at least, is always quiescent; here the reverse holds good, but the same end is gained—the parent being locomotive and the spores quiescent.

The development of the filaments and their relation to the resting and motile spirals we next endeavoured to ascertain. In various preparations, especially in those kept at a temperature of 25° C., we found some long irregularly-curved *Spirilla* (fig. 2), and associated with these, and often scarcely distinguishable from them, were delicate wavy filaments, which were slowly changing their form (fig. 3), sometimes straightening, sometimes looping, but often recurring to the *Vibrio*-like type (fig. 3). These increased in length and thickness, and became motionless (fig. 4*b*), and their protoplasm gradually condensed into clear round spheres, which were at first almost colourless, but became tinged with yellow, which deepened into the characteristic brown, the filament then returning to the motile condition. The young spores were at first arranged with some regularity (fig. 4*c*), but this disappeared as they ripened and divided. The process of division is very remarkable, dumbbell-shaped, triradiate, and budding masses occurring together in the same filament, the resulting spores being

* (1.) *Bacterium rubescens.* Lankester, "Quart. Journ. Micr. Sci.," vol. xvi. (2.) *Bacillus* from sea water described by one of us in the preceding paper.

often unequal in size (fig. 6). Such irregularity of division very often also takes place outside the filaments (fig. 14).

When naturally sown in the glœa, in the way above described, the spores encapsulate themselves, and very often, if not always, divide into two or more sporules (fig. 8*a*). The capsules may remain isolated or also divide, forming patches of two or three (fig. 8*b*), or along with others, uniting into large irregularly cohering masses (fig. 13*c*). Sometimes, also, many may be enclosed in a definite cellulose envelope (fig. 8*d*); this last perhaps arising, like a colony of *Glœocapsa*, from the division of a single spore. When old, the capsules may lose their pale blue colour, and even become suffused with dingy brown.

The sporules may either desert the capsules (fig. 8*e*), leaving vacuoles, as in the ripe filaments, such empty capsules being found in great numbers; or may cause them to become actively motile, their motion being either corkscrew-like or direct without revolution; in the former case resembling a *Spirillum*, in the latter a monad. On escaping from the capsule, the sporules appear generally to rise to the surface, forming large dark granular masses (fig. 10).

A nutritive fluid, prepared by boiling a piece of zooglœa in some water taken fron the aquarium, was inoculated with these surface sporules and placed on the warm stage. Twenty-four hours after, many of the brown sporules were sending out a small curved hypha, at first slightly tinged, while later, distinct "commas," and even young *Spirilla*, were developed.

It is of great interest that we occasionally found a large, very finely granular sphere of the characteristic colour (fig. 9), while smaller and coarser masses were more abundant (fig. 9*b*), affording a transition to the common sporule cyst. These correspond to the smaller "macroplasts" figured by Lankester,* in his *Bacterium rubescens*, and to the finely granular colourless spheres, figured by one of us in the preceding paper† from a species of *Bacillus*. These probably result, as in *Bacillus*, from the long-continued subdivision of a single spore or sporule.

The division of the spores inside and outside the filaments is of great interest, and goes far to prove the unspecialised and Protean nature of Bacterial forms, almost all possible modes of division being found in the same field. Some divide transversely into two equal parts, others bud like *Torulæ*, others again lengthen into rod-like and *Vibrio*-like forms, and then break up into three or four portions (figs. 13 and 14).

The life-history of *Spirillum*, so far as we at present know, may be thus summarised. The well-known motile corkscrew may alternate

* "Quart. Journ. Micr. Sci.," vol. xvi, Plate 3.

† Plate 10, Series IV, *e f g*.

between the active and resting states, and ultimately lengthen out into a small filament, which loses its definite twist and may freely bend or straighten. This thread grows into a much larger and longer motionless filament, in which spores appear. These rapidly divide and acquire a bright brown colour, the filament reassuming the motile condition, and sooner or later breaking up. The freed spores encyst and divide, forming capsules, which after a period of quiescence themselves become motile, the sporules contained in them escape and germinate into "commas," which become *Vibrio*-like, and soon grow into the common motile *Spirillum*.

The resemblance of all this to the life-history of *Bacterium termo* and *Bacillus* described in the preceding paper is at once apparent. Not only is there the same alternation of a resting with a motile phase, but there is a lengthening into filaments the protoplasm of which condenses into spores which divide and germinate. Moreover, there are also moving filaments, and finely granular spheres, while the resemblance to *B. rubescens* is even more striking. That the deeply coloured spherules, figured by Lankester in the filaments and capsules, and described as "loculi," as well as the so called "sulphur-granules" of Cohn,* correspond to our "spores" is extremely probable, although their germination has not yet been observed.

EXPLANATION OF PLATES.

Plate 11.

Figure 1. The left half of the drawing represents a portion of a large glœa film of resting *Spirillum*, containing also (1) a motile mycelium of spore-bearing filaments, (2) many spore capsules, grouped and isolated. On the left are free active *Spirilla*, which have disengaged themselves from the film; at (*a*) they are crowded, showing a distinct brown colour; at (*b*), where the liquid is drying up, is shown the intertwisting of the longer spirals, which not unfrequently occur. Many motile spore capsules are shown, also a filament breaking up.

Figure 2. *Spirilla* beginning to lengthen into filaments.

Figure 3. Young filaments still freely motile.

Figure 4. (*a.*) Similar young motile filaments.
(*b.*) More fully developed filaments, no longer motile.
(*c.*) A filament, of which the protoplasm is condensing into round faintly-coloured spores.

Figure 5. Two successive drawings of a ripe motile filament.

Plate 12.

Figure 6. Part of a mature motile filament, showing the irregular size and arrangement of the budding and dividing spores, also empty spaces whence spores have been dropped (*a*).

Figure 7. Successive sketches of a filament breaking up. (*a.*) Spaces occupied by spores.

* "Beiträge zur Biol. d. Pfl.," 3, 1875.


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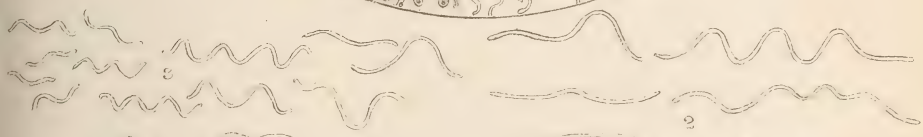
2


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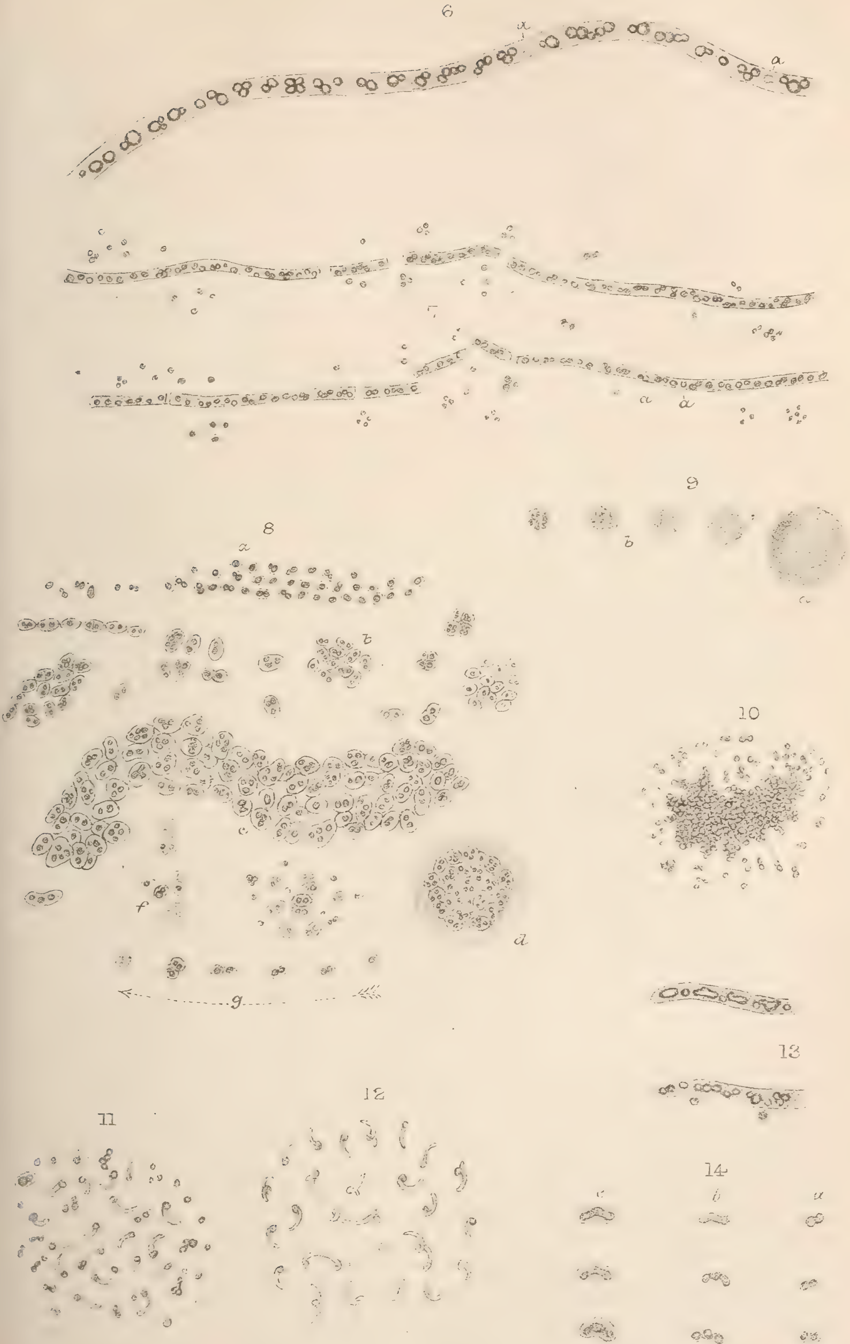

P. Geddes & J.C. Ewart del.

Figure 8. (*a*.) Spores encapsulating and dividing.
(*b*.) A small group of capsules.
(*c*.) A large group of capsules.
(*d*.) A group of capsules surrounded by a common envelope.
(*e*.) Sporules escaping from their capsules.
(*f*.) Large empty capsules.
(*g*.) Phases undergone by a single capsule.
Figure 9. (*a*.) Large finely granular brown sphere.
(*b*.) Smaller coarsely granular spheres.
Figure 10. Patch of naked spores from surface of liquid.
Figure 11. Spores germinating into *Spirilla*, at first comma-shaped.
Figure 12. *Spirilla* resembling germinating spores.
Figure 13. Successive drawings of a portion of same filament, showing extreme irregularity of division of spores.
Figure 14. Irregular division outside filament—(*a*) and (*b*), naked; (*c*) within capsule.

X. "On an Easy and at the same time Accurate Method of Determining the Ratio of the Dispersions of Glasses intended for Objectives." By Professor G. G. Stokes, M.A., Sec. R.S. Received June 18, 1878.

In examining the dispersive powers of a great variety of glasses prepared by the late Rev. W. Vernon Harcourt, I had occasion to examine several prisms which were too much striated to show clearly even the boldest dark lines of the solar spectrum. I found that I was able to get a fair measure of the dispersive powers even of these by a method depending on the achromatizing of one prism by another. If the method succeeded even with such prisms, it stands to reason that it would be still more successful with prisms of good glass.

For the construction of an objective we require but one datum as regards the dispersions, namely, the ratio of the dispersions, or rather, the ratio which on being treated as if it were the ratio of the dispersions gives the best results in practice.

If it were not for irrationality, the matter would be comparatively simple. The ratio of the dispersions would then be the same for whatever interval of the spectrum it were taken; and we should merely have to take two well-defined lines, bright or dark, situated as nearly as may be at the extremities of the spectrum, so that any errors of observation should be divided by as large a quantity as practicable, to measure the refractive indices of the two glasses for each of those two lines, and to take the ratio of the increments in passing from one line to the other. But in consequence of irrationality we get a different ratio according to the particular interval we choose; we are obliged, unless we adopt some different method altogether, to observe more than two lines in each glass; and when we have got the results,

that is to say, the indices for several fixed lines in each glass, it still becomes a question what it is best to do with them so as to produce the best result.

Fraunhofer proposed an empirical rule for combining the results,* but remarked that the number thus obtained was not exactly that which answered best in practice. I have elsewhere shown reason to think that the rule may be taken to be simply that the ratio of dispersions to be chosen is that for an infinitely small portion of the spectrum at its brightest part;† or in other words, that the focal length of the combination of two glasses must be made a maximum or minimum (it is practically a minimum) at the brightest part of the spectrum.

The refractive index of one glass may be expressed in terms of that of another, or of some standard glass, by an interpolation formula with three, or at most four, terms. The most accurate observations with prisms are only just sufficient to show that a fourth term is needed, and for the practical purpose of the construction of object-glasses, where in consequence of irrationality the ends of the spectrum are sure to be a good deal out of focus, it will be amply sufficient to confine ourselves to three terms. The three coefficients in the interpolation formula may be determined by observing in each glass the indices of refraction for three selected lines, though it is well to observe more than three lines, and combine the results.

The angular extent of the spectrum being but small in practical cases, the necessity of determining three constants by observations made in it requires great delicacy of measurement. Small errors of observation would easily produce an error in the deduced ratio which would be sensible, or even material, in practice.

For the actual construction of an object-glass we require, as has been already remarked, the knowledge of only a single constant relating to the dispersion, not of two ; and if we can find some test whereby to know when the required condition is satisfied, we may dispense with such extreme accuracy in the angular measurements.

Such a test is afforded by the secondary tints, which change with extreme rapidity when the refracting angle of one of the mutually achromatizing prisms, or the position of one of them in azimuth, is altered. If a moderately narrow object with vertical sides, black on a white ground, or white on a black ground, be viewed through opposed prisms, one of crown and one of flint glass, with a small telescope, and the prisms be set to achromatize each other as nearly as may be, it will be found that the slightest touch altering one of the prisms in azimuth alters notably the secondary tints.

The secondary tints in an objective are readily shown by directing

* "Denkschriften der K. Akad. der Wiss. zu München," vol. v, p. 215.

† "Report of the British Association for 1855," part ii, p. 14.

the telescope to a vertical line separating light from dark, such as the edge of a chimney seen in the shade against the sky, and covering half the object-glass with a screen having a vertical edge. So delicate is this test that on testing different telescopes by well-known opticians, a difference in the mode of achromatism may be detected. The best results are said to be obtained when the secondary green is intermediate between green and yellow. This corresponds to making the focal length a minimum for the brightest part of the spectrum.

To enable me to form a judgment as to the sharpness of the test furnished by the tint of the secondary green, as compared with the performance of an object-glass, I tried the following experiment.

A set of parallel lines of increasing fineness was ruled with ink on a sheet of white paper, and a broader black object was laid on it as well, parallel to the lines. The paper was placed, with the black lines vertical, at a considerable distance in a lawn, and was viewed through two opposed prisms, one of crown glass and the other of flint, of such angles as nearly to achromatize each other in the positions of minimum deviation, and then through a small telescope. The achromatism was now effected, and varied in character, by moving one of the prisms slightly in azimuth, and after each alteration the telescope was focused afresh, to get the sharpest vision that could be had. I found that the azimuth of the prism was fixed within decidedly narrower limits by the condition that the secondary green should be of such or such a tint, even though no attempt was made to determine the tint otherwise than by memory, than by the condition that the vision of the fine lines should be as sharp as possible. Now a small element of a double object-glass may be regarded, so far as chromatic compensation is concerned, as a pair of opposed prisms; and therefore we may infer that the tint of the secondary green ought to be at the very least as sharp a test of the goodness of the chromatic compensation as the actual performance of the telescope. And such Mr. Thomas Grubb, to whom I mentioned the test, found to be actually the case in the progress of the construction of the 15-inch refractor for the Royal Society, the instrument at present in the hands of Mr. Huggins.

It follows therefore that two opposed prisms, representing the glasses to be employed in an objective, are to be deemed to achromatize each other when one of the two secondary colours is about midway between yellow and green.

The condition of achromatism of two opposed prisms is given in the ordinary treatises on optics, but, so far as I have seen, rather as an optical curiosity than as a matter of practical utility. Sir David Brewster in his treatise on "New Philosophical Instruments," p. 292, alludes to it as furnishing a conceivable mode of determining dispersive powers, but mentions it only to condemn it. I cannot imagine why, for at least with the modifications which I have been led to in-

troduce in putting it in practice, I find it to be no less excellent than easy.

The experimental arrangements are a good deal simplified by making the prisms to be compared achromatize successively one and the same prism chosen arbitrarily, and retained in a fixed position, instead of making them directly achromatize each other. I will first describe the method as I used it, premising that as the prisms for which it was primarily designed were not of good glass, I was content with less perfect arrangements than would have been desirable for really good glasses. Nevertheless the apparatus when used with good prisms gave very good results. The object observed was a vertical slit, so wide that when viewed through the opposed prisms a broad white stripe was seen, merely fringed at the two edges with the secondary green and purple. The slit was fixed at one end of a horizontal plank, near the other end of which was fixed a vertical lens of about four feet focus. The slit was in the principal focus of the lens. The plank rested on three knobs, one under the slit, the other two near the other end. This mode of support prevented torsion of the plank, which would have produced a lateral derangement of the slit. The rays from the slit, rendered parallel by the lens, fell upon a prism, which I will call the primary prism. This prism is fixed during the observation. Its angle and dispersive power, and the azimuth in which it is set, are arbitrary within wide limits.

The beam emerging from the primary prism falls, at a little distance, on a second prism resting on a stand movable along with the verniers of a protractor reading to minutes (for the loan of which I am indebted to Professor Miller), which latter rests on the plank. As a matter of convenience, the prism has an independent motion in azimuth, but when once placed in a convenient position it is not afterwards moved except along with the verniers.

The beam refracted and dispersed in contrary directions by the two prisms is received on the object-glass of an achromatic telescope unconnected with the graduation, with which the image of the slit, if so wide an aperture may so be called, is viewed. In general the edges of the aperture are seen coloured blue and red, which colours, on turning the prism through its azimuth of achromatism, are changed into red and blue by passing through the secondary green and purple. That edge of the slit at which the green is seen is alone attended to, and is treated as the fiducial edge. The prisms are deemed to achromatize each other when this colour is midway between yellow and green.

Should the observer wish to aid his judgment by observing the purple as well, it would be proper to use an aperture of the form represented in the figure, so that the green and purple should be seen right and left along a common edge, and therefore at the same angle of incidence, or rather at angles of incidence having the same hori-

zontal projection. I do not know that anything would be gained by this; I have found it sufficient to attend to the green.

The light employed had best be that of the sky, reflected horizontally by a looking-glass. It should be fairly bright, but not approaching to dazzling. Thus the light reflected from the sky near the sun would be too bright. The room need not be darkened; in fact, it is better that the eye should be kept fresh for the appreciation of colour by habitually looking about on ordinary objects. A simple collimating lens such as I have described is sufficient, though doubtless an achromatic would theoretically be an improvement. It should be of longish focus, at least in the case of a simple lens, lest any slight displacement right or left of the middle of the incident beam should introduce a minute dispersion due to the lens acting as a prism. If the middle of the beam be not quite central, that does not signify, provided the eccentricity be constant; for then the minute dispersion is merely added to that of the primary prism, which is arbitrary. To ensure constancy of incidence on the lens, it is well to limit it by an aperture with vertical sides, and to take care that the beam employed is wide enough to fill the aperture. Similarly it is well to take care that the beam falls centrically, or pretty fairly so, on the achromatic viewing telescope. But what is of much more consequence is that the ray passing through the optical centre of the object-glass should pass centrically through the eye-piece, as otherwise the dispersion of the eye-piece for eccentrical pencils would alter the secondary tint. It is well, therefore, that the viewing telescope should be furnished with cross wires. The telescope is then moved a little till the fiducial edge is on the cross wires when the tint is observed.

The determination of the azimuth of achromatism is the capital observation on which the accuracy of the result depends; in comparison with it, the rest of the measurements required may be deemed exact. Accordingly a number of observations of this azimuth should be taken, and the mean adopted. The mean error will vary with circumstances, but it may be taken ordinarily as a few minutes.

The mean reading for achromatism by itself alone gives nothing, and must be combined with another determination in order to be available. The second determination which I found it most convenient to make was that of the angle of incidence of a particular part of the spectrum on the second prism for a known azimuth of this prism.

If the brightest part of the spectrum were marked by a definite line, we should choose that. As it is, the line D, though not exactly at the brightest part, lies sufficiently near it for our purpose, as will be better understood presently. No dark lines can be seen in the reflected light, because we are using an aperture, and not a mere slit. But if the slit be illuminated by a soda flame instead of daylight, a well-defined yellow image of the aperture will be seen, the fiducial edge of which can be pointed at with precision.

To determine the angle of incidence I have employed two methods, both of which I think it will be worth while to describe. The first is the less direct, and involves a little subsequent calculation, but has the advantage of not requiring any additional apparatus beyond what is wanted for the determination of the azimuth of achromatism. In carrying out the first, and in applying either, I suppose the angle of the prism and its index for the line D known from a determination in the usual way.

First Method.—The azimuth of minimum deviation for the line D could be determined pretty fairly by bringing the wire of the viewing telescope to the fiducial edge when the aperture is illuminated by a soda flame, and the edge is in or near its stationary position, and taking the mean of several determinations of the azimuth at which the edge is stationary. The angle of incidence for minimum deviation is virtually measured in the process of finding the index, and is therefore known; and by applying with its proper sign the difference of mean readings for achromatism with daylight and for minimum deviation with a soda flame, we get the angle of incidence for achromatism.

This method is mentioned merely as naturally leading up to that actually employed. It must be rejected as too slovenly, since the uncertainty of the determination of the azimuth of minimum deviation is liable to be greater than that of the azimuth of achromatism.

If we place the wire of the viewing telescope some way off the stationary position of the fiducial edge, there are two azimuths of the prism at which the edge will be on the wire, across which it will move with a finite velocity as the prism is moved in azimuth. Hence either azimuth could be determined with accuracy, and if they were equidistant from the azimuth of minimum deviation, the latter could be determined at once. This, however, is not the case, but nevertheless the azimuth of minimum deviation can be determined from the result.

Since the course of light may be reversed in refraction, it readily follows that in passing from one to the other of two azimuths which

give equal deviations, the angles of incidence and emergence are simply interchanged. Hence when the prism is turned from one to the other of two azimuths for which the image of the fiducial edge is on the wire of the viewing telescope, the angle moved through is equal to the difference between the angles of incidence and emergence in either position. Both azimuths, and therefore their difference, can be determined with accuracy, provided the azimuths be sufficiently remote from that of minimum deviation.

The process of observation, then, is this. Set the prism a good way, such as 10° or 15°, from the azimuth of minimum deviation, and read the graduation. Turn the viewing telescope till the fiducial edge is on its wire. Taking care to keep the telescope fixed, turn the prism through the position of minimum deviation till the edge is again on the wire, and read again.

Let ϕ, ψ be the angles of incidence and emergence or emergence and incidence, of which let ϕ be the greater. Let $2\ a$ be the angle, i, of the prism, $2\ \beta$ the sum of $2\ a$ and the minimum deviation, $2\ \gamma$ the measured angle through which the prism has been turned, or $\phi-\psi$. Since the sum of the internal angles$=2\ a$, we may represent them by $a+x$ and $a-x$. Let $\phi+\psi$ be denoted by $2\ y$. Then we shall have—

$$\frac{\sin(y+\gamma)}{\sin(a+x)}=\frac{\sin(y-\gamma)}{\sin(a-x)}=\frac{\sin\beta}{\sin a}.$$

Eliminating x from these two equations, we find—

$$\cos^2 y=\frac{\cos^2 a\cos(\beta-\gamma)\cos(\beta+\gamma)}{\cos(a-\gamma)\cos(a+\gamma)}\quad\ldots\ (1),$$

which gives y, and then $y\pm\gamma$, *i.e.*, ϕ or ψ, is known. The angle of incidence for a known reading of the graduated circle being known, we have only to apply the difference between this reading and that for achromatism to that angle of incidence in order to get the angle of incidence for the azimuth of achromatism.

Second Method.—In this a little telescope is used, which is attached to the stand of the prism, so as to move with the verniers of the circle. The telescope need not be achromatic, but has cross wires in its principal focus.

After determining the azimuth of achromatism, the slit is illuminated by a soda flame, and the prism with the verniers turned till the fiducial edge is on the wires of the measuring telescope, when the circle is read. The prism is then removed, and the measuring telescope turned till the fiducial edge is seen directly, and the circle is read again. Half the supplement of the difference of the readings gives the angle of incidence when the reflected image was on the cross wires; and by applying the difference of readings for reflection and for

achromatism, we get the angle of incidence in the position of achromatism.

This angle, ψ, having been determined by either of the above methods, we have, by the known formula—

$$\frac{d\mu}{\operatorname{cosec} i \cos\phi' \cos\psi} = \frac{d\mu_,}{\operatorname{cosec} i_, \cos\phi_,' \cos\psi_,'} \quad \ldots (2),$$

where the letters with the suffix , refer to the second prism. For the prisms would achromatize each other, as is supposed in the deduction of the above formula in treatises on optics, under the same circumstances in which they would achromatize, in succession, the same spectrum. In the application of the formula, it is to be remembered that, of the two angles ϕ, ψ, the former is that which lies on the side of the white light, and is, therefore, the angle of incidence for the first, but of emergence for the second, of two prisms which mutually achromatize each other.

In the application of the formula (2) the angles ϕ', ψ belong, strictly speaking, to the brightest part of the spectrum, which for shortness I will call M, for which the value of the differential coefficient $d\mu_,$: $d\mu$ is supposed to be sought. But the distance of M from D is so small that it will hardly make any sensible error if we use the values of the angles belonging to D, for not only is the correction to the product $\cos\phi' \cos\psi$ for either prism very small, but the two corrections are in the same direction, and therefore tend to neutralise each other in the ratio of the products, with which alone we are concerned. If, however, we care to introduce the correction, it can be done at the expense of a little additional calculation. In a crown glass the index for M may be taken as greater by 0·001 than that for D, and in a flint glass as greater by 0·001 multiplied by a rough value of $d\mu_,$: $d\mu$. The angle of emergence ϕ may be taken to be the same for M as for D. For the deviation, regarded as the function of the index, is a maximum or minimum for M; and D being so near M, the difference of deviations for D and M will be quite insensible compared with the errors of observation of the azimuth of achromatism, with which it is associated. Let the letters $'\mu$, $'\psi$, &c., refer to M, while μ, ψ, &c., refer to D. ψ is obtained by observation, and ψ', ϕ' must, in any case, be calculated from thence by the known values of μ and i. We have now merely to calculate the $'\phi'$ and $'\psi$ for each glass from the formulæ

$$\sin'\phi' = \frac{\mu}{'\mu}\sin\phi', \quad '\psi' = i - '\phi', \quad \sin'\psi = '\mu \sin'\psi',$$

and substitute these values in the equation (2) instead of those belonging to the line D.

The primary prism had best be made of very low flint glass (or else

be a compound prism formed of two prisms of crown and flint glass respectively, with their angles turned the same way), so as to fall about midway between the glasses to be compared, and thereby divide between them the irrationality which has to be encountered in the observation. The observation of the azimuth of achromatism is most accurate when there is little or no irrationality; and, if preferred, the crown glasses might be compared with a standard crown, and the flint glasses with a standard flint, the primary prism being in the one case any crown glass prism that happens to be at hand, and in the other case a flint glass prism. The crown glass to be measured being compared with the standard crown by using them in succession to achromatize the same primary crown in the same position, and similarly for the flint glass to be measured and the standard flint, we can deduce the ratio of the dispersions of the crown and flint glasses to be measured if we know, once for all, the ratio of the dispersions of the standard crown and flint glasses. This may be determined by a specially careful series of observations of the kind above described, made once for all, or, if preferred, by the method of indices.

The direct comparison of a crown and flint glass is, however, so accurate, especially if a glass of intermediate quality be used for the primary prism, that I feel satisfied it will suffice for practical purposes. It is hardly necessary to observe that, if the primary prism be of intermediate quality to the two compared, the right hand edge of the aperture will be the fiducial edge in the one case, and the left hand edge in the other. In saying this, I assume that we have not extravagant inclinations or differences of angles to deal with, for there is a *quasi*-irrationality observed even when two prisms of the same glass, but of different angles, achromatize each other, which is, however, ordinarily so small that it may be neglected in comparison with the real irrationality of the media.

By turning the primary prism into a different azimuth, or substituting a different primary prism, and repeating the observation, an estimate may be formed of the degree of reliance that may be placed on the results.

To give an idea of the degree of accuracy of which the results are susceptible, I subjoin a few numbers extracted from my note-book. The prisms designated H 74, H 88, H 98, were experimental prisms of phosphatic glass of different compositions. They were more or less striated, but were good enough to show the principal fixed lines of the spectrum. In the different determinations of the ratio of dispersions, the primary prism was set at different azimuths. In the calculation of (2) the indices for D were used. The differences from the means are exhibited.

H 74 to H 98, 1·882, 1·892; mean, 1·887; differences, − 0·005, + 0·005.

H 74 to H 98, 1·755, 1·761, 1·781; mean, 1·766; differences, — 0·011, — 0·005, + 0·015.

It will be seen that, even with prisms such as these, by taking the mean of different determinations, the uncertainty can hardly be as great as one half per cent.

Extremely small prisms are quite sufficient for the determination of the ratio of the dispersions of the glasses by the above method. It may, however, happen that an optician cannot afford to remove even so small a piece of glass from a disk intended for an objective, and has not a specimen of glass on the identity of which with the glass of his disk he can thoroughly rely. In such a case it is necessary to determine the optical constants of the disk by means of facets cut on the disk itself. A heavy and costly disk cannot be treated like a small prism, and mounted on a small graduated instrument in the manner I have supposed a small prism treated. To compare the ratio of the dispersions of two such disks, one of crown glass and the other of flint, the most convenient way would seem to be to leave the disk fixed, let the light pass through it first, and then achromatize it by a small prism of very low flint glass, mounted on a small graduated instrument in the manner already explained. The dispersions of the disks would be compared with each other by comparing them in succession with the same intermediate prism.

This, however, demands an additional determination beyond what was required in the other process, since the prism through which the light first passes is not the same in the two cases. The element which best lends itself to measurement is the angle of incidence on the first surface. The most convenient mode of measuring this must depend on the general disposition of the apparatus adopted to take the measurements for the angle of the disk and the deviation of some one line, which must be made in any case; it is accordingly best left to the choice of the observer.

XI. "On the Reversal of the Lines of Metallic Vapours." By G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. No. III. Received June 19, 1878.

In our last communication to the Royal Society we described certain absorption lines, which we had observed to be produced by the vapour of magnesium in the presence of hydrogen, and certain other lines which were observed when potassium, and others when sodium, was present, in addition to magnesium and hydrogen. These lines correspond to no known emission lines of those elements; but, inasmuch as they appeared to be regularly produced by the mixtures

described, and not otherwise, we could only ascribe their origin to the mixtures as distinct from the separate elements. It became a question of interest, then, whether we could find the conditions under which the same mixtures would give luminous spectra, consisting of the lines which we had seen reversed. On observing sparks from an induction coil taken between magnesium points in an atmosphere of hydrogen, we soon found that a bright line regularly appeared, with a wave-length about 5,210, in the same position as one of the most conspicuous of the dark lines we had observed to be produced by vapour of magnesium with hydrogen in our iron tubes. This line is best seen, *i.e.* is most steady, when no Leyden jar is used, and the rheotome (the coil we used has an ordinary self-acting one) is screwed back, so that it will but just work. It may, however, be seen when the coil is in its ordinary state, and when a small Leyden jar is interposed; but it disappears (except in flashes) when a larger Leyden jar is used, if the hydrogen be at the atmospheric pressure. This line does not usually extend across the whole interval between the electrodes, and is sometimes only seen near the negative electrode. Its presence seems to depend on the temperature, as it is not seen continuously when a large Leyden jar is employed, until the pressure of the hydrogen and its resistance is very much reduced. When well dried nitrogen or carbonic oxide is substituted for hydrogen, this line disappears entirely; but if any hydrogen or traces of moisture be present it comes out when the pressure is much reduced. In such cases the hydrogen lines C and F are always visible as well. Sometimes several fine lines appear on the more refrangible side of this line, between it and the *b* group, which give it the appearance of being a narrow band, shaded on that side. We have used various samples of magnesium as electrodes, and they all give the same results. We have also used hydrogen prepared and purified in different ways: hydrogen prepared by the action of zinc on dilute sulphuric acid, purified by an acid solution of bichromate or permanganate, and by potash, and dried by sulphuric acid; electrolytic hydrogen; hydrogen from dry formiate of soda and soda lime; hydrogen occluded by sodium and expelled by heat; and hydrogen occluded by palladium and expelled by heat. In the last two cases the whole apparatus was connected by fusion, and a Sprengel pump, also connected by fusion, employed to remove the air. In all cases the phenomena were the same.

In addition to the above-mentioned line, we observed that there is also produced a series of fine lines, commencing close to the most refrangible line of the *b* group, and extending with gradually diminishing intensity towards the blue. These lines are so close to one another, that in a small spectroscope they appear like a broad shaded band. We have little doubt that the dark absorption line, with wave-length about 5,140, shading towards the blue, which

we previously observed in our iron tubes, and described in our last communication, was a reversal of part of these lines, though the latter extend much further towards the blue than we had observed the absorption to extend. In fact, the bright lines extend somewhat more than half the distance between *b* and F, from 45 to 50 being visible, and placed at nearly equal distances from each other. They also commence close to the *b* group, *i.e.*, with a wave-length nearly 5,164, but the first two or three lines at that end are not so bright as those which immediately succeed them. The light giving these lines does not extend to more than a short distance from the electrodes, and is generally most conspicuous at the negative electrode. There is a difficulty in consequence of the flickering character of the discharge in getting any accurate measures of them, though they are bright enough, especially at the less refrangible end, to be easily seen. The comparative faintness of the light from the iron tubes appears to us almost sufficient to account for our not having seen the reversed lines so completely as the bright ones; nevertheless, it is quite in accordance with what we in other cases observed, to suppose that some of these lines may be more easily reversed at the temperature of the iron tubes than others.

XII. "An Experimental Investigation into the Velocities of Normal Propagation of Plane Waves in a Biaxal Crystal, with a Comparison of the Results with Theory." By R. T. Glazebrook, B.A., Fellow of Trinity College, Cambridge. Communicated by J. Clerk Maxwell, M.A., F.R.S. Received June 19, 1878.

(Abstract.)

In his report to the British Association in 1862, Professor Stokes called attention to the desirability of accurate measurements of the velocity of normal propagation of plane waves in a biaxal crystal, with a view to testing by the results Fresnel's theory of double refraction, and suggested then a method to determine this velocity. Let the crystal to be examined be cut into the form of a prism, two or more natural faces being left to determine accurately the position of the cut faces with reference to the axes of elasticity.

"Let us consider a plane wave of light passing through the crystal prism."

"Let V be the velocity in air, v in the crystal, let $\phi\,\phi'\,\psi\,\psi'$ have their usual meanings, let i be the angle of the prism, D the deviation of the wave normal after passing through the prism."

Let us observe the angle of incidence ϕ, and the deviation D.

Then ψ is given by the formula

$$\psi = D + i - \phi \quad . \quad . \quad . \quad . \quad (1).$$

"But without making any other supposition as to the law of double refraction, or assuming anything beyond the truth of Huyghen's principle, which, following at once from the superposition of small motions, lies at the base of the whole theory of undulations, we may at once deduce from the directions of incidence and emergence the direction and velocity of propagation in the crystal."

For we have—

$$\frac{\sin\phi}{V} = \frac{\sin\phi'}{v},$$

$$v \sin\phi = V \sin\phi' \quad . \quad . \quad . \quad . \quad (2),$$
$$v \sin\psi = V \sin\psi' \quad . \quad . \quad . \quad . \quad (3),$$
$$\phi' + \psi' = i \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Adding and subtracting (2) and (3),

$$v \sin\frac{\phi+\psi}{2} \cos\frac{\phi-\psi}{2} = V \sin\frac{\phi'+\psi'}{2} \cos\frac{\phi'+\psi'}{2},$$

$$v \cos\frac{\phi+\psi}{2} \sin\frac{\phi-\psi}{2} = V \cos\frac{\phi'+\psi'}{2} \sin\frac{\phi'-\psi'}{2}.$$

Dividing and recollecting (4),

$$\tan\frac{\phi+\psi}{2} \tan\frac{\phi-\psi}{2} = \tan\frac{i}{2} \cot\frac{\phi'-\psi'}{2},$$

$$\therefore \tan\frac{\phi'-\psi'}{2} = \tan\frac{i}{2} \cot\frac{\phi+\psi}{2} \tan\frac{\phi-\psi}{2}.$$

This gives $\frac{\phi'-\psi'}{2}$.

Combining with (4) we can get ϕ' and ψ', and then find the value of v from either (2) or (3). In practice the value of μ or $\frac{V}{v}$ was used.

In accordance with these suggestions I undertook a series of observations at the Cavendish Laboratory, Cambridge, which I propose to describe in the paper, adding moreover a comparison of the results with the theories of Fresnel and Lord Rayleigh. Fresnel is the only experimenter who has attempted to verify his theory by experiment, and his attempt affords no verification, for in applying it he used approximate results, and to the degree of approximation to which he went the theory developed by Lord Rayleigh (Phil. Mag., vol. xli, Series iv, 1871) leads to exactly the same equations to determine the velocity of propagation as were used by Fresnel, so that his results form equally a verification of Lord Rayleigh's theory.

The work was carried out on two pieces of aragonite supplied by A. Hilger, Tottenham Court Road. In both cases two of the faces marked $m\,m'$ (Miller's "Mineralogy," p. 567) gave the best reflexions, and were therefore reserved to afford means for the determination of the position of the artificial faces. We will consider the two separately.

The first crystal was cut at Professor Stokes' suggestion, so as to form two prisms.

The edge of one of these was nearly parallel to the axis b of the crystal, the mean axis of elasticity, so that the principal plane of the prism almost coincided with the principal plane AOC of the wave surface, and I was able to show that the error arising from supposing the coincidence to be exact would never amount to as much as ·00001.

The axis OA of the crystal almost bisected the angle of the prism, which was 42° 50′ 30″. The observations of deviation and incidence were made with a goniometer by Grubb, lent me by Professor Stokes. The circle, about a foot in diameter, was graduated on silver, and was read to 10″ by verniers.

Each observation was repeated two or three times on different occasions, and the mean of the results taken. It was rarely that two measures of the same quantity differed by 20″.

Careful precautions were taken to ensure the light passing in a principal plane of the prism.

The observations with this prism extended from about 8° on one side of the axis OC to 16° on the other, passing almost through the extremity of an optic axis. Observations were taken at angles of incidence, increasing uniformly by 2°, thus forming an arithmetical progression.

The values of μ for one wave were nearly constant, and varied but little from

$$1{\cdot}68125.$$

Let a, b, c, be the principal refractive indices. The series of values given above corresponds to the circle of radius b. So that

$$b = 1{\cdot}68125.$$

The values corresponding to the other wave varied considerably. According to Fresnel they ought to be radii vectores to an ellipse axes a and c.

a was determined by passing light along the axis OC, which was possible, the crystal having been cut with this object in view. I found

$$a = 1{\cdot}68580.$$

To find c recourse was had to the second prism, which had its edge nearly parallel to the axis of c. I found

$$c = 1{\cdot}53013.$$

The value for the angle between the optic axes, seen in air through a face normal to c is, from these values

$$31° \ 0' \ 0''.$$

Kirchhoff found by experiment—

$$30° \ 54'.$$

The agreement is fairly close, much closer than that given by the values of the principal indices as determined by Rudberg.

Having thus found a and c, we proceed to calculate the theoretical values of μ in different directions and compare with theory, with the following result:—For from about 8° on one side of the axis a to 10° on the other, theory and experiment agree closely. The difference only in two cases amounts to ·0001, and is sometimes positive, sometimes negative. [The error in experiment is certainly not so great as ·00005.]

But for the next 6° through which the observations extended, the differences continually increase, reaching ·00024 for the last observation, the experimental values of μ being uniformly greater than the theoretical.

So that the results of observation would be represented by a circle of radius 1·68125, and an oval curve with the same axes as Fresnel's ellipse, viz.,

$$a = 1{\cdot}68580$$
$$c = 1{\cdot}53013$$

which agrees closely with the ellipse for 10° on either side of the axis a, and for the rest of the arc observed lies outside it, the difference between the radii vectores of the two curves increasing as we recede from a.

The differences between experiment and Lord Rayleigh's theory increase much more rapidly, and amount, at the end of the arc of 16° observed, to ·00202, or about ten times as much as on Fresnel's theory. Thus Lord Rayleigh's theory differs from the truth by considerably more than Fresnel's.

The second prism was cut so as to have its edge nearly parallel to OC.

The parallelism, however, was not sufficiently exact to enable me to treat the principal plane of the prism as coincident with a principal section AOB of the surface of wave slowness. It was necessary, therefore, to determine the values of v_1 v_2, the velocities of normal propagation, from Fresnel's construction. Let the optic axes meet a unit sphere centre at the centre of the surface in OO′ respectively; let P be the part in which any wave normal meets the sphere OP$=\theta$ O′P$=\theta'$.

Then we may show that the values of v_1 v_2 are given by—

$$v_1^2=\frac{1}{b^2}-\frac{a^2-c^2}{a^2c^2}\sin\left\{\frac{\theta+\theta'}{2}-\mathrm{AO}\right\}\sin\left\{\frac{\theta+\theta'}{2}+\mathrm{AO}\right\},$$

$$v_2^2=\frac{1}{b^2}+\frac{a^2-c^2}{a^2c^2}\sin\left\{\frac{\theta-\theta'}{2}+\mathrm{AO}\right\}\sin\left\{\mathrm{AO}-\frac{\theta-\theta'}{2}\right\}.$$

The work extended over an arc of about 19°, with the following results:—

For one wave the agreement was close throughout. This section differed but slightly from the circular section of radius 1·53013.

For the other the differences were much greater.

The results of experiment were represented by a curve, which in the neighbourhood of the lesser axis of Fresnel's section lies within that section, cutting it at about the middle of the arc considered, and afterwards lying without it.

The excess of experiment over theory changes in the arc considered from

$$-\cdot0002 \text{ to } +\cdot0005.$$

This agrees with the result for the first prism in lying outside Fresnel's surface as we approach the major axis.

I then proceeded to estimate the effect of any possible errors made in the determination of a, b, c, or the position of the plane, and showed that no change at all within the limits of experimental error would reconcile theory and experiment more closely. As a test of the accuracy of the experimental work, it may be stated that a series of observations, taken at an interval of some three months previously to those described above, gave results which rarely differed from those results by more than

$$\cdot00004.$$

To proceed now to the second crystal. The measurements on it were made at an interval of nearly a year after those already described. The crystal was in the form of a hexagonal prism, the base of the prism being nearly perpendicular to c. This base was polished. The other end was cut so as to be inclined to the base at an angle of 35° 2′ 56″, the line of intersection of the faces of the prism thus formed being nearly parallel to that of m and c. One of the faces, m, was cut so as to be inclined to the oblique section at about 37°, the line of junction being again nearly parallel to that of m and c. So that I thus formed two prisms whose principal planes were nearly coincident, having one face in common.

By this means I was able to work over an arc which extended from the neighbourhood of the principal section AOC to more than 70° on the other side of it.

The planes cut the principal plane, AOC, in two points, L L′, such that—

$$\begin{aligned} \text{CL} &= 1^\circ\ 21'\ 42'' \\ \text{CL}' &= 1^\circ\ 15'\ 40'' \\ \text{ALP} &= 59^\circ\ 20'\ 11'' \\ \text{AL}'\text{P} &= 59^\circ\ 13'\ 2'' \end{aligned}$$

while the position of P, the normal to the common face of the two prisms, was given by—

$$\begin{aligned} \text{LP} &= 35^\circ\ 0'\ 19'' \\ \text{or L}'\text{P} &= 35^\circ\ 3'\ 14'' \end{aligned}$$

Let $\theta\ \theta'$ be the angles between any wave normal and the optic axes. The formulæ used to calculate $v_1\ v_2$ were—

$$v_1^2 = \frac{a^2+c^2}{a^2c^2} - \frac{a^2-c^2}{a^2c^2}\cos(\theta+\theta'),$$

$$v_2^2 = \frac{a^2+c^2}{a^2c^2} - \frac{a^2-c^2}{a^2c^2}\cos(\theta-\theta').$$

The values used for a, b, c, were—

$$\begin{aligned} a &= 1{\cdot}68560 \\ b &= 1{\cdot}68115 \\ c &= 1{\cdot}53013 \end{aligned}$$

These values differ slightly from those used in the first part of the paper, though not by nearly so large amounts as Rudberg found between the values for two different specimens of aragonite. He had differences of more than ·0004 ("Pogg. Annalen," xvii, 1). For the outer sheet, which differs least from a sphere, the results of theory and experiment agree fairly closely. But for the inner sheet the curve given by theory agreeing with that given by experiment at the extremity of an axis, lies outside of it throughout the whole of its course away from that axis. The difference is greatest at about 35° away from the principal section, AOC being there as great as ·0009.

From this point the differences decrease, and at the end of the arc considered, or 74° away from the same axis, the value is

·0003.

I found also that no changes in the values of a, b, c, would produce closer agreement, but that if we suppose the arcs LP, L′P, to be increased by 17′, and the angles ALP, AL′P, by about 1°, the differences between theory and experiment were reduced about ·00006, taken throughout the arc, being sometimes positive, sometimes negative.

I have shown, however, that this change in the position of P involves alterations in the measures by which its position relative to the faces mm' was determined, which are far in excess of any possible experimental error, and that the only way of accounting for them is by supposing the axes of elasticity of aragonite to be slightly variable in position relatively to the faces of the crystal.

On the whole, however, I prefer to regard Fresnel's theory as a close first approximation to the truth, and to look to the phenomena of dispersion to explain the variation from it.

I may perhaps be allowed to close with a suggestion, which, judging from the results of a few experiments I have already made, appears to have some basis of truth.

Let us suppose that in a crystal

$$\mu = a + \frac{b}{\lambda^2} + \frac{c}{\lambda^4} + \ .$$

where a, b, c, &c., are functions of the directions of vibration and propagation.

Let us suppose that for waves of infinite length Fresnel's construction is true, so that a is a radius vector of Fresnel's surface of wave slowness, and can therefore be calculated, and suppose we neglect the terms $\frac{c}{\lambda^4}$, &c. Observing the values of μ in this direction for different rays, we get

$$\mu_1 - a = \frac{b}{\lambda_1^2},$$

$$\mu_2 - a = \frac{b}{\lambda_2^2},$$

$$\mu_3 - a = \frac{b}{\lambda_3^2}, \text{ &c.}$$

So that

$$\frac{\mu_1 - a}{\mu_2 - a} = \frac{\lambda_2^2}{\lambda_1^2}, \text{ &c.}$$

The results of experiments on the rays C, D, and F, in two different directions give

$$\frac{\lambda_C^2}{\lambda_D^2} = 1{\cdot}2403.$$

$$\frac{\mu_D - a}{\mu_C - a} = \begin{cases} 1{\cdot}2875 \text{ (1st experiment).} \\ 1{\cdot}2770 \text{ (2nd direction).} \end{cases}$$

$$\frac{\lambda_D^2}{\lambda_F^2} = 1{\cdot}46978.$$

$$\frac{\mu_F - a}{\mu_D - a} = \begin{cases} 1{\cdot}47208 \text{ (1st direction).} \\ 1{\cdot}47348 \text{ (2nd direction).} \end{cases}$$

These numbers, especially the last, are sufficiently close to make it worth while continuing the observations.

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Zinno (Silvestro) Altri nuovi processi per ottenere Ossigeno a caldo ed a freddo e nuove teoriche relative. Memoria. 4to. The Author.

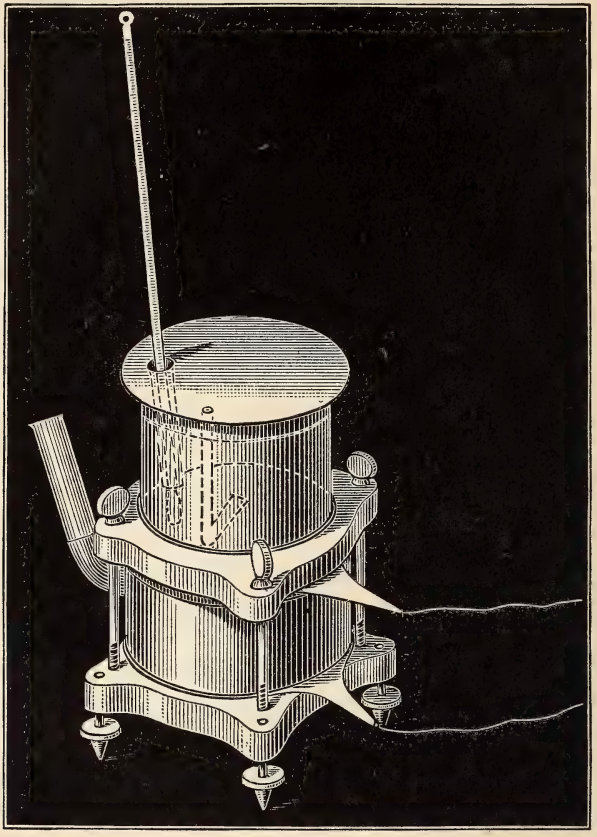
"On the Thermo-Electric Properties of Liquids." By G. Gore, LL.D., F.R.S. Received March 12, 1878.* Read March 28.

In the "Philosophical Magazine," January, 1857, I published an account of an investigation on the "Development of Dynamic Electricity by the Immersion of unequally-heated Metals in Liquids," and observed that, if we heat the upper end of a column of an electrically conducting liquid, the two ends of the column being bounded by plates of the same metal, an *upward* current of electricity is usually produced if the liquid has an *acid* reaction, and a *downward* one if it is *alkaline;* and I showed, in a brief manner, that this statement was true in various instances, provided chemical action was excluded. This latter condition was, in many cases, largely secured by employing platinum plates, and avoiding the use of such liquids as corrode that metal. I also stated that the currents thus produced did not appear to arise from any action of heat or other force in the *mass* of the liquid.

I have recently investigated this phenomenon more extensively, employing, in the first series of experiments, a similar apparatus to the one described in that paper, but of a much larger and more effective size, as shown below. The circular disks of metal which bounded the ends of the glass cylinder were $4\frac{3}{4}$ inches in diameter, and the column

* See *ante* p. 272.

of liquid was 4 inches in diameter and $1\frac{1}{2}$ inch high, and consisted of about 12 ounces by measure. The metals employed in this apparatus were in nearly all cases either platinum, gold, or palladium.*



The apparatus was made water-tight, by the aid of vulcanized india-rubber washers; but, as those washers affected the electric currents, they were covered on each side by others of pure india-rubber, somewhat wider than themselves, secured to them by means of spirit-varnish. In some cases, especially for the lower end of the cylinder, where the heat did not affect them, washers of the pure rubber alone were employed; and, for use with strongly alkaline solutions, the vulcanized washers were, in certain instances, previously immersed during five or six hours in a hot or boiling and strong solution of caustic soda, to remove sulphur, and then thoroughly washed. Nearly all the liquids were, previous to using, boiled, to expel dissolved air,

* Messrs. Johnson and Matthey were kind enough to lend me a valuable pair of palladium plates for these experiments.

cooled and filtered; the presence of air in the liquid was, however, found to interfere but little with the chief results, except with certain solutions, or unless the air was large in amount. It is necessary that the liquids be clear, for if a solid deposit settles upon the lower plate, it interferes with the current.

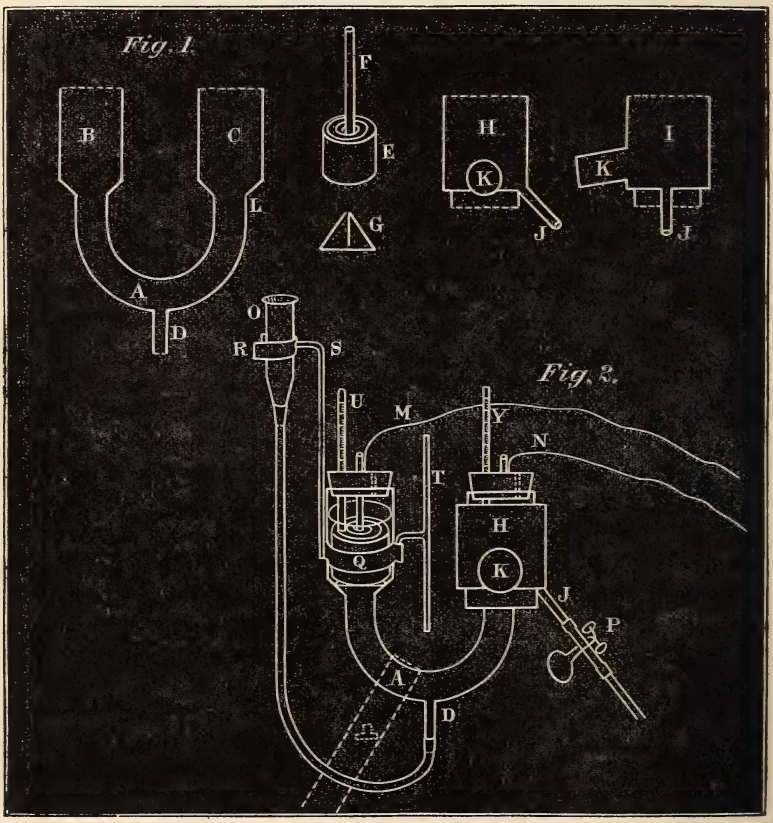
In using the apparatus it was essential to screw it together very tightly before each experiment, otherwise it became somewhat loosened by the influence of the heat upon the upper washers and the screws, and the currents were thereby greatly increased in magnitude. The quantity of water put into the cistern above the upper plate before each experiment was two ounces, and the current of steam passed into it raised its temperature from 60° to 210° F. in about seven or eight minutes. It was necessary to take the apparatus asunder, and wash the plates and washers thoroughly before each experiment, and to remove from them every particle of extraneous substance. In many cases the gold or platinum plates (but never the palladium ones, because they oxidize) were heated to redness before an experiment. The galvanometer employed was in nearly all cases an astatic one, with a coil offering a resistance of 100 ohms, and was found to be more suitable than others which offered less resistance.

I examined, by means of this apparatus, a large number of conducting solutions, in order to include conspicuous instances of the phenomena and exceptional cases, as well as to more completely ascertain any sources of interference, and the proper conditions for obtaining the most reliable results. Metals or liquids which chemically affected each other were usually avoided, and base metals, and even silver, were rarely used. Plates of palladium were found to be much more frequently corroded than those of platinum or gold, and were therefore much less suitable for these experiments; they were thus acted upon in hot or cold ones of chloride of chromium or perchloride of iron, in hot solutions of either chloride of manganese, acidulated ones of nitrate or chloride of copper, and in one of iodate of potassium. Those of copper were corroded in a hot solution of carbonate of sodium. With different liquids, also, different circumstances occurred, which interfered with the currents; in some cases the liquid corroded both plates, as with palladium in chloride of chromium or perchloride of iron; in others it corroded the hot plate only. In some instances the liquids were chemically changed by contact either with the hot plate or the washers, and deposited oxide or basic salt upon them; for instance, perchloride of iron in contact with hot palladium, platinum, or gold; or permanganate of potassium in contact with hot platinum or with the washers. With strongly alkaline solutions, the prepared washers, and even those formed of pure rubber only, were acted upon, the upper stratum of liquid became slightly yellow, and therefore altered in chemical composition, and the currents were greatly altered

in magnitude, especially if the heat was continued until bubbles of steam formed against the upper plate.

During an examination, by means of this apparatus, of nearly all the liquids named in this paper, I gradually ascertained the extent of this interference of the washers, and found that even the most carefully-prepared ones affected, more or less, the magnitude of the currents on all solutions which were not neutral, or nearly so, to test-paper; they, however, rarely reversed the direction of the current. The degree of tightness, also, to which the apparatus was screwed up largely affected the quantity of the current in such liquids.

In consequence of these circumstances, I devised and constructed the apparatus here represented, and employed it for the purpose of checking the results obtained with strongly alkaline or acid solutions, in the one with circular plates; those obtained with the more neutral liquids less required checking.



The chief part of this apparatus consists of a bent glass-tube (fig. 1), with open cup-shaped ends, B and C, and an open exit-tube, D. This

glass vessel, when first constructed, was straight, and the narrow part of it, A, afterwards bent by means of a row of Bunsen's burners. Each of the cups contained a ribbon of sheet platinum, E, welded to the lower end of a thin platinum tube, F, and coiled into a spiral form, the coil resting upon a triangle of glass rod or platinum wire, G, placed inside each cup; each ribbon employed was 48 inches long and 1 inch wide, and weighed two troy ounces; shorter and somewhat wider ones would be more convenient, and for the cold electrode a much smaller one would probably suffice, but only for use with such liquids as offer but little electric conduction-resistance; the hot electrode must, however, be large, in all cases, in order to obtain conspicuous results. Each of the platinum tubes was tightly fixed in a hole in the centre of a bung of cork, which fitted the mouth of the cup, by which the electrode might be lifted out; each bung also had a hole in it, for the insertion of a thermometer, and a small one, also, for escape of air. The ends of thin platinum wires, M, N, for conveying the current to the galvanometer, were also fixed securely between the tubes and the bungs. The cup to be heated, C, was surrounded by a brass jacket or cistern, H (viewed from the front), and I (viewed from the side), provided with an exit-tube, J, and a projecting chamber, K (closed at its outer end only), beneath which the flame of a lamp might be conveniently applied. The jacket was secured water-tight around C (fig. 1), by fixing upon the narrow part of the glass tube, at L, a split bung, of a diameter such as to tightly fit the lower opening of the cistern, and securing the cistern to it water-tight by means of asphalt varnish. The jacket was kept concentric with the cup C, by means of three pieces of cork fixed between the two vessels at their upper end.

The exit-tube, D, was provided with a piece of pure india-rubber tubing, 15 inches long (fig. 2), provided with a narrow glass funnel, O, at its extremity, by means of which both the filling and the emptying of the cups, B and C, was effected; a glass stop-cock was substituted for the tubing when liquids which acted upon india-rubber were employed. The exit-pipe, J, of the cistern was also provided with a pinch tap, P, so that the hot water might be quickly removed from the cistern after an experiment. Fig. 2 shows the complete apparatus. The glass tube was supported in a vertical position by a firm clip-support, which grasped it tightly at the point A, and the funnel, O, was held in an elevated position by means of a separate clip-support, R, attached by a vertical metal rod to a metal hoop, Q, which encircled the cup, B, T is a screen of cardboard, also supported by the hoop Q; U, and V, are thermometers. The glass vessel contained about five ounces of solution, and the cistern required about three ounces of water. All the sketches are about one-fourth the actual linear dimensions.

In using the apparatus the electrodes were first well cleaned, washed,

and drained as completely as possible, heated to redness, coiled, and placed in position; the galvanometer connected; the vessel charged with the clear and cold solution by means of the funnel, until the electrodes were quite submerged; and the deflection of the galvanometer needles was also noted. The cistern was then charged with water; and as soon as the needles of the galvanometer settled at or near zero, a Bunsen's burner (previously placed beneath the projecting chamber K) provided with a regulating tap, was lighted, the water heated to boiling, and boiled until the temperature of the solution ceased to rise, the deflections of the needles during the process of heating being noted. The hot water was then ran out, the apparatus allowed to cool, then discharged of its solution, and thoroughly washed by pouring abundance of cold water through each limb. It usually required about three or four minutes to raise the water in the cistern to 212° F.; the liquid in the cup C usually acquiring a temperature of nearly 150° F. by that time; and it required the water in the cistern to be kept boiling about six or eight minutes longer in order to raise the temperature of the inner liquid to 200° F., and that was nearly the highest temperature it would acquire.

This apparatus acted very satisfactorily, and enabled me to more accurately examine the influence of heat in such actions. The results obtained with this form of thermo-electric liquid examiner, confirmed in nearly every case, where the currents were feeble, those obtained with the other one, and confirmed also so far as regards the chief results (viz., the existence and direction of the current) those obtained with strongly acid or alkaline liquids, but less frequently confirmed with such solutions the relative magnitudes of the currents in different cases; the large cylinder apparatus may therefore be satisfactorily employed with neutral solutions, but is not suitable for purposes of accurate measurement of the currents, especially of those obtained with strongly acid or alkaline liquids.

Although the experiments made with the cylinder apparatus were liable to interferences caused by the washers, as nearly all of them gave substantially reliable results, it was desirable to record them in the following pages for the purpose of comparison. A sufficient number of liquids was examined by means of each apparatus so as to include apparently exceptional cases as well as conspicuous and extreme instances of the phenomenon; and the following is an abbreviated statement of the results obtained, with occasional remarks upon them.

Behaviour of Different Liquids and Metals.

A.—*In the Cylinder Apparatus.*

In all the experiments with this apparatus, the water in contact with the upper plate was raised to a temperature of about 210° F.,

and the lower plate was at the atmospheric temperature, varying from 55° to 65° F. In all instances where the temperature is not specified, the upper plate was at 210° F. Distilled water was used in making all the various solutions, unless otherwise stated.

Experiment No. 1. 14 oz. by measure of water and 1 oz. of a saturated solution of pure sodic carbonate. Platinum plates. Galvanometer of 25 ohms resistance. Current ↓, hot plate positive, deflection ·25°.

No. 2. 7 oz. of water and 7 oz. of the same saturated solution. Platinum plates. Current ↓ 7·5.

No. 3. 1 oz. of water and 14 oz. of the same saturated solution. Platinum plates. Current ↓ 10.

No. 4. Same mixture as in No. 3. Platinum plates. Galvanometer of 100 ohms resistance.* Current ↓ 15. (Compare Nos. 39 and 118.)

No. 5. The same mixture. Silver plates. Current ↓ 15·75.

Remarks. No sign of corrosion of the plates appeared in either of these five experiments. The current increased in magnitude with the strength of the solution.

No. 6. The same mixture. Copper plates. Current ↓ 65·5. The upper plate was found corroded after this experiment. (Compare No. 164.)

No. 7. Upwards of forty different experiments were made with pure dilute sulphuric acid of seven different degrees of strength, varying from 12 oz. of water and 3 oz. of the acid to 14 oz. of water, and $\frac{1}{16}$ oz. of the acid; and all the mixtures were tried with plates of platinum, gold, and palladium. The current was upward in every case, and the amount of deflection varied from $1\frac{1}{2}$° to 45°. (Compare Nos. 115, 116, and 117.) The currents were reverse in direction to those which would have been produced by chemical action; and were strongest with that metal which was the least likely to be chemically affected, viz., platinum.

No. 8. 14 oz. of water and $\frac{1}{8}$ oz. of sulphuric acid. Silver plates. Current ↑ 20·5.

No. 9. 14 oz. of water, and $\frac{1}{4}$ oz. of sulphuric acid. Silver plates. Current ↑ 32.

No. 10. 14 oz. of water and 3 oz. of glacial acetic acid, or 14 oz. of water, and either $\frac{1}{2}$ oz. or $\frac{1}{16}$ oz. of the acid; each mixture being used with palladium plates gave very feeble current ↓; and then, on suddenly applying much cold water to the hot plate, a less feeble ↑ current was produced. Platinum and gold plates gave similar effects with a mixture of 14 oz. of water and $\frac{1}{2}$ oz. of the acid.

No. 11. 14 oz. of water and $\frac{1}{2}$ oz. of thick syrupy solution of ordinary phosphoric acid. Platinum, gold, and palladium plates. Current ↑ about ·5. Cold water then applied produced in each case a feeble ↓ current.

* This galvanometer was used in all the subsequent experiments.

No. 12. 14 oz. of water and $\frac{1}{4}$ oz. of pure nitric acid. Platinum plates. Current ↑ 23. Gold plates. Current ↑ 12·5. Palladium plates. Current ↑ ·75. Cold water produced a feeble current ↓.

No. 13. 14 oz. of water and 1 oz. of pure hydrochloric acid. Platinum plates. Current ↑ 2. Gold plates. Current ↑ 2. Palladium plates. Current ↑ 2. Cold water produced feeble ↓ currents. (Compare No. 143.)

No. 14. 12 oz. of water and 3 oz. of a saturated solution of pure boracic acid. Platinum plates. Current ↓ 1·5. Cold water produced a weak current ↑.

No. 15. 12 oz. of water and 4 oz. of crystals of sulphate of copper. Plates of platinum, gold, or palladium, gave ↓ currents, strong with the two first-named metals and feeble with the last ones. (Compare Nos. 42, 43, 44, and 113.)

No. 16. 12 oz. of water, 4 oz. of crystallized chloride of copper, and $\frac{1}{2}$ oz. of pure concentrated hydrochloric acid. Gold plates. Current ↑ $52\frac{1}{2}$ at 160° F.° and 48 at 210° F. Cold water produced a very strong ↓ current.

No. 17. The same mixture, with platinum plates. Current ↑ $54\frac{1}{2}$ at 170° F. and $51\frac{1}{2}$ at 210° F. (Compare No. 125.) Cold water strongly reversed the current.

No. 18. The same mixture, with palladium plates. Current ↓ 45 at 190° F., and 36 at 210° F. Cold water produced a very strong ↓ current, and the upper plate was found corroded after the experiment. *Remarks.* These circumstances show that the downward current was due to the chemical action excited by the higher temperature. This experiment also illustrates the utility of subsequently applying cold water to reveal the true effect of heat in cases where chemical action interferes. (Compare Nos. 32, 60, and 125.)

No. 19. 7 oz. of water and $6\frac{1}{2}$ oz. of a saturated solution of chlorate of copper containing very little free acid. The mixture reddened blue litmus paper. Platinum plates. Current ↑ 28 at 195° F., and 15 at 210° F. Cold water produced a ↓ current. (Compare No. 126.)

No. 20. The same mixture. Gold plates. Current ↑ $5\frac{1}{4}$. Cold water produced a ↓ current.

No. 21. The same mixture. Palladium plates. Current ↑ 35. Cold water reduced the deflection to 0.

No. 22. A nearly saturated solution of nitrate of copper, not containing any free acid. Platinum plates. Current ↑ $8\frac{1}{4}$. Cold water reversed the direction of the current.

No. 23. The same solution. Gold plates. Current ↑ 2 at 180° F. Cold water produced a ↓ current.

No. 24. The same solution. Palladium plates. Current ↑ $15\frac{1}{2}$. Cold water produced a ↓ current.

No. 25. 14 oz. of a nearly saturated solution of cupric nitrate and

$2\frac{1}{2}$ oz. of pure concentrated nitric acid. Platinum plates. Current ↑ 48. Cold water produced a strong ↓ current. (Compare No. 138).

No. 26. The same mixture. Gold plates. Current ↑ 53. Cold water reversed the current strongly.

No. 27. The same mixture. Palladium plates. Current ↑ 56. Cold water produced a very strong ↓ current. The upper plate was found corroded after the experiment. *Remarks.* A comparison of the results of Experiments Nos. 25, 26, and 27, with those of the three immediately preceding ones, shows the effect of the free acid in increasing the negative condition of the hot plate.

No. 28. 14 oz. of water and $2\frac{1}{2}$ oz. of pure concentrated nitric acid. Platinum plates. Current ↑ $20\frac{1}{4}$ at 160° F., and $16\frac{1}{2}$ at 210° F. Cold water reversed the current strongly. (Compare No. 119.)

No. 29. The same mixture. Gold plates. Current ↑ $24\frac{1}{2}$. *Remarks.* A comparison of the last eight experiments shows that a mixture of an aqueous solution of cupric nitrate with one of nitric acid yields stronger currents than either solution alone.

No. 30. A strong aqueous solution of chloride of chromium. Gold plates. Current ↑ 44 at 200° F., and 42 at 210° F. This liquid had not been pre-boiled, and therefore many air-bubbles accumulated against the hot plate, but appeared to only slightly decrease the amount of deflection. Cold water produced a very strong ↓ current.

No. 31. The same solution. Platinum plates. Current ↑ 45 at 180° F., and 42 at 210° F. Cold water produced a very strong ↓ current. (Compare No. 123.)

No. 32. The same solution. Palladium plates. Current ↓ 63. Cold water diminished the amount of deflection. Both the plates were a little corroded after the experiment, and each equally so. Repetition of this experiment yielded similar effects. (Compare Nos. 18 and 60.)

No. 33. 14 oz. of water and 1 oz. of acid chromate of potassium. Platinum plates. Current ↓ $1\frac{1}{2}$. Cold water reversed the current.

No. 34. The same mixture. Gold plates. Current ↓ $4\frac{1}{4}$ at 200° F. Cold water produced a feeble ↑ current.

No. 35. The same mixture. Palladium plates. Current ↓ $5\frac{1}{2}$. Cold water produced a feeble ↑ current.

No. 36. The same mixture, with 1 oz. of dry crystals of chromic acid added. Platinum plates. Current ↑ $4\frac{1}{4}$. Cold water first slightly increased and then, to a large extent, decreased the current. (Compare No. 124.)

No. 37. The same mixture as in No. 36. Gold plates. Current ↑ $1\frac{1}{4}$. Cold water feebly reversed the current.

No. 38. The same mixture as in No. 37. Palladium plates. Current ↓ 3 at 120° F., and ↑ 20 at 210° F., and gradually increased to 30 by continuance of heat. Cold water first slightly increased and then

equally decreased the current. No signs of chemical action were observed.

No. 39. 1 oz. of water and 14 oz. of a saturated solution of sodic carbonate. Platinum plates. Current ↓ 12, and gradually increased to 25 by continuance of heat. Cold water reversed the current. (Compare Nos. 4 and 18.)]

No. 40. The same mixture. Gold plates. Current ↓ 28, and remained at 28 by continuance of heat. Cold water strongly reversed the current.

No. 41. The same mixture. Palladium plates. Current ↓ 35, and gradually increased to 40 by continuance of heat. *Remarks.* The increase of current by continuance of heat in these three last experiments is not wholly due to an action of the liquid upon the washers. (Compare Nos. 112, 121, 142, 145, 149, 151, and 153.)

No. 42. 12 oz. of water, 4 oz. of crystals of cupric sulphate, and $\frac{1}{4}$ oz. of pure concentrated sulphuric acid. Gold plates. Current ↑ 48. Cold water decreased the deflection to 30.

No. 43. The same mixture. Platinum plates. Current ↑ 50. Cold water diminished the deflection to 30. (Compare No. 127.)

No. 44. The same mixture. Palladium plates. Current ↑ $37\frac{1}{2}$. Cold water sent the needles back strongly. (Compare No. 15.)

No. 45. 2 oz. of water and 12 oz. of a saturated solution of sulphate of potassium. Solution very faintly blued neutral litmus paper. Platinum plates. Current ↓ $1\frac{3}{4}$. Cold water produced a feeble upward current.

No. 46. The same mixture. Gold plates. No current. Palladium plates. Current ↓ $2\frac{1}{2}$.

No. 47. 3 oz. of water and 12 oz. of a saturated solution of acid sulphate of potassium. Liquid strongly acid to litmus paper. Palladium plates. Current ↓ $\frac{1}{2}$. Cold water produced a very feeble ↑ current.

No. 48. The same mixture. Gold plates. No current. Platinum plates. No current, but cold water produced a feeble downward current in each case. *Remarks.* The acid sulphate behaved very much like the neutral sulphate in these and other experiments, and both behaved like the corresponding chromates. (Compare No. 128.)

No. 49. 14 oz. of water and $\frac{3}{4}$ oz. of crystals of cupric acetate. Gold plates. No current. Cold water produced a feeble ↓ current.

No. 50. The same mixture, with platinum plates. Current ↑ $\frac{1}{2}$. Palladium plates. No current. Cold water produced a feeble ↓ current, in each case.

No. 51. The same mixture, with 3 oz. of glacial acetic acid added to it. Palladium plates; no current. Gold plates; current ↑ $1\frac{1}{2}$. Platinum plates; current ↑ $1\frac{1}{2}$. Cold water produced a downward current in all three cases. (Compare No. 122.)

No. 52. 12 oz. of water and 4 oz. of yellow chromate of potassium.

The solution was alkaline to litmus paper. Platinum plates. Current ↓ $4\frac{1}{4}$. Cold water produced an ↑ current.

No. 53. The same mixture. Gold plates. Current ↓ $1\frac{1}{2}$. Cold water reversed the deflection.

No. 54. The same mixture. Palladium plates. Current ↓ $2\frac{3}{4}$. Cold water produced an ↑ current.

No. 55. 14 oz. of water and 4 oz. of dry crystals of sulphate of nickel. The solution was faintly acid to litmus paper. Palladium plates; no current. Gold plates; current ↓ 1. Platinum plates; current ↓ $2\frac{3}{4}$. Probably the washers interfered.

In all three instances cold water produced feeble ↑ currents. (Compare Nos. 15, 113, and 155.)

No. 56. A strong solution of chloride of chromium in alcohol. The solution was acid to litmus paper. Platinum plates. Current ↑ 10 at 180° F., at which temperature large bubbles of vapour were formed. Cold water produced a strong ↓ current. *Remarks.* Probable interference produced by upper washer. (Compare No. 129.)

No. 57. 12 oz. of water and 4 oz. of crystals of nitrate of cobalt. The solution was strongly acid to blue litmus paper. Platinum plates. No current. Cold water produced a feeble ↓ current.

No. 58. 12 oz. of water and 4 oz. of dry chloride of cobalt. The solution strongly reddened blue litmus paper. Platinum plates. Current ↑ 37. Cold water produced a very strong ↓ current. (Compare No. 120.)

No. 59. The same mixture. Gold plates. Current ↑ $38\frac{1}{2}$. Cold water produced a very strong ↓ current.

No. 60. The same mixture. Palladium plates. Current ↑ 25 at 160° F., 0 at 180° F., and ↓ 11 at 200° F. Cold water produced a rather strong downward current and then an upward one. (For similar effects compare Nos. 18 and 32.)

No. 61. 12 oz. of water and 4 oz. of crystals of sulphate of glucinum.* Solution strongly acid to litmus paper. Platinum plates. Current ↑ $1\frac{1}{2}$ at 180° F., and $\frac{1}{2}$ at 210° F. Cold water reversed the deflection.

No. 62. The same mixture. Gold plates. Current ↑ $\frac{2}{3}$. Palladium plates. No current. Cold water produced a feeble downward current in each instance.

No. 63. 12 oz. of water and 4 oz. of chloride of ammonium. The solution was very faintly acid to litmus paper. Platinum plates. Current ↑ $1\frac{1}{2}$, and, in a second experiment, ↑ $2\frac{1}{2}$. Cold water reversed the deflection in both cases.

No. 64. The same mixture. Palladium plates. No current. Cold water produced a feeble ↑ current.

No. 65. 12 oz. of water and 3 oz. of chloride of potassium. Solu-

* Prepared for me by Dr. H. Trommsdorff, of Erfurt.

tion neutral to litmus paper. Palladium plates. Current ↑ $6\frac{1}{2}$ at 160° F. and $2\frac{1}{4}$ at 210° F. Cold water produced a strong ↓ current.

No 66. The same mixture. Platinum plates. Current ↑ 10. (Compare No. 130.) Gold plates. Current ↑ 11. Cold water strongly reversed the current in each case.

No. 67. A nearly saturated solution of sulphate of magnesium. Neutral to litmus paper. With palladium, platinum, or gold plates, no current. Cold water produced a very feeble downward current in each case.

No. 68. 12 oz. of water and 4 oz. of very pure crystals of nitrate of lead. Solution was acid to blue litmus paper. Gold plates. Current ↓ 1. Palladium plates. Current ↓ $\frac{3}{5}$. Platinum plates. Current ↓ $\frac{1}{2}$. Cold water reversed the current in each case.

No. 69. A nearly saturated solution of pure calcic chloride. Neutral to litmus paper. Platinum plates. Current ↓ $\frac{1}{2}$. Cold water produced a feeble upward current. Gold plates; no current. Palladium plates; current ↓ $\frac{1}{2}$. Cold water reduced the deflection nearly to zero.

No. 70. 12 oz. of water and 4 oz. of extremely pure anhydrous carbonate of potassium. Platinum plates. Current ↓ 2. Cold water reversed the deflection. (Compare No. 121.)

No. 71. The same mixture. Gold plates. Current ↓ $2\frac{3}{4}$, and $4\frac{1}{2}$ by continuance of heat. Cold water first increased the deflection, and then reduced it nearly to zero.

No. 72. The same mixture. Palladium plates. Current ↓ 20, and increasing by continuance of heat. Cold water sent needles back to zero.

No. 73. 13 oz. of water and 2 oz. of dry chloride of manganese. Solution acid to litmus paper. Palladium plates. Current ↑ $58\frac{1}{2}$ at 180° F., and 41 at 210° F. Cold water reduced the amount of deflection. There were very faint signs of corrosion of the upper plate.

No. 74. The same mixture. Platinum plates. Current ↑ $63\frac{1}{2}$. (Compare No. 114.) Gold plates. Current ↑ $63\frac{1}{2}$. Cold water reduced the current greatly in each case. No signs of corrosion in either instance.

No. 75. A moderately strong solution of very pure chloride of zinc. Solution nearly neutral to litmus paper. Palladium plates. Current ↑ $\frac{3}{4}$. Gold plates; no current. Platinum plates. Current ↑ $\frac{1}{2}$. Cold water reversed the deflection in all three instances.

No. 76. 13 oz. of water and 2 oz. of dry perchloride of iron. The solution was acid to litmus paper. Palladium plates. Current ↑ $7\frac{1}{2}$ at 120° F., and ↓ 18 at 210° F. Both the plates were corroded, especially the upper one.

No. 77. The same mixture. Platinum plates. Current ↑ $72\frac{1}{2}$ at 210° F. No signs of corrosion, but a film of sesquioxide of iron or of basic salt of iron was found all over the upper plate only. (Compare

No. 160.) With gold plates, current ↑ 72, and a similar film was found upon the upper plate only. Cold water reduced the deflection greatly in each instance.

No. 78. 14 oz. of water and 2 oz. of crystals of nitrate of potassium. Neutral to litmus paper. Palladium plates. Current ↓ $\frac{3}{4}$. Cold water increased the deflection to 3.

No. 79. The same mixture. Platinum plates. Current ↓ $1\frac{1}{4}$. Gold plates. Current ↓ $2\frac{1}{8}$ at 180° F., and $1\frac{1}{4}$ at 210° F. Cold water produced a feeble upward current in each instance.

No. 80. 13 oz. of water and $\frac{3}{4}$ oz. of potassic chlorate. Neutral solution. Palladium plates. Current ↑ 1. Cold water increased the deflection to 3.

No. 81. The same mixture. Platinum plates; no current. Cold water produced a feeble ↓ current. Gold plates; no current. Cold water had no effect.

No. 82. 13 oz. of water and 2 oz. of large crystals of sulphate of zinc. Solution strongly acid to litmus paper. Palladium plates. Current ↓ $2\frac{1}{2}$. Platinum plates. Current ↓ 3. Gold plates. Current ↓ $1\frac{1}{2}$. Cold water produced an upward current in each instance.

No. 83. A saturated aqueous solution of potassic iodate. Strongly acid to litmus paper. Palladium plates. Current ↑ 30, and increased by continued heat. The upper plate was slightly corroded.

No. 84. The same mixture. Platinum plates. Current ↑ 14. Cold water decreased the deflection. (Compare No. 132.)

No. 85. The same mixture. Gold plates. Current ↓ $2\frac{1}{4}$ at 170° F., and $\frac{1}{2}$ at 210° F. Cold water produced a very feeble ↑ current.

No. 86. 13 oz. of water and 1 oz. of sticks of ordinary caustic soda. Palladium plates. Current ↓ 68. Cold water reduced the deflection only to 60. No signs of corrosion.

No. 87. The same mixture. Platinum plates. Current ↓ 50. Cold water reduced the deflection only to 45. After boiling the washers half an hour in a similar mixture, and washing them, current ↓ 15. By employing new washers of pure india-rubber only, current ↓ 28. (Compare Nos. 108 and 148.) The same washers with gold plates, current ↓ $4\frac{1}{2}$. Palladium plates, and the same washers, current ↓ 41. Cold water reduced the deflection in each of these instances.

No. 88. The same mixture and washers. Silver plates. Current ↓ 18 at 180° F., and 10 at 210° F. Cold water increased the deflection to 25. No signs of corrosion.

No. 89. 14 oz. of water and 3 oz. of crystals of sodic hyposulphite. Solution very feebly alkaline to litmus paper. Platinum plates. Current ↓ $9\frac{1}{2}$. (Compare No. 135.) Gold plates. Current ↓ 6. Palladium plates. Current ↓ 16. Cold water reduced the deflection in each instance.

No. 90. 14 oz. of water and 2 oz. of nitrate of strontium. Neutral

solution. Platinum plates. No current. Gold plates. Current ↓ $\frac{1}{2}$. Palladium plates. Current ↓ 1. Cold water produced a very feeble upward current in each instance.

No. 91. 14 oz. of water and 1 oz. of crystals of nitrate of barium. Neutral solution. Platinum plates. Current ↓ $1\frac{1}{2}$. Gold plates. Current ↓ $1\frac{3}{4}$. Palladium plates. Current ↓ $\frac{3}{4}$. Cold water reversed the deflection feebly in each instance.

No. 92. 14 oz. of water and 3 oz. of baric chloride. Neutral solution. Platinum plates. Current ↓ $1\frac{1}{2}$ at 200° F. Gold plates. Current ↓ $1\frac{1}{2}$. Palladium plates. Current ↓ $2\frac{3}{4}$. Cold water reversed the deflection feebly in each instance.

No. 93. 14 oz. of water and 1 oz. of crystals of ordinary phosphate of sodium. Solution strongly alkaline to litmus paper. Platinum plates. Current ↓ $2\frac{1}{2}$. (Compare No. 133.) Gold plates. Current ↓ $1\frac{3}{4}$. Palladium plates. Current ↓ 3. Cold water produced a feeble ↑ current in each case.

No 94. 14 oz. of water and $\frac{1}{2}$ oz. of crystals of permangante of potassium; pure india-rubber washers. Platinum plates. Current ↓ 50 at 200° F. Cold water reduced the deflection to zero. A thin film of oxide of manganese was found all over the surface of the upper plate only, and a line of the oxide upon each plate close to the edge of the washers, especially on the upper plate. (Compare No. 161.)

No. 95. The same mixture. Gold plates. Current ↓ 6 at 120° F., and $\frac{1}{2}$ at 210° F. Palladium plates. Current ↓ $51\frac{1}{2}$ at 170° F., and 42 at 210° F. Cold water reversed the deflection strongly in each case.

No. 96. 14 oz. of water and 1 oz. of ammonia alum. Solution strongly acid to litmus paper. Gold plates. Current ↑ $1\frac{1}{2}$ at 180° F., and $\frac{3}{4}$ at 210° F. Palladium plates. Current ↑ $1\frac{1}{2}$ at 200° F. Platinum plates. Current ↓ 1. (Compare No. 136.) Cold water either reduced or reversed the deflection in each instance.

No. 97. 14 oz. of water and 1 oz. of potassium alum. Strongly acid to litmus paper. Palladium plates. Current ↓ $1\frac{1}{2}$. Platinum plates. Current ↓ 1. Gold plates. Current ↓ $1\frac{1}{2}$. Cold water reversed the deflection in each case.

No. 98. 14 oz. of water and 2 oz. of slightly decomposed iodide of potassium. Solution alkaline to litmus paper. Palladium plates. Current ↓ 44 at 140° F., and 34 at 210° F. Cold water first increased the deflection and then reduced it to 31. No signs of corrosion.

No. 99. The same mixture. Platinum plates. Current ↓ 50. (Compare No. 132.) Gold plates. Current ↓ 45. Cold water reduced the deflection in each case, and no signs of corrosion appeared.

No. 100. 10 oz. of water and 4 oz. of a saturated solution of bromide of potassium. Solution alkaline to litmus paper. Palladium plates. current ↓ $2\frac{3}{4}$. Platinum plates. Current ↓ $1\frac{3}{4}$. (Compare No. 137.)

Gold plates. Current ↓ $2\frac{1}{2}$. In each instance cold water reversed the direction of the current feebly.

No. 101. 14 oz. of water and $\frac{1}{2}$ oz. of partly dried borax. Palladium plates. Current ↓ 2. Gold plates. Current ↓ $2\frac{1}{4}$. Platinum plates. Current ↓ $4\frac{1}{2}$. (Compare No. 134.) Cold water reversed the current feebly in each case.

No. 102. 8 oz. of water and 6 oz. of a saturated solution of chloride of strontium. Liquid neutral. Palladium plates. Current ↓ $1\frac{3}{4}$. Cold water feebly reversed the current. Platinum plates. Current ↑ $2\frac{1}{4}$ at 140° F., and ↓ $\frac{1}{2}$ at 210° F. Cold water produced a rather strong downward current, and then a feeble upward one. Gold plates; no current. Cold water produced a weak ↓ current.

No. 103. 10 oz. of water and 4 oz. of dry chloride of nickel. Solution acid to litmus paper. Gold plates. Current ↑ $1\frac{3}{4}$. Platinum plates. Current ↑ $3\frac{1}{2}$. (Compare No. 141.) Cold water reversed the current feebly in each instance.

No. 104. 3 oz. of dry bromide of copper made into 14 oz. of solution with water. Liquid acid to litmus paper. Gold plates. Current ↑ $7\frac{1}{2}$. Platinum plates. Current ↑ $75\frac{1}{2}$. Cold water greatly reduced the current in each instance.

No. 105. The same mixture, with 1 oz. of hydrobromic acid of sp. gr. 1·45 added to it, to redissolve a little basic salt which had subsequently separated. Platinum plates. Current ↑ $73\frac{1}{2}$. (Compare No. 111.) Sufficient cold water reduced the deflection to zero.

No. 106. 14 oz. water, and 2 oz. of dry bromide of nickel. Acid reaction to litmus paper. Gold plates. Current ↑ 42. Cold water quickly brought the needles to zero. Platinum plates. Current ↑ 51 at 200° F. and 48 at 210° F. (Compare No. 139.) Cold water produced a strong ↓ current, permanent for a time.

No. 107. 14 oz. of water and 1 oz. of dry bromide of cobalt. Solution acid to litmus paper. Gold plates. Current ↑ 16. Platinum plates. Current ↑ $43\frac{1}{2}$. (Compare No. 140.) Cold water reversed the current in each instance.

No. 108. 14 oz. of water, and $\frac{1}{2}$ oz. of pure potassic hydrate. Platinum plates. Current ↓ 26 at 210° F., increased to 60 by persistent heat, and was still increasing. (Compare Nos. 87, 112, and 142.) Cold water then reduced the current greatly. The washers imparted a slightly yellow colour to the upper layer of liquid. Two more experiments were made with this solution, the washers being again digested several hours in a strong and hot solution of caustic soda before each experiment; the quantity of the electric current diminished in each experiment.

No. 109. 14 oz. of water, and 1 oz. of cyanide of potassium. Iron plates. Current ↑ 25 at 140° F. No signs of corrosion of the plates were visible. (Compare Nos 145, 167.)

No. 110. In order to ascertain the effect of entire absence of the washers with a strongly alkaline solution, I took the solution of caustic soda used in experiments No. 86, 87, and 88, and put it into a large V-shaped glass tube of 1-inch internal diameter, containing two large platinum electrodes wholly submerged, and suspended by thin platinum wires, the ends of the tube being nearly closed by corks, and heated one limb of the tube. The hot platinum became positive $5\frac{1}{2}^{\circ}$ at 210° F. By allowing the liquid in the heated limb to come into contact with a bung of vulcanized india-rubber, the deflection was increased to 40, thus further proving that vulcanized washers were liable to interfere with the results.

Conclusions and Remarks. On classifying and comparing in different ways the foregoing results obtained with the large cylinder apparatus, we find 1st, that in nearly all cases where the liquid is strongly acid, and chemical action is absent, the cold metal is positive to the hot one, and the electric current flows upwards. And 2nd, that in nearly all those where the liquid is strongly alkaline, and chemical action does not occur, the hot metal is positive to the cold one, and the current flows downwards; thus affording additional evidence in support of the conclusions I drew in my former investigation already referred to (see p. 513).

The nearest approach to exceptions to the above conclusions are in the instance of dilute acetic acid (see No. 10); solution of boracic acid (No. 14); acid chromate of potassium (Nos. 33, 34, and 35); acid sulphate of potassium (No. 47); chloride of potassium (Nos. 65, 66); (compare No. 130); and potassium alum (No. 97); (compare No. 136); but in each of these cases the electric currents were very feeble, and may have been due to the influence of the washers.

Nos. 18, 32, and 60, exhibit rather anomalous results, but in the two first of these corrosion of the metal occurred; and in each case the metal plates were of palladium, and the solution contained a chloride.

It is important to notice that, provided chemical action is excluded, the direction of the current in all the experiments was scarcely at all dependent upon the kind of *metal*, but almost entirely upon that of *liquid;* the only exception to this were Nos. 85, 96, and 102, and these were but feeble ones. In the exceptional cases, Nos. 18 and 32, chemical action occurred. I therefore conclude that *it is the nature of the liquid, and not that of the metal, which most determines the existence and direction of the current.* The magnitude of the current, however, is largely affected in many cases by a variation in the kind of metal employed, even when no chemical change takes place.

In some cases, especially with strongly alkaline liquids, the current due to the heat is greatly increased by keeping the hot plate at 210° F. for some time; for instance, with palladium in an aqueous solution

of acid chromate of potassium acidified with chromic acid (see No. 35); with platinum, gold or palladium, in solution of carbonate of sodium (see Nos. 39, 40, 41); with gold or palladium in carbonate of potassium (Nos. 71, 72); also with platinum in potassic hydrate (No. 108. See also No. 112).

B.—*In the Bent-tube Apparatus.*

In the experiments with this apparatus, the solution in the cup B was at the atmospheric temperature, viz., from 55° to 65° F., and that in cup C was raised to between 190° and 200° F., rarely beyond, by keeping the water in the outer cistern at a boiling temperature. The solutions were not deprived of dissolved air previous to the experiments, because the presence of air had been found to have very little effect.

No. 111. The solution of acidulated cupric bromide of No. 105. Platinum ribbons. Cold metal positive 63°. (Compare No. 105°.)

No. 112. 14 oz. water and $\frac{1}{2}$ oz. of pure potassic hydrate. Hot platinum positive 5° at 200° F., and 31° by persistent continuance of heat. (Compare Nos. 87, 108, 142, and 149.)

Remarks. This result proves that the great increase of the current by continuance of heat in Experiment No. 108 was not *entirely* due to the influence of the washers.

No. 113. 14 oz. of water and 4 oz. of crystals of cupric sulphate. Hot platinum positive 3°. (Compare Nos. 15, 42, 43, and 44.)

No. 114. The solution of chloride of manganese of Nos. 73 and 74. Cold platinum positive 39°. No signs of chemical change. (Compare No. 73.)

No. 115. 14 oz. of water, and $\frac{1}{2}$ oz. of pure sulphuric acid. Cold platinum positive 33°, but diminished to 10 by prolonged heat. (Compare No. 7.)

No. 116. 14 oz. of water, and 1 oz. of the same acid. Cold platinum positive 7°, and decreased at 200° F.

No. 117. 14 oz. of water, and 2 oz. of the same acid. Cold platinum positive $2\frac{1}{2}$°, and decreased at 200° F. (Compare Nos. 115 and 116.)

Remarks. In these three last experiments the magnitude of the current decreased with the increase of amount of acid present.

No. 118. 13 oz. of water, and $7\frac{1}{2}$ oz. of a saturated solution of pure carbonate of sodium. Hot platinum positive $10\frac{1}{2}$°. (Compare Nos. 4 and 39.)

No. 119. The mixture of nitric acid and water of No. 28. Cold platinum positive $47\frac{1}{2}$°. (Compare No. 28.)

No. 120. The solution of chloride of cobalt of No. 58. Cold platinum positive 36°. (Compare No. 58.)

No. 121. The solution of potassic carbonate of No. 70. Hot pla-

tinum positive 26° at 200° F., and increased to 50° by long-continued heat. The needles returned to zero on cooling. (Compare No. 70.)

No. 122. The acidulated solution of cupric acetate of No. 51. Cold platinum faintly positive at 200 F. (Compare No. 51.)

No. 123. The aqueous solution of chloride of chromium of Nos. 30 and 31. Cold platinum positive 48°. Needles returned to zero on cooling. (Compare No. 31.)

No. 124. The acidulated solution of acid chromate of potassium of No. 36. Cold platinum positive 32°. (Compare No. 36.)

No. 125. The acidulated solution of cupric chloride of Nos. 16 and 17. Cold platinum positive 52° at 200° F., and increased to 56° by continued heat. (Compare No. 17.)

No. 126. The aqueous solution of cupric chlorate of No. 19. Cold platinum positive 26°. (Compare No. 19.)

No. 127. The acidulated solution of blue vitriol of Nos. 42, 43, and 44. Cold platinum positive 42°. (Compare No. 43.)

No. 128. The solution of acid sulphate of potassium of No. 47. Cold platinum feebly positive 4° at 200° F., and decreased by continued heat. (Compare No. 48.)

No. 129. The solution of chloride of chromium in alcohol of No. 56. Hot platinum positive $1\frac{1}{2}$° when the alcohol began to boil. (Compare No. 56.)

No. 130. The solution of chloride of potassium of No. 65. Cold platinum positive 20° at about 180° F., and declined to 14 at 200° F. (Compare No. 66.)

No. 131. The saturated solution of potassic iodate of No. 83. Cold platinum positive 5° at 200° F., and increased to 14 by continued heat. (Compare No. 84.)

No. 132. The solution of iodide of potassium of No. 98. Hot platinum positive 23°, and increased to 37° by long continued heat. (Compare No. 99.)

No. 133. The solution of phosphate of sodium of No. 93. Hot platinum positive 5° at 200° F. by continued heat. (Compare No. 93.)

No. 134. The solution of borax of No. 101. Hot platinum positive 3° at 200° F. (Compare No. 101.)

No. 135. The solution of hyposulphite of sodium of No. 89. Hot platinum positive about 20° at 200° F., and increased to 38 by continued heat. (Compare No. 89.)

No. 136. The solution of ammonia alum of No. 96. Hot platinum feebly positive $1\frac{3}{4}$° at 200° F. by continued heat. (Compare No. 96.)

No. 137. The solution of potassic bromide of No. 100. Hot platinum positive $7\frac{1}{2}$° at 200° F. by continued heat. Compare No. 100.)

No. 138. The acidulated solution of cupric nitrate of No. 25. Cold platinum positive 40° at 190° F., and increased to 44° at 200° F. by continued heat. (Compare No. 25.)

No. 139. The solution of bromide of nickel of No. 106. Cold platinum positive 27° at about 150° F., and declined to 10° at 200° F. (Compare No. 106.)

No. 140. The solution of bromide of cobalt of No. 107. Cold platinum positive 29° at 200° F., and increased to 40 by continued heat. (Compare No. 107.)

No. 141. The solution of chloride of nickel of No. 103. Cold platinum positive 17° at 200° F., and decreased 10° by continued heat. (Compare No. 103.)

No. 142. 14 oz. of water and $\frac{1}{2}$ oz. of very pure hydrate of sodium. Hot platinum feebly positive $1\frac{1}{2}$° at 180° F., but increased gradually to 74° in half an hour at 200° F. by continued heat, and then began to decline. The needles returned to zero on cooling. (Compare Nos. 87, 108, 110, and 112.)

No. 143. The dilute hydrochloric acid of No. 13. Cold platinum positive 3° at 200° F., and decreased to 1° by continued heat. (Compare No. 13.)

No. 144. 6 oz. of water, and 1 oz. of anhydrous sulphate of cobalt. Cold platinum positive 6° at 200° F., but declined to 1° by continued heat.

No. 145. The solution of potassic cyanide of No. 109. Hot platinum positive 35° at 200° F., and increased to 57 by prolonged heat. No signs of corrosion of the plates. (Compare No. 109.) In these two instances a difference of metal was apparently attended by a reversal of direction of the current (see No. 167).

No. 146. 6 oz. of water, and $\frac{1}{4}$ oz. of bichloride of mercury. Not the slightest sign of a current. *Remark.* This is the most neutral solution yet found.

No. 147. 6 oz. of water, and 1 oz. of dry crystals of argentine nitrate. Hot platinum positive 5° at 200° F., and increased to 16° by continued heat, and was still increasing when the experiment was stopped. *Remark.* This solution behaved like one of an alkali. (Compare Nos. 108, 110, 112, 142, and 149.)

No. 148. 12 oz. of water, and 4 oz. of clear, dry crystals of sulphate of manganese. Hot platinum positive 17° at 200° F., and increased to $25\frac{1}{2}$° by continued heat. *Remark.* This solution also behaves like an alkaline one.

No. 149. 6 oz. of water, containing the carbonate of rubidium from $2\frac{1}{4}$° oz. of dry and well crystallized bitrartrate of rubidium, which yielded the exact equivalent weight of the carbonate by ignition, &c. Hot platinum positive 5° at 200° F., and increased to 32° by continued heat. The needles returned to zero on cooling. (Compare Nos. 151 and 153.)

No. 150. 6 oz. of water, containing the carbonate of cæsium, from 875 grs. of the bitartrate. (N.B.—The salt contained some silica.)

Hot platinum became very feeble and very slowly positive 3° at 200° F., by prolonged heat. The current declined on cooling.*

No. 151. 6 oz. of water, containing about 300 grs. of very pure fluoride of potassium, quite free from hydrofluoric acid. Solution strongly alkaline. Hot platinum positive 15° at 200° F., and increased to 18° by continued heat, and then began to decrease. (Compare No. 153.)

No. 152. 6 oz. of water and $\frac{1}{2}$ oz. of clear colourless crystals of pure selenate of sodium. Hot platinum feebly positive 1° at 200° F., and increased to $1\frac{3}{4}$° by continued heat.

No. 153. 6 oz. of water and 565 grs. of fluoride of rubidium, perfectly free from hydrofluoric acid. Solution strongly alkaline. Hot platinum positive 7° at 200° F., gradually increased to 10° by continued heat, and then slowly decreased. (Compare Nos. 149 and 151.)

No. 154. 6 oz. of water and about $\frac{3}{4}$ oz. of clear crystals of sulphate of didymium.† Hot platinum positive 3° at 200° F., and increased to $4\frac{1}{2}$° by continued heat, and then began to decline.

No. 155. The solution of sulphate of nickel of No. 55. Cold platinum feebly positive 1° at 100° F., and hot platinum positive 17° at 204° F. (Compare No. 55.)

No. 156. 14 oz. of water and 2 oz. of crystals of chrome alum. Hot platinum positive $17\frac{1}{2}$° at 170° F. No deflection at 194° F., and cold platinum positive 2° at 198° F. No signs of corrosion of either platinum ribbon.

No. 157. 6 oz. of water and $\frac{1}{4}$ oz. of dry selenious acid. Hot platinum positive $9\frac{3}{4}$° at 190° F., and $8\frac{1}{2}$° at 200° F. No signs of corrosion.

No. 158. 7 oz. of water and 2 oz. of solid and colourless sulphate of alumina. Cold platinum positive 35° at 200° F.

No. 159. 7 oz. of water and $2\frac{1}{4}$ oz. of dry crystals of green vitriol, very slightly oxidized. Hot platinum positive 60° at 198° F. The heated part of the solution became quite cloudy, and yielded an ochreous deposit, showing a chemical change.

No. 160. The solution of perchloride of iron of No. 77. Cold platinum positive 59° at 200° F. The needles returned to zero on cooling. A very slight film of brown oxide was found on that electrode only which had been heated, and probably increased its negative condition. (Compare No. 77.)

No. 161. The solution of permanganate of potassium of No. 94. Hot platinum positive 17° at 140° F., the deflection diminished to 0 at 190° F. On cooling the apparatus, the electrode which had not been heated became positive, and when both electrodes were at 56° F. the

* The bitartrates of rubidium and cæsium were prepared by Dr. H. Trommsdorff, of Erfurt.

† Prepared by Dr. Trommsdorff.

deflection was 18°. A film of brown oxide was found on that electrode only which had been heated, and it was probably this which made that electrode first neutral and then negative. (Compare Nos. 77, 94, and 160.)

No. 162. A deep blue solution of sulphate of chromium, probably containing much free sulphuric acid. Cold platinum positive $7\frac{1}{2}°$ at 180° F., and $5\frac{1}{2}°$ at 200° F. On cooling the liquid the needles returned to zero.

No. 163. An aqueous solution of iodide of cadmium. Hot platinum was feebly positive 1° at 200° F.

No. 164. A nearly saturated aqueous solution of pure carbonate of sodium. Copper ribbons, 30 inches long and $1\frac{1}{4}$ inch wide. Hot copper positive $70\frac{1}{2}°$ at 200° F. (Compare No. 6.)

No. 165. The same solution. Iron ribbons, 14 inches long and $1\frac{1}{2}$ inch wide. Hot iron positive 70° at 200° F. No signs of corrosion of the iron. *Remarks.* A cheap liquid thermo-electric battery might perhaps be formed by means of this combination. It is well known that solutions of alkalies protect iron from corrosion, and lime water is used for that purpose.

No. 166. The solution of sodic hydrate of No. 142. Iron ribbons. Hot iron positive $75\frac{1}{2}°$ at 200° F. (Compare Nos. 87, 110, and 142.)

No. 167. The solution of potassic cyanide of No. 109. Iron ribbons. Hot iron positive 44° at 200° F. (Compare No. 109 and 145.) *Remarks.* As an illustration of the absence of corrosive action of this liquid upon iron, I took two thin wires of equal dimensions and perfectly clean and bright, one of iron and the other of pure gold, twisted them together, and immersed them in a solution of potassic cyanide in a closed bottle kept at ordinary atmospheric temperature. After a few weeks the gold had entirely dissolved and disappeared, but the iron remained unaltered in appearance. I also found iron to be electro-negative to gold in such a solution. Iron employed also as an anode in such a liquid offers a much greater degree of resistance than gold to the passage of an electric current.

Remarks and conclusions. A review of the results obtained with the improved apparatus confirms, in nearly all cases, the general conclusions (see p. 528) arrived at by means of experiments with the cylinder one. The most conspicuous exceptions yet met with have been obtained with a solution of sulphate of manganese (No. 148), chrome alum (No. 156), sulphate of nickel (No. 155), and argentic nitrate (No. 147), probably also of sulphate of iron (No. 159); cupric sulphate also (Nos. 15 and 113) appears to be a feeble exception. In some acid solutions, also, hot platinum is feebly positive, for instance, dilute selenious acid (No. 157), an alcoholic solution of chloride of chromium (No. 129), and an aqueous one of ammonic alum (Nos. 96 and 136). No instance, however, has yet been met with in which, in the absence

of chemical change, hot platinum is strongly positive in a strongly acid solution, or strongly negative in a very alkaline one; the nearest approach to this was in the case of aqueous selenious acid. A law without an apparent exception is an unusual phenomenon, and, as a single real exception will invalidate a general law, if the above exceptions are real ones, they prove that the theory that acids are thermo-electro-positive, and alkalies thermo-electro-negative, is not an exact one.

Some strongly acid solutions give only feeble currents; for instance, acid sulphate of potassium (Nos. 48 and 128), and some strongly alkaline ones behave similarly, except when persistently heated; for example, potassic hydrate (No. 112), sodic hydrate (No. 142), and carbonate of rubidium (No. 149). On the other hand, some neutral solutions yield currents stronger than the chemical theory would lead us to expect; for instance, chloride of potassium (Nos. 66 and 130).

One rather common effect observed has been a very great increase in the magnitude of the current by continuance of the heat without increased rise of temperature, as if molecular inertia or some other slowly yielding resistance had to be overcome. This occurred in nearly all solutions which were strongly alkaline, and was at one time suspected by me to be wholly due to the action of the liquid upon the washers; it occurred freely, however, in experiments with the bent tube apparatus in which no washers were employed. The following are examples of such an effect:—with carbonate of sodium (Nos. 39, 40, and 41), carbonate of potassium (Nos. 70, 71, 72, and 121), caustic potash (Nos. 108 and 112), iodide of potassium (No. 132), sodic hyposulphite (No. 135), cyanide of potassium (No. 145), carbonate of rubidium (No. 149), fluoride of potassium (No 151), and fluoride of rubidium (No. 153). It also took place, though less frequently and to a less degree, in liquids of acid reaction; for instance, an acidified solution of acid chromate of potassium (No. 38), iodate of potassium (No. 131), an acidified solution of cupric nitrate (No. 138), bromide of cobalt (No. 140), argentic nitrate (No. 147), and sulphate of manganese (No. 148). In some other cases the quantity of the current *diminished* by continuance of heat.

The whole of the results obtained with this apparatus further confirm the conclusion obtained from the results of the experiments made with the cylinder one, viz., that, provided chemical action is not present, it is *the nature of the liquid*, and not so much that of the metal of the electrodes, which determines the existence and direction of the current.* If also we class under the heads of their respective acids all the salts employed in the experiments, and then class them similarly under their respective bases, we find that both the direction and the magni-

* No. 109 is not a real exception to this statement. Compare Nos. 109, 145, and 167.

tude of the current varies with each different base and acid; but in the few cases in which electrodes of different metal were employed with the same liquid, as with platinum, copper, and iron in solution of sodic carbonate in Nos. 118–164 and 165; with platinum and iron in solution of sodic hydrate of Nos. 142 and 166, and with iron and platinum in solution of potassic cyanide of Nos. 145 and 167, the quantity only, and not the direction, of the current was affected. This agrees with the results obtained with the cylinder apparatus, in which a greater variety of metals was employed. The only apparent exception to this statement was with iron in solution of potassic cyanide of No. 109; but the contradictory result obtained in that case was evidently due to the liquid acting upon the washers.

Cause of the Currents.

The heat applied to one of the plates must either act as the real cause of the currents, or merely as an exciting cause, by liberating from a potential state another force which produces them. If it acts as the real cause, the effect will be proportional to the heat applied, except in those cases where some condition (such as conduction resistance, or a reversal of electric polarity) exists to prevent or diminish it. If, however, it operates merely as an exciting cause, the current would either be large in proportion to the amount of heat applied, or vary in a different ratio to the temperature, because the liberated force would probably obey a different and higher law of increase than that of the temperature. The substances also, having evolved a portion of their stored-up power, would be in a different potential condition after an experiment to what they were before it. If, however, we examine all the results, we find that in nearly the whole of the instances the currents increased in magnitude (usually in approximate proportion) with the temperature, and the metals and liquids after an experiment were in precisely the same physical and chemical state as they were before it, and might therefore be used again for the same experiment any number of times without loss of efficiency.

Heat alone, or chemical affinity excited by heat, appears to be the only probable cause of the currents. That chemical action, started by heat, did not produce them, appears certain—1st, because no corrosive effect (shown by loss of brightness) was manifested upon the plates, even after many experiments, except in certain cases, and this was quite a delicate test;* nor in the great bulk of the instances did any other

* As the plates were burnished, a change of brightness was a much more delicate test than any loss of weight, I therefore did not test for corrosion by weighing them. To generate a current also of equal amount to that produced in some of the instances, an appreciable amount of metal would have been required to dissolve. Further; no signs of dissolved platinum, gold, or palladium were observed in the residues obtained on evaporating and crystallizing the various liquids, in those cases where no signs of corrosion had been observed upon the plates.

chemical change occur; and, 2nd, because when chemical action of any kind did take place, it either produced no electric currents, or it produced currents either in a reverse direction, or greater in amount than those due to the heat. It also in nearly all such cases either caused the plates to lose their lustre, produced a deposit upon them, or made the liquids change in colour or become cloudy. In consequence of these different effects, the apparatus and method of experiment may be employed for detecting some of the chemical (as well as the electrical) effects of heat upon electrically conducting liquids. No signs of electrolytic action, or deposition of metal, even in the most suitable solutions, such as those of copper, were observed; and there was no reason to suppose that the metal plates contained occluded gases. The phenomena were also inconsistent with the supposition that the heat operated by temporarily dissociating the constituents of the liquid. (Compare Nos. 77, 94, 159, 160, and 161.) The fact also that the currents produced in acid liquids were opposite in direction to those which would have been produced by chemical action, is very strong evidence against the cause being chemical. From these various circumstances it is clear that, except in those cases where signs of chemical change were detected, heat was the true and only cause of the currents, and not merely an exciting cause of chemical action producing them.

Although heat is the true and only cause of the currents, it may operate either in a direct manner in producing them, or through the agency of some intermediate force. If it operates through some other force, then an action of that force must form an essential part of the phenomena. As the currents are not confined to magnetic liquids, magnetic action cannot be an essential condition. Light also does not manifestly influence the results, and chemical action can be excluded; and the only forces likely to be active in the circumstances of the case are those of cohesion and adhesion. We know that rise of temperature usually diminishes both these powers, expands both solids and liquids, and rearranges their molecules; it usually diminishes the adhesion of liquids to solids; and in certain cases it even disassociates the elements of a compound. As the action, however, which produces the current always requires *two* substances, viz., a liquid and a metal, and evidently occurs either wholly or chiefly at their immediate surfaces of mutual contact, we may conclude that it is not an effect of heat upon the force of cohesion in the *mass* of liquid or of metal.

Although various effects of heat upon cohesion and adhesion must occur in the metals and liquids employed in these experiments, and cannot be excluded, they may still not necessarily form an essential part of the phenomena, but be only concomitant circumstances, and the existence of the electric currents may be in no degree dependent upon them. As also the currents continue not only during the rise and fall of temperature, but likewise during a stationary and prolonged difference

of it, and are not reversed in direction during the cooling, it is evident that they cannot be due to the act of diminution of cohesion or adhesion produced by the heat. As, further, all the substances employed are in precisely the same physical and chemical states after an experiment as they were before it, and as the portion of heat expended in raising the temperature, expanding the substances, and diminishing their cohesion and adhesion, must have been given out again on the cooling and contracting of those bodies, that portion cannot have been expended in producing the currents. From these considerations I conclude that neither the force of cohesion, nor that of adhesion, acts as an intermediate cause of the currents, and that the latter are, in an ordinary sense of the word, *direct* effects of the heat. (It may be further remarked that if a change of the force of cohesion, or of that of adhesion, gave rise to the currents, then such a change when produced by other means than heat (such as enormous pressure) must be capable of causing similar effects.)

As heat was the true, immediate, and only cause of the currents, the action may, in accordance with usage in similar cases, be properly termed *thermo-electric;* and as heat was the only force expended, we may also, in accordance with the great law of conservation of energy, safely conclude that heat disappeared in the process. And as it is the nature of the *liquid* which most determines both the existence of the current and its direction, it is true thermo-electric action of liquids, and liquids possess true thermo-electric properties.

The heat must, however, be subject in its operation to some guiding condition which determines the kind of effect to be produced, and both the cause, heat, and the effect, electricity, must be essentially related to that condition. Such a condition *appears* to be the acid or alkaline nature of the liquid; as, however, chemical action is not necessary, and different liquids of acid reaction (or of alkaline reaction) give a reverse direction of current, the chemical conditions of acidity or alkalinity are probably only concomitant circumstances, and do not really, but only apparently, determine the kind of effect; and as those conditions are usually of an opposite kind in liquids which yield an opposite direction of current, and are in most cases stronger as the currents are more powerful, they are probably themselves dependent upon some more general condition which itself determines the direction of the currents.

A single clear exception invalidates a general inference, and indicates the existence probably of the action of a second law, or of a more general cause or condition. That the thermo-electric-negative property of a liquid is separable from and may exist without the alkaline property, is clearly proved by the exceptional direction of the current in solutions of selenious acid (No. 157), sulphate of nickel (No. 155), nitrate of silver (No. 147), chrome alum (No. 156), sul-

phate of manganese (No. 148), and probably also sulphate of iron (No. 159); and in a less conspicuous degree in solutions of sulphate of copper (Nos. 15 and 113), sulphate of didymium (No. 154), and other substances.

These anomalous results indicate the existence of a deeper seated condition of the currents than that of the acid or alkaline nature of the liquid, because the currents in those instances are not merely altered in quantity but actually reversed. And this deeper condition must also affect the quantity of the current in different cases, because a diminution of quantity is a step towards a reversal of direction, and an actual reversal is probably only an extreme degree of the same change.

This more abstruse condition consists probably of the structural arrangement of the molecules of the liquid; for we know as a matter of course that tridimensional geometrical, as well as mechanical conditions of the molecules must be fundamentally involved in the action. It is also probable that as the chemical and electrical phenomena in these experiments are usually coincident in their changes, this hidden condition is of a more general kind than either, and determines not only the acid or alkaline reaction of the solution, but also the production and direction of the currents (*i.e.*, it determines whether the heat shall produce a current at all, and also the direction of the current produced); and that these two effects, the chemical and electrical, are but concomitant ones. It is, however, evident that heat acting upon a saline conducting liquid in the manner described is not alone sufficient to cause a current, but that another condition is necessary, because with some good conducting solutions, such as were used in Nos. 47, 49, 51, 57, 67, 69, 75, 90, 122, and especially No. 146, little or no current was produced.

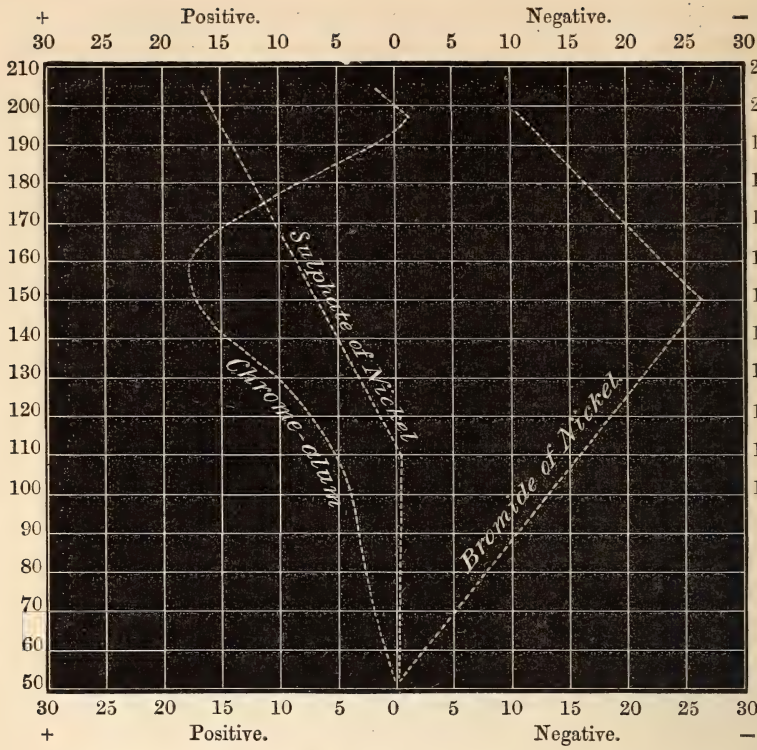
The heat then acts as the real cause of the currents, and a particular molecular state of the liquid probably determines the kind and direction of effect the heat shall produce, and as we also know that the molecular structure of all bodies is profoundly altered by change of temperature, we may reasonably conclude that whilst a portion of the heat is absorbed in temporarily changing the molecular structure, and is again set free when the structure returns to its original state during the process of cooling, another portion is expended and lost in producing the current.

According to this interpretation the phenomenon of the current is a product of three circumstances, viz., 1st, a particular molecular structure of the liquid; 2nd, a change of the latter by alteration of temperature; and 3rd, a direct conversion of heat into dynamic electricity; such conversion being dependent upon the two prior conditions. The second circumstance also agrees with the fact that in certain liquids, for instance, Nos. 130, 139, 155, 156, 157, and 162, the current does

not increase uniformly with the temperature, but in some cases diminishes, and in others, for instance, Nos. 155 and 156, is actually reversed.

The following table and diagram represent the change which occurred with the solution composed of 14 oz. of water and 2 oz. of chrome alum (No. 156); (those of sulphate of nickel (No. 155), and bromide of nickel (Nos. 106 and 139), are also shown for three temperatures each in the diagram). The dotted lines represent the electrical states of the *heated* platinum.

F.		
52°	0.	
60	$1\frac{1}{4}$	→ hot platinum positive.
70	$1\frac{3}{4}$	→.
80	$2\frac{1}{2}$	→.
90	$3\frac{1}{2}$	→.
100	$4\frac{1}{2}$	→.
110	6	→.
120	7	→.
130	11	→.
140	14	→.
150	$16\frac{1}{2}$	→.
160	$17\frac{1}{2}$	→.
170	15	→.
180	$8\frac{1}{2}$	→.
190	2	→.
194	0.	
198	2	← cold platinum positive.
200	1	←.
201	1	→ hot platinum positive.



These diminutions of current and reversals of its direction by rise of temperature indicate irregular molecular changes in the layer of liquid next to the heated plate; and the examination of electrically conducting solutions by this apparatus and process constitutes a new mode of discovering such alterations in them. By obtaining also a sufficient variety and number of instances of such change, for comparison, classification and generalization, we may probably be enabled to infer and discover some general truths respecting molecular action, or gain some knowledge of the molecular conditions necessary for the conversion of heat into dynamic electricity. As also molecular change is a fundamental action in bodies, and many of the secondary properties of substances depend upon molecular state, an alteration of the former is usually attended by more than one of the latter; and we may therefore reasonably infer that the foregoing change in the electrical properties of a solution of chrome alum and other liquids is attended by other coincident physical changes, such, for instance, as that of expansion, or of specific heat capacity.

According to the contact theory of voltaic action, a metal touching another metal, or touching a liquid, tends to produce an electric cur-

rent by the mere contact alone, and when this contact effect of a metal with a liquid differs in strength with two different metals, such metals, each at the same temperature, and immersed in a liquid of that temperature (whether cold or hot, and without the aid of chemical action), generates a current, provided the contact effect of the metals themselves does not neutralise that of the metals and liquid.

Such an effect has been already obtained. Becquerel, for instance, immersed pure platinum and gold in various acids, ammonia, and neutral saline solution, and obtained very feeble currents. He concluded that the state of the surface of the metals had great influence, and that consequently capillary affinity played a very important part in producing the effects. (See "Comptes Rendus Acad. Sci," Paris, 1870, pp. 480, 961, 1313; also "Chemical News," vol. xxii, p. 21, and vol. xxiii, p. 222.) As neither gold nor platinum is perceptibly corroded by dilute acids, nor usually by saline liquids, at ordinary atmospheric temperatures, any electric current produced by such a combination can only be due to contact, either of the metals with each other or with the liquid.

According to these facts, as in the thermo-electric apparatus there is no contact of dissimilar metals, both the contact of the cold metal (as well as that of the hot one) with the liquid, tends to generate a current, and rise of temperature makes the warmer metal only more or less electro-positive than it was previously. Another reason also for concluding that both the cold and the hot piece of metal, at each different temperature, tends to generate a current, is, that with two pieces of the same metal at 60° F. in the apparatus, if one was made colder, a current was produced in the same direction as when the other was heated. The electrical effect, therefore, is a result of a *difference* of action at the surfaces of contact of the two pieces of metal with the liquid; and the apparatus and method may be employed for detecting differences of electric potential between metals and liquids at different temperatures.

In addition to Volta and Becquerel, others, viz., Pfaff, Fechner, Peclet, Matteucci, Kohlrausch, Marianini, Hankel, Zamboni, Buff, and Thomson, have investigated the contact theory of voltaic action, and Clifton has examined the difference of electric potential produced by the contact of metals and liquids (see "Proc. Roy. Soc.," 1877, vol. xxvi, p. 299), but the metals he employed were easily corrodible ones, viz., zinc, iron, and copper.

As in my experiments the origin of the current appears to lie in a conversion of heat into electricity, and heat probably disappears in the process, it is a reasonable inference that, in the above experiments of Becquerel's, &c., heat also disappears and produces the currents; and, if this be correct, then it is also likely that, when a piece of metal is simply immersed in a suitable liquid, the temperature is altered; and

this would be a parallel fact in the science of heat to that of the production of electricity by contact action in electrical science.

This research throws some light upon the origin of the voltaic current, and the results accord with the original contact theory of Volta (of late years revived in this country), that electric currents may be produced between metals and liquids, by mutual contact, without the aid of chemical action.

I should much like to determine, by actual experiment, various points suggested by this research:—1st. Whether, by the mere contact of a metal and liquid, an alteration of temperature is produced. 2nd. To make a model thermo-electromotor, composed of iron and a solution of sodic carbonate; and ascertain its electromotive power, and whether a series of several such cells will electrolyse a liquid. 3rd. To test the effect of rough and smooth surfaces of the plates. 4th. To endeavour to obtain an electric current with iron plates under the influence of magnetism. 5th. To search for other instances of molecular change, also for other physical changes (such as that of irregular absorption of heat) coincident with the molecular ones. 6th. To compare the electromotive power of a platinum pair against one of iron, gold, silver, &c., and to settle other questions which have occurred to my mind.

(Added March 17, 1878.)

The thermo-electric properties of liquids appear to be related to the statical electric effects of the contact of metals with liquids. Peclet found that platinum and gold exhibited, by contact with acidulated water, electric signs contrary to those which would have been produced had those metals been oxidised or chemically corroded; also, that they gave opposite electric signs with solutions of potash, ammonia, or hydrosulphate of potassium, to those which they gave with dilute nitric, hydrochloric, or sulphuric acid. (See "Annal de Chim.," 1841, vol. ii, pp. 233–255; "Arch. de l'Elect.," 1841, vol. i, pp. 621–650; and De la Rive's "Treatise on Electricity," English edition, vol. ii, p. 849.) Pfaff, also, found those metals exhibit opposite electric signs by contact with solutions of caustic potash, soda, or ammonia, to those obtained by contact with strong nitric, hydrochloric, or sulphuric acids. (See Poggendorff's "Annalen," 1840, vol. li, pp. 110–124, 197–212; Wiedemann's "Galvanismus," 1872, vol. i, p. 32; and Peschel's "Elements of Physics," vol. iii, pp. 73–75.) Buff obtained similar results to the above, with platinum in contact with solution of potash, dilute nitric or sulphuric acid, commercial nitric acid, and in fully concentrated nitric acid. (See Liebig's "Ann. de Chem. and Pharm." 1842, vol. xlii, pp. 5–14; "Archives de l'Elect.," 1843, vol. iii, pp. 561–572; Wiedemann's "Galvanismus," 1872, vol. i, p. 29.) Each of these investigators also found platinum become charged with

positive electricity by contact with acids, and with negative by contact with alkalies; and its electrical condition in acids to be the opposite of that which would have been produced by chemical corrosion. All these results agree with mine. The statical electric effects which Pfaff obtained with other liquids, such as solutions of argentic nitrate, cupric sulphate, ferrous and ferric sulphates, and other salts, also agree in the main with the dynamic ones obtained with my apparatus. Rise of temperature, therefore, usually only increased and sustained the difference of electric condition between metals and liquids which contact produced; and this effect was the same in acid and alkaline solutions. As also the direction of the current agreed usually with that of electric polarity produced by contact, if we know that of the one at a particular temperature, that of the other may be predicted from it. These considerations also indicate the existence of a zero of temperature for each pair of substances, below which contact would not develop electricity in them.

(Added March 25, 1878.)

Since writing the above, I have met with a paper by M. Pouillet on "New Phenomena of the Production of Heat,"* in which the author stated, that at the instant a liquid wets a solid, or is absorbed by it, there is a disengagement of heat, and proved this statement by numerous experiments, made with a variety of substances, in such a manner as was likely to secure trustworthy results. The liquids employed were confined to water, oil, alcohol, and acetic ether only; but the solids were very various, chiefly organic substances, but including also (in a state of fine division) porcelain, glass, clay, carbon, sulphur, silica, alumina, magnesia, oxides of chromium, the peroxides of iron and of manganese; also finely-powdered metals, viz., iron, zinc, tin, antimony, and copper. The rise of temperature found with inorganic bodies varied from ·2 to ·5 of a C. degree, and lasted from two to four minutes, and then the temperature returned to its former state. Regnault has confirmed the general result.†

As the mere contact of metals and liquids develops heat, and as (without the aid of chemical action) heat excites an electric current, and the direction of the current produced by heating platinum in contact with either acid or alkaline solutions, agrees with the signs of static electric polarity excited by the contact of that metal with those liquids, we may reasonably conclude that the heat and free electricity, excited by such contact, are related to each other, either as cause and effect, or as coincident effects of some other cause contained in the act of contact.

* "Annales de Chimie et de Physique," 1822, vol. xx, pp. 141–162.

† "Annales de Chimie et de Physique," 1841, vol. i, p. 133. Gmelin's "Handbook of Chemistry," vol. i, p. 300.

"Observations on Arctic Sea-water and Ice." By Staff Surgeon Edward L. Moss, M.D., R.N., late of H.M.S. "Alert." Communicated by Sir George Nares, Captain, R.N., K.C.B., F.R.S. Received May 3, 1878. Read May 23.*

In order that observations on the specific gravity of sea-water should be made in the Arctic Expedition of 1875, by the method successfully used on board Her Majesty's ship "Challenger,"† Sir George Nares, when he left that ship to take command of the expedition, brought with him one of Mr. Buchanan's hydrometers.

Professor Hartley superintended the construction of its graduated weights, and suggested the supply of apparatus for the volumetric estimation of chlorine; and, on the departure of the expedition, both sets of observations were allotted to me.

Storms interfered with observations in the Atlantic, but they were begun on entering Baffin's Sea, and continued till the ships finally rounded Cape Farewell. Most of the samples of water examined were from the surface, or from close beneath the floes; but whenever the exigencies of ice navigation permitted, samples were obtained from various depths by means of "Buchanan's bottle." The "bottle" was also used in winter quarters, at first through holes made in the new ice, and afterwards of greater depths, when tidal fissures formed wide enough to let it pass down between the heavy floes. In very cold weather, or when surface fresh water lay over a salt stratum with a temperature below the freezing point, it was found necessary to be specially careful to prevent the addition or withdrawal of ice by the cold brass of the bottle; and during winter, water, however obtained, had to be re-mixed after it had acquired the temperature of my cabin, for ice formed in transit from the floes to the ship, melted, and lay in a differently refracting layer on the surface. The temperatures were ordinarily taken with an open scaled centigrade thermometer, verified at its zero. Its readings were constantly checked by observations with the Miller-Casella and Negretti and Zambra instruments, by which all the temperatures away from the surface were obtained.

In the reduction of the specific gravities to a common temperature, Hubbard's coefficients were at first used, but all the observations on sea-water in the subjoined tables have been re-calculated by the formulæ published during the absence of the expedition by Professors Thorpe and Rücker.‡ In the few instances in which the observations were made at minus temperatures, the reductions have been arrived at

* See *ante*, p. 446.

† Mr. J. Y. Buchanan, "Proc. Roy. Soc.," vol. xxiii, 1875, p. 301.

‡ "Trans. Roy. Soc.," vol. clxi, Part II, p. 405.

by extrapolation; but taking Hubbard's curve as an extreme, the error possible in the small range of temperature involved cannot extend beyond the fifth place of decimals. The uncertainty of the process has, however, induced me to discard a column in which the specific gravities were expressed at the natural temperatures. In both tables Stampfer's coefficients have been used for the waters from melted ice.

But that an Oertling balance and a reserve of pure nitrate of silver were supplied, the chlorine estimations must have been omitted, for the standard silver solutions were destroyed by freezing—a disaster which considerably curtailed the number of experiments.

The water of the polar basin, to such depths as we reached, had already acquired the low specific gravity characteristic of outflowing polar currents. This low specific gravity was maintained during winter and spring, after nine months' perpetual freezing. It is to be remembered, however, that the samples were from no great depth, and from a zone of the Polar Sea that annually receives, not only its own precipitation, but also the precipitation of the neighbouring shores, and that, too, at a comparatively high temperature.

The channels between the Polar Sea and Smith's Sound contain two strata of sea-water, not owing their temperatures to local causes—an upper stratum of polar water overlies a warmer northward flowing extension of the Atlantic. The specific gravity observations show that the relative position of the layers is due to salinity influencing their density more than temperature. To overlie the polar water, the denser stratum would require a temperature above 10° C. The highest deep temperature obtained was below the freezing point, but there is sufficient range between the specific gravities of the deepest samples obtained and of Atlantic water to permit warmer Atlantic water to exist at a greater depth.

In places where the deep water yet retains some temperature above the freezing point, direct dilution at its own depth, with fresh water from deep icebergs or subglacial streams, may so far reduce its salinity as to let it carry warmth to the surface, and thus help to occasion the well known "rottenness" of floes in the neighbourhood of icebergs and glacier cliffs.

In addition to low specific gravity and temperature, the waters of outflowing polar currents possess a third characteristic in the disturbed proportion between their chlorides and sulphates, pointed out by Forchhammer.* His great range of observations gives an oceanic proportion of 11·87 of sulphuric acid to 100 of chlorine; while his polar currents show an average increase in the proportion of sulphates amounting to ·52. The sealed samples of water brought home by me

* "Phil. Trans.," 1865, Part I, p. 228.

for verifications yielded 11·21 of sulphuric acid* (SO_3) to 100 of chlorine in the deep warm water of Smith's Sound, and 11·59 in the polar water, showing an excess of ·38 in the latter.

Forchhammer attributes this disturbed proportion either to a scarcity of fucoidal plants or to the vicinity of volcanoes.

If it be assumed that the dilution of outflowing polar currents is due rather to shore precipitation than to transference of polar precipitation through the perennial floes, the littoral character of the currents will alone account for the increase of sulphates.

On quitting Robeson Channel, Her Majesty's ship "Alert" found herself on the shores of a sea covered exclusively with the heavy ice before met with only in outlying fragments. The pack edge grounding along the shore in fourteen fathoms, and forced upwards towards the beach, formed a barrier reef, inside which the ship found shelter. As soon as new floe consolidated it, it was found that the grounded masses, resembling icebergs in their size and stratification, differed from them in being salt, and the first chlorine estimations were made in search of ice fit for the ship's use during winter.

On the return of daylight, further observations both on the composition and structure of the ice were begun, in the hope that they would throw some light on the place and mode of growth of the stupendous floes.

All but the surface of the floating ice was out of reach, but plenty of sections were afforded by the great fragments forced upwards on the beach.

A floe grounded a quarter of a mile ahead of the ship had split into several rectangular segments. Its largest masses were separated by a narrow cleft passing vertically through the ice and exposing a fresh section, measuring 47 feet at right angles to the plane of stratification. The floe was belted with a shelf of ice, marking the sea-level previous to its last upheaval. The cleft intersected this tide-mark 11 feet from its top surface. The floe sides under the mark were grooved with the vertical channellings of last season's subaqueous thaw, but neither the tide mark nor the groovings passed into the cleft, and the sharp edges of the latter had evidently not been exposed to the warmth of last summer.

Dr. Rae has pointed out† how readily salt ice may lose its entangled brine by drainage; it thus also loses its cryohydrates when the temperature rises above their freezing points; but since the upheaval of this floeberg was recent, and since the temperature of the air was yet below what the ice possessed when submerged, it was fair

* Precipitated with barium chloride instead of nitrate, as the latter, used by Forchhammer, requires much manipulative skill in its subsequent separation from the sulphate.

† Dr. Rae, "Proc. Phys. Soc.," Part I, p. 14.

to infer that this fragment of floe had lost little of its saltness, certainly none of its salt ice by drainage; moreover, it afforded decided facilities for obtaining samples of ice from every height of a considerable section, and accordingly serial chlorine estimations were made from top to bottom of it.

The lowest part of this section was probably not the bottom of the floe, but samples of deep ice were obtained from an almost overturned mass, exhibiting the mammillary elevations common on the under side of the floes, and in one part studded with stones and grooved by motion against the bottom. Our watering berg offered a section where 30 inches of snow-formed ice lay over what had once been a superglacial pool in the hollows of a "blue-domed" floe. It will be seen, on reference to the tabulated chlorine estimations, that, while the annual floe of one winter's quarters held one-sixth the chlorine of sea water, the most salt parts of the polar floe held but one-fifteenth. The chlorine, however, does not represent all the salts, and water from such ice is quite too brackish to drink.

If the salinity of the polar floes leaves their mode of growth doubtful, evidence much more to the point is supplied by their structure. The upper ice of upheaved segments, not only at Floeberg Beach, but along the shore as far as my sledge journeys extended, displayed more or less distinct horizontal stratification, like that of glacier *névé*. The stratification was strikingly regular and parallel, decreasing in width from above downwards, sometimes ending abruptly, but generally becoming indistinct and gradually disappearing 20 feet or more from the top. Each stratum consisted of an upper white, merging into a lower blue part, the difference in colour depending on the greater or less number and size of the air-cells in the ice. The lower blue passed abruptly, but without break in structure, into the spongy white of the layer beneath. After spring, when all exposed ice-surfaces became white and granular, the stratification was to be detected only by the unequal disintegration of the white and blue parts, causing in well-marked floebergs a crenulated outline in the profile of a section; the blue, hard and unchanged, lying in the angle, and the white, granular and swollen, in the convexity. This unequal disintegration is to be attributed to the lesser transparency of the white parts to solar radiation rather than to any difference in salinity represented by the trifling and possibly experimental difference in specific gravity, shown by estimations Nos. 106 and 107. The indistinctness of stratification produced by summer is not permanent. The old floebergs worn into "blue domes," and "pie" shapes were well marked in early spring, though they had evidently borne the thaws of many seasons. The stratification was most readily seen from a little distance. I have marked it in a sketch of a floeberg at Cape Joseph Henry, made quite a quarter of a mile off, but frequently the limits of

the layers could not be decided upon at arm's length, and in getting samples and measurements the easiest method of procedure was to stick on pieces of paper and adjust them to the layers, by retiring to a distance of 20 or 30 yards. The thinnest strata observed occurred in the sides of a water-worn ravine in a floeberg off Cape Rawson; in the middle of the bank, where the sections were nearly vertical, they measured 7 inches. An iceberg off Cape Napoleon had strata only 4 inches wide. The widest I have any notes of were in the top of the cleft floeberg already described. Here they were 3 feet deep; but, 28 feet lower down, the lowest distinguishable strata were 18 inches deep. Although the extremes differed so widely, the great majority of the stratifications showed layers between 10 and 15 inches wide. In instances where stratification ceased abruptly, the ice immediately beneath was a sort of conglomerate formed of masses broken from older floes cemented together by frozen sea-water,* with differently inclined lines of air-cells, and occasionally enclosing aggregations of bright-yellow *Diatomaceæ*. One such aggregation from a floeberg grounded on an island in Black Cliffs Bay, has been submitted to Rev. Eugene O'Meara. He has identified the *Diatomaceæ* in it, and informs me that they are all of decidedly marine growth. Stratification was not distinguishable in the masses of which the conglomerates were composed; indeed, submersion appears to play an important part in its obliteration.

On the 28th March, 1876, I cut a pit through the snow on the top of our watering berg and filled it in with minute-crystalled snow from a little under the surface. On the 8th of May, the lower crystals were adherent to the ice beneath, and had become distinctly larger and granular, differing little from those in the undisturbed snow beside them, which were by this time like small transparent hailstones, often already grown together into rods and groups. The temperature of the air in the meantime, though rising, had never reached 0° C., while the ice yet retained much of the cold of early spring.

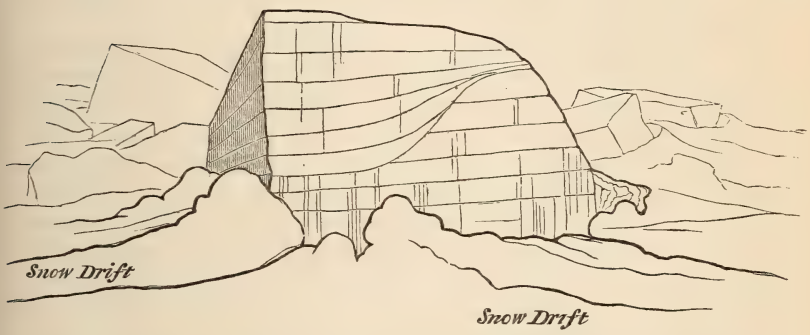
A similar growth towards the cold, but upwards instead of downwards, takes place while the temperature of the air is below that of the floes or the earth, and helps the wind to harden and crust the upper snow. With a hope of testing this growth, two cubes were cut out of the same block of snow, and made to weigh exactly 50 grms. each. No. 1 was suspended level with the ground, and No. 2 4 feet 8 inches above it, in a niche in a snow-house. They were then built up and left for ten days, meantime the temperature of the air averaged −35°·5 C., while the ground under 5 feet 6 inches of snow was −20°·6. On re-weighing, No. 1 had lost 1·15 grms., and No. 2 had gained ·3. After the lapse of a second ten days, with an air temperature of

* The "Porphyritic ice" of Parry, 4th voyage, p. 88.

— 37°·4 C., No. 1 had lost an additional ·9, and No. 2 gained an additional ·6.

Admiral Wrangell, in describing the heavy ice grounded along the northern shores of Siberia, accounts for both its thickness and its stratification by the sliding one upon another of thinner floes.* Admiral Belcher referred some of the great ice mases in Wellington Channel to the same cause,† but the stratification of the floes met with by our expedition cannot be thus accounted for. The behaviour of strata overlying an old surface-dome, best shown in the accompanying little diagram (fig. 1)—a copy of a pencil sketch made from nature—is utterly inconsistent with the sliding-up hypothesis.

Fig. 1.



The subject of this sketch afforded the only instance of stratification overlying a "blue dome" seen by me. Any deviations from the horizontal and parallel were in fact extremely rare, and one of the first convictions forced upon me by the facts of the stratification was that the floes which showed it could not have been formed in such a region of chaotic disturbance as lay about us.

Massive floes drifted from the westward into the channels of the Parry group, or floating southward in Spitzbergen seas, have made arctic travellers familiar with the glassy "blue tops" or domes of ice standing up through the surface snows.‡ The upper part of many of our grounded floes exhibited such domes in section, and in every case the wavy outline cut through the horizontal strata. The larger mounds often occurring in miniature mountain ranges on heavy floes are not to be confounded with "blue domes." They occur along ancient lines of fissure, and are simply old hummocks rounded off by snowdrift.

The occurrence of air-carried dust on the surface of both glacier and

* Wrangell's "Siberian Journey," edited by Sabine, Appendix, p. 393.

† Belcher's "Last of Arctic Voyages," vol. i, p. 101.

‡ The journal of Lieutenant Mecham of the "Resolute" gives a typical description and sketch of them.—Parl. Reports, 1855, vol. xxv, p. 694.

floe ice has been frequently recorded; sometimes it is clearly traceable to neighbouring land, at others, as in the case of the kryokonite of Professor Nordenskiöld,* and the dust of the East Greenland,† and Spitzbergen ice-fields,‡ its source is more or less conjectural.

At Floeberg Beach every breeze strewed the ice with dust from shore, consisting of fine sand from the neighbouring hills (effervescing with acid), and organic *débris* of all sorts, bits of saxifrage and moss and lemming pellets predominating. Such dust was generally confined to the surface of the ice, but a fragment from a bud of *Saxifraga oppositifolia*, recognised by its claw, was found in a dust-line 45 feet deep in the ice of a large berg off the "south ravine," and a dust-line 4 feet deep in the ice of our watering berg, marking the bottom of one of three successive "super-glacial lakes,"§ yielded a lemming hair and a shred of bird's down.

Another kind of dust of less obvious origin commonly occurred in, as well as on, the floes. It was first noticed in the "Cleft Floeberg," already described in connexion with the chlorine estimations. The walls of the cleft exhibited duplicate sections of an old surface pool, and the bottom of the pool was marked with a dust-line. A similar, but fainter, line passing through the whole berg, lay 8 feet deeper down without any appearance of a pool over it. In these and other dust-lines observed in the floes the line consisted of scattered black points in the ice, sometimes two or three in a cubic inch, oftener only the same number in a cubic foot. Each spot consisted of a single air-cell, rarely more than $\frac{1}{20}$ inch in diameter, with its lower half lined with impalpable dust, but otherwise like the air-cells around it. The spot was

Fig. 2.

sometimes quite solid. Specimens were obtained for examination by chipping out lumps of ice holding granules or freighted cells, washing them in their own surface-melting, and putting them into clean bottles till the cells released a little bubble and the dust fell to the bottom, often retaining its shape till shaken. As the surface of a wasting floe progressively lowers, these granular masses are collected into groups, and keep below the general surface in little pits.

Enough dust was obtained from the "Cleft Floeberg" and from the

* Prof. Nordenskiöld's "Expedition to Greenland," Arctic Manual, p. 325.

† "German Arctic Expedition." Koldewey, p. 290.

‡ "Parry's" 4th Voyage, pp. 73–75.

§ "Parry," 4th Voyage, p. 75.

subglacial surface of the salt ice of our watering berg to study its characters *en masse*. Similar dust, but in microscopic quantity, was procured from a line 2 feet deep in a heavy floe, a mile in diameter and two from the shore, drifted into a curve of the coast south of Cape Joseph Henry, and at the same depth from an old "blue top" in Black Cliffs Bay, and also from the dust-line already mentioned as containing the saxifrage leaf at a depth of 45 feet.

A very much worn floe to seaward of the ship supplied a large quantity of dust with precisely similar inorganic contents, but enormously magnified in apparent quantity and converted into a slimy, granular, sour-smelling mud by a growth of a phycochromaceous Alga.* Some of the granules deep in ice were altogether inorganic, others held another Alga composed of groups of dark brown spheres $\frac{1}{3000}$ inch in diameter, associated with spined hemispherical cups $\frac{1}{1600}$ inch diameter, apparently the skins of zygospores. In dust-spots underlying distinctly salt ice *Diatomaceæ* naturally attached† were not uncommon.

The inorganic part of the dust consists of a reddish impalpable sand of even-sized particles, rarely reaching $\frac{1}{200}$ inch in diameter, but averaging one-tenth of that size. Placed in a Sonstadt's solution, specific gravity 2·72, it separates into a red part that floats, and a much smaller dark green part that sinks. The former consists of angular and rounded transparent quartz and rounded red quartz. The fraction that sinks contains rounded grains of hornblende, angular and rounded augite, brown scales of mica, and numerous crystals of magnetite, sharply angular and often imbedded in augite or quartz.

The dust becomes bright red on incineration, but some of the particles still remain magnetic. I searched carefully for, but never found, a magnetic particle capable of reducing sulphate of copper.

Two samples of ice-dust were obtained from icebergs, the one at Cape Frazer, the other at Cape Louis Napoleon. In both instances the dust was in granules imbedded 20 feet or more from the top surface of the berg. Neither contained any organic matter. One remained on the filter-paper aggregated in the granules or oolite-like grains. Both effervesced with hydrochloric acid, but otherwise possessed the same mineral characters as the floeberg dust.

During winter at Floeberg Beach an attempt was made to collect atmospheric dust by means of an extemporised Maddox aereoscope, but it invariably got blocked with snow when there was any wind, and was of course useless when there was none. A sheet of glass, exposed in absolutely calm weather on top of Thermometer Hill, was more successful. When the minute prisms of ice constantly falling (even in an atmosphere so clear that our astronomer made successful observa-

* Identified by Professor Dickie as *Nostoc aureum*.

† Identified by Rev. Eugene O'Meara as *Fragillaria oceanica*.

tions on a star of $3\frac{1}{2}$ magnitude 2° 15′ from the horizon) were swept off into a clean stoppered bottle, and brought on board the ship and melted, a very small, but quite distinct, sediment formed, consisting of quartose particles, a few red grains, and very minute opaque nodules; thus fairly representing the chief constituents of the ice-dust.

I have to regret the incompleteness and absence of pre-arranged order in the observations that were made, but most of the subjects were altogether unforeseen, and it was necessary to take them up and make the best of them just as they presented themselves.

My notes have perhaps little bearing on the place of growth of the so-called "polar floes;" any hypothesis on that subject must give full weight to their wide distribution on both the oceanic and littoral confines of the unknown area; but the stratification of the floes unmistakeably indicates that the precipitation of the region where they form accumulates where it falls. The "conglomerate" sometimes underlying it tells of slow lateral removal by fission and intermediate freezing, while the "blue-topped" floes and the wasting grounded ice mark a region where temperature gathered in summer by the naked land releases the precipitation and restores it to the sea in the shape of outflowing arctic currents of low specific gravity.

Estimations of the Specific Gravity and Percentage of Chlorine in Sea-Waters.
(Arranged according to Latitude.)

Number.	Place.	North latitude.	West longitude.	Date.	Depth.	Temperature *in situ*.	Temperature of observation.	Specific gravity. Water at 4° = unity. At T. of Observation.	Specific gravity. Water at 4° = unity. At 0.	Percentage of chlorine.		Remarks.
1		82° 27′	61° 42′	17 Sept., 1875	Surface.....	−1°4	−1°4	1·02615	1·02608			The sea-water north of 81° 42′ was blue in colour. That in Kennedy Channel and Smith's Sound a decided green.
2		,,	,,	12 Oct., 1875	Under floe	−1·9	−1·1	1·02662	1·02657	1·97		
3		,,	,,	1 Nov., 1875	4 ft. below floe	−1·9	+8·1	1·02576	1·02655	1·97		
4		,,	,,	29 Nov., 1875	,, ,,	−2·1	+6·6	1·02606	1·02671	1·96	†	Some *Copepoda* and one small *Pleurobranchia* were captured in this water.
5	H.M.S. "Alert's"	,,	,,	24 Dec., 1875	Under floe	−2·1	+8·5	1·02516	1·02597	1·92		
6		,,	,,	30 Dec., 1875	27 feet bottom	−2·1	+9·4	1·02573	1·02672	1·95	*	
7	Winter Quarters,	,,	,,	27 Jan., 1876	Under floe	−2·1	+8·3	1·02587	1·02669	1·96	†	
8		,,	,,	6 March, 1876	,, ...	−2·1	+12·5	1·02510	1·02663	1·97		
9	1875–76.	,,	,,	21 June, 1876	27 feet bottom	−1·5	+10·3	1·02517	1·02630	1·85	*	Sealed sample examined after return of Expedition, gave by volume bottle, specific gravity 1·02682 at 0. Water at 4° = unity.
10		,,	,,	15 July, 1876	11 fms. bottom	−1·8	+16	1·02384	1·02608	1·80	†	
11		,,	,,	15 July, 1876	Surface.....	+·1	+16	1·00027	1·00138	·46		
12		,,	,,	21 July, 1876	47½ fms. bottom	−1·7	+9·8	1·02580	1·02687	1·89	*	
13		,,	,,	21 July, 1876	Surface.....	0	+20·5	·99976	1·00110			Chlorine ..... 1·89 per cent.
14	Off Black Cape.....	82 21	61 10	1 Sept., 1875	,, ...	−1·3	+1·4	1·02610	1·02603			Sulphuric acid... ·2146 ,,
15		82 20	61 10	29 May, 1876	Under floe	−1·4	+8·5	1·02603	1·02694	1·93	*	Total solids ..... 3·761 ,,
16		82 15	61 10	2 Aug., 1876	Surface.....	−1·1	+6	1·02607	1·02658			
17	Robeson Channel .	82 6	61 40	30 Aug., 1875	,, ...	−1·3	−1·3	1·02615	1·02608			
18		81 51	63 10	7 Aug., 1876	,, ...	−1·1	+12	1·02460	1·02602			
19	Discovery Harbour	81 42	64 48	28 Aug., 1875	,, ...	−1·3	−1·2	1·02558				
20		81 42	64 48	18 Aug., 1876	7 fathoms	...	+12	1·02473	1·02616			
21		81 40	65 75	20 Aug., 1876	5 ,, ...	...	+12	1·02480	1·02623			
22		80 39	66 5	22 Aug., 1875	Surface.....	−1·6	−1·6	1·02606				
23	Kennedy Channel .	80 36	68 20	20 Aug., 1876	,, ...	−1·1	+6·5	1·02538	1·02592			
24		80 10	69 0	21 Aug., 1875	,, ...	−1·4	+1·4	1·02541				
25		79 47	71 48	23 Aug., 1876	,, ...	−1·7	+14·7	1·02446	1·02643	1·89		Floe ice 400 feet distant.
26		79 47	71 48	24 Aug., 1876	70 fathoms	−1·1	+12·8	1·02606	1·02769	1·99	*	

* Temperature *in situ* by Deep-Sea Negretti and Zambra Thermometer. When not otherwise specified the Thermometer used was an open scaled centigrade instrument, verified at its zero, † Temperature *in situ* by Deep-Sea Miller-Casella Thermometer.

Estimation of the Specific Gravity and Percentage of Chlorine in Sea-Waters.
(Arranged according to Latitude.)—*continued.*

Number.	Place.	North latitude.	West longitude.	Date.	Depth.	Temperature *in situ*.	Temperature of observation.	Specific gravity. Water at 4°=unity. At T. of Obser. vation.	Specific gravity. Water at 4°=unity. At 0.	Percentage of chlorine.	Remarks.
27		79° 46′	71° 13′	19 Aug., 1875	Surface.....	−1·2	−1·2	1·02558	1·02551		
28		79 40	71 45	17 Aug,, 1875	,, ...	− ·6	− ·6	1·02500	1·02496		
29		79 40	72 10	25 Aug., 1876	,, ...	−1·4	+13·4	1·02443	1·02612		
30		79 40	72 50	28 Aug., 1876	9 feet	−1·1	+13·4	1·02224	1·02382		
31		79 40	72 50	28 Aug., 1876	20 fathoms.....	−1·5	+13·5	1·02506	1·02679		Full of *Fragillaria*.
32		79 40	72 50	28 Aug., 1876	40 ,, ...	−1·2	+13·4	1·02553	1·02727		Contained a few rods of *Fragillaria*.
33		79 32	73 10	31 Aug., 1876	115 ,, ...	− ·6	+12·4	1·02634	1·02791	2·01	* Two litres filtered through a small plug of cotton wool yielded no organism.
34		79 37	71 13	16 Aug., 1875	Surface.....	−1·0	− 1	1·02588	1·02583		
35		79 39	74 13	4 Sept., 1876	Under floe	− ·1	+ 7·8	1·00358			
36		79 30	74 13	4 Sept., 1876	2 fathoms	−1·1	+11	1·01835			* A litre filtered yielded two rods of *Fragillaria*.
37		79 30	74 13	4 Sept., 1876	3 ,, ...	−1·4	+14	1·02422	1·02602		*
38		79 21	74 40	10 Aug., 1875	2 feet...	− ·3	+ 1	1·00600			Sealed sample examined after return of Expedition, gave by volume bottle, specific gravity 1·02724 at 0. Water at 4° = unity.
39		79 21	74 40	10 Aug., 1875	15 fathoms	−1·4	+10	1·02523	1·02631		†
40		79 17	74 35	8 Sept., 1876	56 ,, ...	−1·1	+ 8·2	1 02641	1·02724	1·95	*
41	Buchanan Strait ...	79 8	74 28	9 Sept., 1876.	Surface.....	−1·7	+ 8	1·02486	1·02561		
42		79 2	76 20	6 Aug., 1875	,, ...	− ·2	+ 2·2	1·02440	1·02454		Chlorine ..... 1·95 per cent.
43	Payer Harbour.....	78 52	76 0	5 Aug., 1875	,, ...	+1	+ 3·3	1·02492	1·02513		Sulphuric acid... ·2187 ,,
44		78 43	74 24	1 Aug., 1875	,, ...	0	+ 7·6	1·02590	1·02693		Total solids ..... 3·7922 ,,
45		78 43	74 24	2 Aug., 1875	,, ...	0	+ 6	1·02580	1·02638		
46		78 40	74 23	9 Sept., 1876	,, ...	−1·3	+ 8·3	1·02539	1·02627		
47		77 30	74 30	10 Sept., 1876	,, ...	−1·0	+11·2	1·02503	1·02633		
48		77 16	71 5	11 Sept., 1876	,, ...	−1·0	+14	1·02444	1·02626		
49		76 33	70 37	13 Sept., 1876	,, ...	0	+ 9	1·02503	1·02594		*Sagittæ* and two genera of *Appendicularia* together with *Ceratium* and other *Peridineæ* occurred in samples of water raised for examination in Smith's Sound.
50		76 9	73 16	14 Sept., 1876	,, ...	−1·0	+13·8	1·02422	1·02597		
51		75 20	66 19	25 July, 1875	,, ...	+3·3	+ 3·3	1·02467	1·02488		
52		75 10	75 5	15 Sept., 1876	,, ...	+1·1	+10·2	1·02553	1·02666		Filter choked with *Ceratium tripos*.
53		74 19	61 72	19 Sept., 1876	,, ...	+ ·5	+11·5	1·02460	1·02592		
54		73 50	69 44	18 Sept., 1876	,, ...	+2·0	+ 9	1·02564	1·02657		
55		73 40	73 7	17 Sept., 1876	,, ...	− ·8	+ 7	1·02460	1·02521		
56		73 33	63 19	24 July, 1875	,, ...	−1	+ 9	1·02162	1·02241		Many bergs in sight.

57		73 33	76 59	16 Sept., 1876	,, ...	− ·5	+13·8	1·02338	1·02509		
58		73 33	58 48	20 Sept., 1876	,, ...	+1·0	+11·2	1·02503	1·02631		A change from green to blue in colour of the water.
59		73 30	56 56	23 July, 1875	,, ...	+1·7	+10	1·02470	1·02576		
60		73 07	58 26	21 Sept., 1876	,, ...	+1·0	+ 9·8	1·02512	1·02614		
61		71 58	60 19	22 Sept., 1876	,, ...	0	+11·2	1·02414	1·02539		
62		71 12	57 37	24 Sept., 1876	,, ...	+2·5	+ 8·2	1·02574	1·02655		Very blue in colour.
63		70 23	55 53	24 Sept., 1876	,, ...	+2·0	+ 7·8	1·02513	1·02585		
64		69 45	54 35	24 Sept., 1876	,, ...	+2·8	+10·9	1·02560	1·02663	1·92	Close to position of Rink's sample, July, 1849, examined by Forchhammer.
65	Egedesminde Harbour	69 20	54 26	25 Sept., 1876	,, ...	+2·8	+11	1·02527	1·02652		
66				30 Sept., 1876	,, ...	+3·3	+10	1·02624	1·02736		
67		67 59	55 37	3 Oct., 1876	,, ...	+3·6	+ 8	1·02639	1·02719		
68		66 40	54 30	4 Oct., 1876	,, ...	+3·6	+10	1·02553	1·02662		
69		64 46	56 29	6 Oct., 1876	,, ...	+3·3	+ 5	1·02531	1·02579		
70		63 42	54 32	7 Oct., 1876	,, ...	+3·3	+12	1·02457	1·02599		
71		61 55	54 41	8 Oct., 1876	,, ...	+5·5	+14·2	1·02499	1·02688		
72		58 39	46 4	26 June, 1875	,, ...	+5·0	+ 9	1·02670	1·02767		
73		58 13	54 7	10 Oct., 1876	,, ...	+6·6	+10·2	1·02676	1·02793		
74		55 44	35 38	19 Oct., 1876	,, ...	+7·8	+15	1·02620	1·02833		

* Temperatures *in situ* by Deep-Sea Negretti and Zambra Thermometer. When not otherwise specified the Thermometer used was an open-scaled centigrade instrument, verified at its zero. † Temperatures *in situ* by Deep-Sea Miller-Casella Thermometer.

Estimations of the Percentage of Chlorine, and Specific Gravity of Water from various sorts of Sea Ice.

Number.	Date of observation.	Nature of the ice.	Position and character of sample.	Percentage of chlorine.	Specific gravity of ice water at 0°, unity at 4° C.
		Polar floe.	"*Cleft Floeberg*." A mass of polar floe, grounded 400 yards north of H.M.S. "Alert's" winter quarters, afforded a recent fissure where samples of ice could be obtained from various depths of a section measuring 47 feet across the planes of originally horizontal stratification.		
75	26 Feb., 1876.		1 foot from surface	·033	
76	16 March, „		5 feet „ „	·104	
77	14 „ „		11 „ „ „	·126	
78	16 „ „		15 „ „ „	·156	
79	18 Feb., „		19 „ „ „	·059	
80	18 „ „		21 „ „ „	·071	
81	18 „ „		22 „ „ „ undulating bed of an old "superglacial pool" interrupting the horizontal strata under it, and marked by an irregular line in which the air-cells of the ice are here and there coated with atmospheric dust	·041	1·00025
82	26 „ „		25 feet from surface	·097	
83	28 „ „		25 „ „ „ but 40 feet from last	·100	
84	16 March, „		30 „ „ „ occasional dust-coated air-cells	·123	
85	14 „ „		38 „ „ „	·031	
86	11 „ „		41 „ „ „	·117	
87	26 Feb., „		47 „ „ „	·040	
88	14 March, „		47 „ „ „ 1 foot deeper into ice than last	·036	
89	14 „ „		47 „ „ „ 10 feet from last	·079	
90	28 Feb., „		Old tide ridge projecting as a shelf from side of floeberg, and from 9 to 16 feet from top	·085	

		„ „	"*Glaciated Floeberg.*" An inverted berg exposing part of its bottom, which has been in contact with the beach, and is studded with scratched stones.		
91	11 March, „		Bottom ice with very small air-cells in planes	·102	1·00162
92	24 „ „		5 feet from, and originally above first sample	·107	
93	24 „ „		10 „ „ „ „ „	·101	
94	24 „ „		19 „ „ „ „ „	·146	
		„ „			
			"*Watering Floeberg*" aground in 80 feet of water. Top surface measures 245 feet by 171. The small part of its section which is exposed shows—		
95	27 „ „		1st. A foot of recent snow, becoming granular in its lower four inches	·0007	
96	16 Oct., 1875.		2nd. 30 inches of white ice with numerous large air-cells never submerged	·0003	1·00013
97	25 March, 1876.		The same in another place 50 feet off	·006	
98	24 „ „		3rd. Ice of a frozen "superglacial pool" without cells	·003	
99	25 „ „		The same 50 feet off	·004	
100	25 „ „		Centre and last frozen parts holding vertical air-tubes as large as crow-quills.	·030	
101	25 „ „		4th. The usual blue cellular ice with its original undulating surface dotted with dust-lined cells. Highest undulation 2 feet 6 inches from present surface	·053	
102	25 „ „		6 feet 6 inches from present surface	·078	
103	24 „ „		10 feet; the lowest attainable	·137	
104	24 „ „	„	"*Pyramid Floeberg*" at the end of "Ladies' Mile" ice above a "dust layer" 45 feet from top surface	·053	
105	24 „ „		Ice 3 feet below	·017	
			Pie-shaped floeberg near ship. The unequal decay of blue and white parts of the stratification causes the outline of the dome to be crenulated.		
106	21 June, „		White and convex part of stratum	..	1·00067
107	21 „ „		Blue in recess	..	1·00077

Estimations of the Percentage of Chlorine, and Specific Gravity of Water from various sorts of Sea Ice—*continued.*

Number.	Date of observation.	Nature of the ice.	Position and character of sample.	Percentage of chlorine.	Specific gravity of ice water at 0°, unity at 4° C.
108	23 Aug., 1876.	Iceberg.	Grounded iceberg off Cape Louis Napoleon (opposite Humboldt Glacier), samples unavoidably taken from below tide mark. The exposed cliff of the berg exhibits innumerable regular horizontal strata 4 inches deep, below two somewhat deeper above	·010	1·00028
109	30 „ „	„ „	Iceberg in Dobbin Bay	·001	1·00010
110	5 May, „	Annual floe.	One season floe, formed between the barrier of grounded polar floes and the shore, 18 inches deep	·203	1·00409
111	5 „ „		3 feet deep	·357	1·00479
112	5 „ „		6 „	·363	1·00522
113	19 April, „		Frozen sea-water free of snow from ice of "fire hole" 40 days frozen	·456	1·00656
114	21 March, „		Frozen sea-water from ice of tide hole	·675	
115	9 May, „		Ice fresh formed in temperature −17°·7 where sea-water rises about the grounded "floebergs"	·860	1·01298
116	21 March, „	"Slush."	Pasty slush over the ice and snow where water has risen in tide cracks, remaining unfrozen in temperature −29° C.	5·112	1·06118
117	8 May „	Efflorescence.	Efflorescence forming in temperature at −17°·7 C. after the sun had left the surface of "floeberg" ice containing chlorine percentage ·078	·178	

118	9 „ „		Efflorescence in temperature −16° similarly forming on "floeberg" ice with chlorine percentage ·126	·216	
119	9 „ „		Efflorescence in temperature −17°·7 from new floe ice with chlorine percentage ·860. This efflorescence melted at −3°·4 C.	2·787	
120	4 March, „		Efflorescence at temperature −31°·8 on fresh frozen "slush."	3·789	
121	16 June, „	Icicles from floebergs.	Icicles depending from "floeberg" ice containing ·018 per cent. chlorine	·004	
122	6 March, „	Successive crops of ice from sea-water.	1,529 cub. centims. of sea-water, sp. gr. at 4° 1·02630, percentage of chlorine 1·979, exposed in a beaker to air at temperature −5° and constantly stirred, yielded crystals above −1°·5. The crystals drained and pressed in gauze melted below −1°·3, and water from them measured 165 cub. centims.	1·525	1 02100
123			A second crop from mother-liquor, formed above −1°·8 and melted below −1°·7, measured 410 cub. centims.	1·624	1·02176
124			A third crop, formed above −2°·0 and melted below −1°·7, measured 390	1·819	1·02475
125			A fourth crop, formed above −2°·7 nearly all melted below −1°·1, but a few crystals remained unmelted in −1°·7, measured 354	2·003	1·02746
126			The mother brine remaining measured 210 cub. centims., and it alone deposited bubbles on the sides of the containing vessel	2·987	1·04033

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END OF THE TWENTY-SEVENTH VOLUME.

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Austen (N. Laurence.) Natural History Papers, and Memoir, edited by Frank Buckland. In Memoriam. 8vo. *London* 1877. Mrs. Austen.

Duncan (P.M.), F.R.S. Cassell's Natural History. Vol. I. 8vo. *London* 1877. The Publishers.

Fayrer (Sir J.), F.R.S. Destruction of Life by Wild Animals and Venomous Snakes in India. 8vo. *London* 1878. The Author.

Ferguson (J.) Introductory Address. 8vo. *Glasgow* 1875. Recent Inquiries into the Early History of Chemistry. 8vo. 1876. Biographia Paracelsica, an Examination of Dr. F. Mook's Theophrastus Paracelsus eine Kritische Studie. 8vo. *Glasgow* 1877. The Author.

Jordan (W. L.). Lecture on the Winds, Ocean Currents, and Tides. 8vo. 1877–78. The Author.

Parkes (Dr. E. A.), F.R.S. A Manual of Practical Hygiene. Fifth Edition. Edited by F. S. B. François de Chaumont. 8vo. *London* 1878. The Editor.

PROCEEDINGS OF

THE ROYAL SOCIETY.

VOL. XXVII. No. 186.

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March 7, 1878.

F. A. ABEL, C.B., Vice-President, in the Chair.

The Right Hon. Sir William Henry Gregory was admitted into the Society.

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, as follows:—

Henry James Alderson, Lieut.-Col. R.A.
Thomas Clifford Allbutt, M.A., M.D.
John Attfield, Ph.D., F.C.S.
John Gilbert Baker, F.L.S.
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It is important to bear in mind that the lines recorded in the foregoing tables are in most cases the very longest visible in the photographic region of the respective spectra. In some cases they are limited to the region 39–40, which I have more especially studied. So that the fact of their being reversed in the solar spectrum must be considered as the strongest evidence obtainable in favour of the existence in the sun of the metals to which they belong, pending the complete investigation of their spectra.

Where, however, there is only one line, as with Li, Rb, &c., the presence of these metals in the sun's reversing layer can, for the present, only be said to be probable. Neither must it be forgotten that in addition to the long lines which a spectrum may contain in the red, yellow, or orange, long lines may exist in the hitherto unexplored ultra-violet region, so that the necessity for waiting for further evidence before deciding finally upon the presence or absence of such metals in the sun will be rendered obvious.

It will be thought remarkable that if the long lines of such metals as lithium and rubidium are found in the photographic region of the spectrum, the long lines (Li, W.L. 6705; Rb, W.L. 6205 and 6296), should have escaped detection.

To this it may be replied that, although these red lines may be apparently the brightest to the eye, it by no means follows they are the longest, since they are situated in a part of the spectrum which affects the visual organ more strongly than the photographic region does. It is possible also that the reasoning I have lately used in a paper communicated to the Society on the spectrum of calcium may be applied in these cases.

Since a sensitized film is affected by some rays more strongly than by others in determining the lengths of lines from a photograph, it is not fair to compare together portions of the spectrum separated by too great an interval.

Furthermore the fact of these red lines having been overlooked in the solar spectrum is not conclusive proof of their absence, inasmuch as this portion of the spectrum is both brighter and less refrangible, and a greater degree of dispersion would be necessary when prisms are employed to render visible faint dark lines which are easily detected in the photographic region.* I hope to be able to make special search for these lines on some future occasion.

For metals having long lines in the green a special search was made. The long thallium line (W.L. 5349) was photographed, but no distinct evidence of a corresponding solar line was obtained.

Two long silver lines were found also, about W.L. 4018 and 4212,

* It is significant that there is a dark line near the position of the Li line both in Ångström and Kirchhoff's maps not assigned to any metal.

but these lines, which are reversed, are of such great width that it is at present impossible to say whether they are coincident with lines in the solar spectrum.

March 28, 1878.

ERRATUM.

Throughout Plate 4 the letter $\varkappa$ ought to match in size the letter α.

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ERRATA.

Page 78, lines 13 and 27, and page 79, line 16, *for* Greenwich-Kew *read* Kew-Greenwich.

Page 119, line 8, *for* hypochloric *read* hydrochloric.

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positive electricity by contact with acids, and with negative by contact with alkalies; and its electrical condition in acids to be the opposite of that which would have been produced by chemical corrosion. All these results agree with mine. The statical electric effects which Pfaff obtained with other liquids, such as solutions of argentic nitrate, cupric sulphate, ferrous and ferric sulphates, and other salts, also agree in the main with the dynamic ones obtained with my apparatus. Rise of temperature, therefore, usually only increased and sustained the difference of electric condition between metals and liquids which contact produced; and this effect was the same in acid and alkaline solutions. As also the direction of the current agreed usually with that of electric polarity produced by contact, if we know that of the one at a particular temperature, that of the other may be predicted from it. These considerations also indicate the existence of a zero of temperature for each pair of substances, below which contact would not develop electricity in them.

(Added March 25, 1878.)

Since writing the above, I have met with a paper by M. Pouillet on "New Phenomena of the Production of Heat,"* in which the author stated, that at the instant a liquid wets a solid, or is absorbed by it, there is a disengagement of heat, and proved this statement by numerous experiments, made with a variety of substances, in such a manner as was likely to secure trustworthy results. The liquids employed were confined to water, oil, alcohol, and acetic ether only; but the solids were very various, chiefly organic substances, but including also (in a state of fine division) porcelain, glass, clay, carbon, sulphur, silica, alumina, magnesia, oxides of chromium, the peroxides of iron and of manganese; also finely-powdered metals, viz., iron, zinc, tin, antimony, and copper. The rise of temperature found with inorganic bodies varied from ·2 to ·5 of a C. degree, and lasted from two to four minutes, and then the temperature returned to its former state. Regnault has confirmed the general result.†

As the mere contact of metals and liquids develops heat, and as (without the aid of chemical action) heat excites an electric current, and the direction of the current produced by heating platinum in contact with either acid or alkaline solutions, agrees with the signs of static electric polarity excited by the contact of that metal with those liquids, we may reasonably conclude that the heat and free electricity, excited by such contact, are related to each other, either as cause and effect, or as coincident effects of some other cause contained in the act of contact.

* "Annales de Chimie et de Physique," 1822, vol. xx, pp. 141–162.

† "Annales de Chimie et de Physique," 1841, vol. i, p. 133. Gmelin's "Handbook of Chemistry," vol. i, p. 300.

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February 21, 1878.

February 28, 1878.

ERRATUM.

Vol. 26, p. 511, for $y = \mathrm{D}d$, read $y = \mathrm{D}d^x$.

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