THURSDAY, MARCH 26, 1874

THE SCIENCE COMMISSION'S MUSEUM REPORT

THE Royal Commission on Scientific Instruction and the Advancement of Science have just issued their fourth Report, which is mainly concerned with the principal public Scientific Museums and Collections of the Metropolis, touching also briefly on the Scientific Museums and Botanic Gardens of Edinburgh and Dublin, and at some length upon provincial Local Museums generally, and upon the means by which these last might be made widely beneficial for scientific instruction. The Report also deals with the subject of Public Lectures in connection with Museums.

The Metropolitan Museums dealt with in the Report are the British Museum, the Museum of the Royal College of Surgeons, the National Botanical Collections and Gardens, the Museum of Practical Geology, and the South Kensington Museum, with its branch at Bethnal Green. The Report of the Commission is founded upon a thorough investigation into the growth and present condition of these institutions, and the opinions of a large number of men competent to speak on the subject as to the best means of systematising the various institutions, and of enabling them to discharge efficiently the objects for which they exist. The following are the principal recommendations of the Commissioners, which we prefer to give in the words of the Report.

With regard to the Natural History Collections of the British Museum, it is recommended :—

"That a Director be appointed by the Crown, and should have the entire administration of the establishment, under the control of a Minister of State, to whom he should be immediately responsible. That the appointments of keepers and other scientific officers should be made by the Minister, after communication with the Director and with the Board of Visitors.

"That a Board of Visitors be constituted, to be nominated in part by the Crown, in part by the Royal and certain other Scientific Societies of the metropolis, and, in the first instance, in part also by the Board of Trustees; the members to be appointed for a limited period, but to be re-eligible; and that the Board of Visitors should make annual reports to the Minister, to be laid before Parliament, on the condition, management, and requirements of the Museum, and should be empowered to give him advice on any points affecting its administration."

With regard to the National Botanical Collections and Gardens, the Commission recommend :—

"That the collections at the British Museum be maintained and arranged with special reference to the geographical distribution of plants and to palæontology; and that the collections at Kew be maintained and arranged with special reference to systematic botany.

"That all collections of recent plants made by Government expeditions be, in the first instance, sent to Kew, to be there worked out and distributed, a set being reserved for the British Museum; and that all collections of fossil plants made by Government expeditions be sent to the British Museum.

"That opportunities for the pursuit of investigations in physiological botany should be afforded in the Royal Gardens at Kew."

With regard to the Museum of the Royal College of Surgeons, it is recommended :—

"That, should the fund at the disposal of the College prove inadequate for the efficient maintenance and continued extension of the Museum, it should receive support from the State, as an institution intimately connected with the progress of biological science in this country."

With regard to the Scientific Collections of the South Kensington Museum, the Commissioners recommend :—

"The formation of a collection of physical and mechanical instruments; and they submit for consideration whether it may not be expedient that this collection, the collection of the Patent Museum, and that of the scientific and educational department of the South Kensington Museum, should be united and placed under the authority of a Minister of State."

With regard to Provincial Museums, the Commissioners recommend :—

"That, in connection with the Science and Art Section of the Education Department, qualified naturalists be appointed to direct the collection of specimens in order to supply whatever deficiencies exist in the more important provincial museums; and also in order to organise typical Museums, to be sent by the Department of Science and Art into the provinces to such Science Schools as may be reported to be likely to make them efficient instruments of scientific instruction.

"That a system of inspection of provincial museums be organised with a view of reporting on their condition, and on the extent to which they are usefully employed, and whether the conditions of the loan or grant from the Department of Science and Art have been fulfilled."

The final recommendations are on the subject of Lectures, and are : —

"That courses of lectures be given in connection with the collection of physical and mechanical instruments, the object of these lectures being to illustrate the progress of scientific and mechanical discovery and invention.

"That the establishment of lectures on Science, accessible to all classes on the payment of a small fee, should be promoted by the Government in the great centres of population.

"That, in the first instance with the view of carrying out the preceding recommendation, the system of instruction of this kind, which has already been established by the Government in the metropolis, should be developed by the institution of courses of lectures on the principal branches of Experimental and Natural Science.

"That the proposed lectures be of two kinds (1) lectures of an elementary character on the general principles and most important facts of Science; (2) lectures specially intended for the working classes on the application of Science to the arts and industries of the country."

Until this Report was issued no general survey had ever been taken of our Museum system, if that can be called a system the growth of which has been almost entirely the result of accident. Both in the metropolis and in the provinces there exists a large number of museums and of collections of various kinds—to a large extent, however, connected with Natural History, and in local museums with Antiquities—but in almost every case, when the history of any of these institutions is traced, it will be found that it had its origin in quite an accidental way, and that no well-defined and intelligent system has been followed in the establishment of those institutions meant for public instruction. Some of the consequences of this capricious birth and untrained growth of the institutions referred to are that, especially in the metropolis, we have a heterogeneous collection of museums that have no relation whatever to each other, each pursuing the even tenor of its way without any regard to its neighbours ; the collections in these museums often overlap each other, thus wasting means that might be expended to better purpose in developing some well-organised common system of aid to scientific research and instruction, and consequently some departments of Science are represented and endowed almost to excess, while others of at least equal importance are not represented at all ; and although all of them have ostensibly the same objects in view, viz. to afford facilities for scientific research and for the scientific instruction of the public, some are directly under the control of a Minister of State, others are not.

If the recommendation of the Commission, that the government of the British and Patent Museums be transferred to a department of State with a responsible Minister at its head, is adopted by the Government, no doubt some of these anomalies will be abolished ; the institutions will be made to fit into each other, and their government will be reorganised on some common and intelligent system, such as that recommended by the Commission.

One of the most glaring of these anomalies is the almost exclusive representation and endowment in our public museums of the Natural Sciences ; Botany, *e.g.*, being twice endowed, in the British Museum and at Kew—while the Physical Sciences, as if they were the illegitimate offspring of man and nature, are left to pick up a living as best they may, so that had it not been for their inherent vitality they would long ago have been starved out of existence.

In our Museums and Gardens, and elsewhere, aid to research in connection with the Biological Sciences is well provided for, while students of Botany, of Zoology, and of Geology in its various departments, have abundant facilities afforded them for the practical study of these sciences. The result is that there is nothing to check the career of these sciences ; they have been rapidly extending their domain, and may go on extending it still further without much anxiety as to where the sinews of war are to come from; all this with the very best results to our country. Our readers need no reminding of the immense strides recently made by Physical Science in its various departments, departments increasing in number and complication, and of the vistas of possible discovery of the most stupendous character which have been opened up, but for which private enterprise is utterly inadequate, and which must remain shrouded in mystery unless assistance similar to that which has been so amply accorded to the favourite Natural History Sciences be also given to the hitherto neglected Physical Sciences. Physical Science, though she sees many a glorious world that she longs to conquer, and whose conquest would be fruitful of the best results, can only in sadness let her hands dangle idly by her side, because unaided she cannot reach these fields of battle. No one competent to pronounce an opinion would venture to say that Physical Science has done less for the material prosperity of this country than Natural Science ; indeed within the last few years our rapid advance has been almost entirely owing to the practical application of physical discoveries. Yet what encouragement is held out to those who are able and willing to devote themselves to research which brings no profit to the researcher, but which is fraught with ultimate benefit to the race ?

The public, as we have shown, is made familiar in our museums with the results which have been reached in the Natural History Sciences, as well as with their *matériel*, but looks in vain for any exhibition of the instruments, the methods, and equally valuable results which belong to Physical Science ; hence, no doubt, partly the reason why the latter has been hitherto almost entirely left out in the cold ; it is not known, and has but little opportunity of letting the public know its history and achievements, though it has something to show of at least equal value with the umbrella or the boomerang of a conquered savage. In chemistry, heat, light, sound, electricity, astronomy in its various branches—if a student wishes to have something more than a mere book knowledge of the methods of work and of the results obtained (and there are many such students), where can he obtain it in the same way as students of Zoology, Botany, Geology, Comparative Anatomy, and Physiology can carry on the practical study of these sciences ? And yet no one, we dare say, would venture to give any better reason for this state of things than that Chance, which has hitherto governed the growth of our museums, has ordered it so.

There can be now, however, no possible excuse for the continuance of this anomalous system, seeing that the Report of the Commission has thoroughly exposed it, and suggested methods whereby to some extent its glaring defects and anomalies may be remedied. If Government wish to find a model for their guidance in reorganising and supplying the deficiencies of our public museums and institutions intended for the researches of students and for popular instruction, let them turn to Appendix III. of the Report of the Commission, containing the Report of the Secretary on the Aid given by the State to Science in France. In a previous article (vol. ix. p. 217) we referred to the disgraceful condition of our Patent Museum, and contrasted it with the magnificent Conservatoire des Arts et Métiers of Paris, extracts from the long and complete catalogue of which, as well as syllabus of its well-organised courses of lectures, will be found in the Appendix referred to.

> "Evil is wrought by want of thought,
> As well as by want of heart."

Government can no longer plead the excuse implied in Hood's lines for neglecting to remedy the evils so forcibly brought under their notice by the Report of the Commission. If means are not forthwith taken to organise our public museums and institutions for scientific research and instruction on some intelligent system, to supplement their lamentable deficiencies, and make them as widely beneficial to the advancement of Science in all its departments and conducive to the highest instruction of the public as they are calculated to be, it can no longer be set down to ignorance, but to an utter disregard to the highest welfare of the country. In this direction the new Government have a chance of distinguishing themselves and winning for themselves an enduring and worthy popularity ; let them lose no time in showing their wisdom by appointing a responsible Minister of Education whose duty it will be to keep all our public scientific and educational institutions up to the highest pitch of efficiency, to re-organise them upon some common basis, and to see that the progress of research in all branches of Science is not hampered by the want of adequate means for its pursuit. By such means will our rulers show themselves to be the real well-wishers and benefactors of their country.

TODHUNTER'S "MATHEMATICAL THEORIES OF ATTRACTION"*

A History of the Mathematical Theories of Attraction and the Figure of the Earth from the time of Newton to that of Laplace. By I. Todhunter, M.A., F.R.S. Two vols. (London: Macmillan, 1874.)

II.

OF the great Scotch mathematician, Maclaurin, we read—"The importance of his investigations may be seen by observing how great has been his influence on succeeding writers. Clairaut, D'Alembert, Lagrange, Legendre, Gauss, Ivory, and Chasles show by reference, explicit or implicit, their obligations to the creator of the theory of the attraction of ellipsoids.

Maclaurin well deserves the memorable association of his name with that of the great master in the inscription which records that he was appointed Professor of Mathematics at Edinburgh, "*ipso Newtono suadente.*" His main contribution to the theory of the figure of the earth was an exact demonstration of Newton's postulate, of which only approximate solutions had previously been given. We may note on § 260 that we have seen the French version of his "Treatise on Fluxions," "traduit de l'Anglois, par le R. P. Pezenas, Paris, 1749; 2 vols." The first volume has li. pp. of Introduction; v.-viii. *Avertissement par le traducteur* ix.-li., Translation of Author's Preface and Introduction with *Table des Matières;* then 344 pp. of text, plates, and 4 pp. of errata. The second volume has viii. pp. of contents, 322 pp. of text, plates, with 6 pp. at end (4 pp. of errata), for errata, *approbation*, and *privilège*.

The next noteworthy name is again that of an English writer, Thomas Simpson. His contributions are of eminent importance. The analysis he employed, Mr. Todhunter observes, "would not have been unworthy of Laplace himself." There is here an interesting biographical note of a kind which the writer so well knows how to introduce, and which adds a charm to the more general details. In writing our notice we have especially dwelt upon the English contributors to our subject; on the whole it can hardly be denied "that Newton's countrymen have left to foreigners the glory of continuing and extending his empire." Singularly enough Mr. Todhunter gives no account of Simpson's work, "A New Treatise of Fluxions . . . with a variety of new and curious problems." London, 1737. 8vo. This is six years earlier than the "Mathematical Dissertations." Problems XXI. to XXIII. (§§ 201-206) deal with attractions of a circular plate on a point on the axis; of a cylinder on a point on its axis; of a sphere on a particle on its surface, or any distance above it, for law varying as inverse of $(\text{distance})^2$ and for $(\text{distance})^n$. They correspond to Problems II., IV., V., VI. of 1823 edition of the "Doctrine and Application of Fluxions."

The great work of Clairaut, "Théorie de la figure de la terre," &c., appeared in 1743. In this branch "no other person has accomplished so much as Clairaut; and the subject remains at present substantially as he left it, though the form is different. The splendid analysis which Laplace supplied adorned but did not really alter the theory which started from the creative hands of Clairaut." Laplace, too, places it "au rang des plus belles productions mathématiques."

The expedition to Peru gave rise to much paper warfare, and Mr. Todhunter has collected together, in a useful form, the titles of the original pamphlets. We think he has overlooked the following, "Nouveau projet d'une mesure invariable propre à devenir universelle, extrait d'un memoire lû . . . le 24 avril, 1748, par M. de la Condamine," viii. pp. A copy we have consulted of No. xx. (p. 236) is dated "Plombières, juin 30, 1754." (Consult Lalande, p. 455.)

D'Alembert need not long detain us. Laplace points out that his writings want "clarté." Mr. Todhunter says of him, "The errors of D'Alembert are certainly surprising; they seem to me to indicate that he was little in the habit of enlarging his own views by comparing them with those of others. His criticisms of Clairaut prove that he had not really mastered the greatest work which had been written on the subject he was constantly studying. His readiness to publish unsound demonstrations and absolute errors is abundantly shown in the course of our criticism. On the whole the blunders revealed in the history of the 'Mathematical Theory of Probability,' and in the present history, constitute an extraordinary shade on a fame so bright as that of D'Alembert."

Here we must give an account of a work not mentioned in the History. The "Considerazioni sopra la Figura della Terra"* of Tommaso Narducci appeared about the year 1747. It comprises two Lemmas (in modern geometrical conics they would be for the ellipse (1), GN equal $\frac{CB^2\,CN}{CA^2}$, (2) radius of curvature equal $\frac{PG^3\,CA^2}{CB^4}$), and nine problems. The first problem is "Dati due gradi di meridiano e loro latidudine, trovare la ragione degl' assi, e gl' assi stessi;" the last is "Data la ragione de' due assi, che sia di 1 ad m, trovare nel meridiano un grado, che sia eguale al grado dell' equatore." It is an interesting piece of geometrical work.

In his § 490 Mr. Todhunter considers it curious that the (Cambridge) University library does not possess a complete copy of the famous work of Stay and Boscovich. His surprise will probably be increased when we state that, if we are not mistaken, neither do the libraries of the British Museum or that of the late Mr. Graves; at any rate, we do not remember to have met with Boscovich's commentary on the poem. "These writings furnish elementary accounts of the most important results which had been obtained up to their date, and reveal apparently great knowledge and judgment in Natural Philosophy." A copy of Boscovich's "Dissertatio de telluris figurâ, habita in seminario romano Soc. Jesu nunc primum aucta et illustrata ab ipsomet auctore, P. R. J. Boscovich," forms pp. 161-218 of vol. ii. of Giuliani's memoirs (cited at the foot, date 1744). In p. 184 he speaks of Maclaurin's "Fluxions" as "Newtono ipso dignissima;" there is a noteworthy passage, pp. 217, 218, and also a notice on p. xii.

* Continued from p. 380.

* It occupies pp. 225-266 of vol. iii. of the "Memoria sopra la Fisica e Istoria Naturale di diversi valentuomini," edited by Carl Antonio Giuliani, in 4 vols. (1743-1747).

of the introduction of the volume.* The date on the copy of Frisi's dissertation (not seen by Mr. Todhunter) we consulted was 1751; it contains 8 pp. of *antecessio;* 86 pp. of text, 3 plates of figures, and 2 pp. of dedication. It consists of ten chapters, and may possibly, as Mr. Todhunter suggests, have been incorporated by Frisi in his "Cosmographia" (§ 532), though it certainly was not transferred bodily; on p. 65 he gets "axis terræ ad diametrum sibi normalem erit ut 229 : 230."

We have seen one of the works alluded to in § 551, viz., that of Bouguer; it is entitled, "Opérations faites par ordre de l'Académie . . . pour la vérification du dégré du méridien compris entre Paris et Amiens." Paris, 1757. 8vo. It consists of 28 pp., including two for title, and was read March 23, 1757.

In § 717 reference is made to the account of a measurement of an arc of the meridian in Lombardy by Beccaria, published at Turin "in 1744." We know of no work by Beccaria on this subject, except the "Gradus Taurinensis." The two copies of this which we have seen are dated 1774 and 1775; possibly from its position in the book the former of these dates is intended by Mr. Todhunter. It is a scarce work.

Mr. Todhunter has in § 725 devoted more attention to a supposed work by Newton than perhaps it really deserves. At any rate, De Morgan ("Budget of Paradoxes," p. 83) greatly doubts that Newton wrote it. He remarks that it has been treated with singular silence, and the name of the editor has never been given.

Lagrange contributes a memoir, in which he arrives by analysis at the point Maclaurin had reached by geometrical methods.

The operations carried on at Schehallien for ascertaining the density of the earth are next noticed, and with the conclusion of vol. i. the history of the two subjects during the century which followed the appearance of the "Principia" is nearly completed.

Some works mentioned as not having been seen (in § 738) may here be described.

The title of D'Anville's book is incorrectly given by Lalande.† For "*la* circonférence," read *sa;* for "*de l'équateur*," read *sur les parallèles.* There are 8 pp. of dedication (to Duc de Chartres), 20 pp. of *avertissement*, 3 pp. of *privilége*, &c, 11 pp. of observations, and 147 pp. of text, with a plate.

Lalande describes very accurately the contents of the "Anecdotes physiques et morales."

Mayer's "Basis Palatina" has 6 pp. of dedication, 14 pp. *lectori astronomo;* 23 pp. are taken up with "Series et ordo triangulorum quæ ex propriis suis observationibus, anno 1763 habitis, deduxit et correxit C. M.; 2 pp. of *conspectus totius operis.*

Hennert's work (Utrecht, 1778) contains five dissertations, of which the fourth is—"Sur le mouvement que prend un corps, quand il est parvenu au centre d'attraction, et sur l'attraction considerée comme principe universel." It takes up pp. 125-166. On p. 166 he says—

"Concluons de nos recherches qu'on n'a pas assès de preuves pour admettre l'attraction comme principe universel de tous les changemens qui arrivent dans le monde matériel. Nous avons vu que la cohésion des corps ne se déduit pas sans difficulté de l'attraction. . . . . Je sens bien, qu'il reste d'autres recherches à faire sur cette matière. J'en ai ebauché quelques unes que je pourrai publier si elles me paroissoient pouvoir contribuer au progrés des sciences physico-mathèmatiques."

His fifth dissertation is "Sur la figure de la terre relativement à la parallaxe de la lune et à la navigation." It occupies pp. 167-214. In § 18 we read, "Après avoir cherché inutilement de concilier les observations avec les hypothèses, dont les astronomes ont fait usage jusqu'aujourdhui, tachons de tirer un meilleur parti de notre formule générale," &c. And in § 25, "Il résulte de nos calcules que l'hypothèse de M. Bouguer et la notre donnent plus exactement le degré de longitude que l'hypothèse elliptique qui s'écarte considérablement de l'expérience." There are 4 pp. of dedication, 11 pp. of preface (interesting and amusing), 3 pp. of contents, 214 pp. of text, 2 pp. of errata, and 3 plates.

We may here describe a volume containing "Dissertationes de uniformitate motus diurni," by Hennert and Frisius, "præmio coronatæ," Petropoli, October 10, 1783. That by Hennert contains 42 pp., and *additamenta* making up 70 pp. in all; that by Frisius goes on to p. 112, and then 40 pp. more. They treat of attraction, but are not, apparently, of much importance.

Frisius it was who first introduced the ellipsoid as distinguished from the oblatum and the oblongum; from § 669, we learn, also, that he has no hesitation in adopting the truth as to the earth's motion.

Hube's work is in 87 pp. 8vo., with 1 page of plates. Its value may be seen from the following extract § 43:—

"Demonstravimus itaque tellurem nostram omnino esse homogeneam, vel potius densitatem variam illius partium, ratione universæ massæ, nullius fere momenti esse . . . probavimus porro, pendulorum experimentis, differentiam gravitatis sub polo et æquatore, tantam esse, ut terræ forma elliptica omnino esse nequeat . . . ad finem itaque meum pervenisse mihi videor, qui tantum fuit, ut experimentorum hucusque factorum ope, summâ qua fieri potuit evidentiâ, telluris formam certo definirem ac omnia dubia, quæ in hac re jure moveri possent, tollerem; neque adeo immorabor in consectariis variis nostræ theoriæ explicandis, quoniam vera telluris forma semel certo stabilita, haud difficile est, eo colligere atque perspicere, quæ inde necessario efficiuntur."

Further particulars concerning Thomas Williams (§ 988) are given in the "Budget of Paradoxes," p. 102.

The work referred to in § 1,000, is a 4to. volume, and contains 1 page of *avertissement*, 13 pp. of introduction, 1 page of table, 94 pp. of text, and 1 page of errata (in this Houslowheat is corrected to Hounslowheat). We think we have somewhere seen the preface ascribed to Legendre; it is clear, however, from the introduction (from the words "deux de mes confrères de l'Académie M.M. Mechain et Legendre, p. xiv.) that it is due to Cassini, as Lalande correctly states. With the copy we examined was bound up "Descriptions des moyens employés pour mesurer la base de Hounslow Heath dans la province de Middlesex, publiée dans le vol. lxxv. des 'Trans. Phil.' par le Major-Général William Roy, traduite de l'Anglais par M. de Prony," Paris, 1787. There is a *discours préliminaire du traducteur* 18 pp. There is a notice of 1 page, a page of errata: the translation occupies 80 pp., there are 3 tables, and 5 planches. Mr

* We have also seen another work, the "De centro Gravitatis, editio altera, accedit disquisitio in centrum magnitudinis" (Rome, 1751), but it does not, if we remember, bear upon our subject.

† There is a very valuable copy of this work ("Bibliographie Astronomique") in the Graves' Library, containing Lalande's autograph notes, and many other interesting features.

Todhunter gives no account of this work: it is apparently a translation of the work described in § 984.

The following work by Delambre and Legendre (will this throw light on § 1,146?) does not seem to be described: it is "Methodes analytiques pour la détermination d'un arc du méridien précédées d'un memoire sur le même sujet," par A. M. Legendre. Paris, 1799. 4to.) In note ii. to "Methode pour determiner la longueur exacte du quart du meridien," par A. M. L. occurs *Legendre's Theorem*. There are 176 pp., 16 pp. of tables at the end, 5 pp. of "Observations sur quelques endroits du mémoire du cit. Delambre," par A. M. Legendre; 2 plates, 7 pp. of *avertissement*, and 4 pp. of contents.

We now close our notes with a few remarks on §§ 531, 682, and 1,584. The memoir, which neither Playfair nor Mr. Todhunter have been able to procure a sight of, is to be met with in vol. i. of the Giuliani memoirs cited above, consequently its date is antecedent to 1743. It occupies pp. 65-88, and is entitled "Problema mecanicum de solido maximæ attractionis solutum, a P. Rogerio Josepho Boscovich." The problem is, "Data quantitate materiæ punctum attrahentis, in quacunque lege distantiarum invenire solidum ipsum continens, quod maximè omnium attrahat ipsum punctum positum in axe solidi producto ad datam distantiam ob ipsius solido vertice propiore." The author gives a geometrical and an analytical solution of the problem, and concludes, "Solutio geometrica in eo huic posteriori præstat quod ibi determinatur solidum maximæ attractionis etiam inter solida omnia irregularia, hic tantum inter solida genita rotatione curvæ circa axem."

The greater part of this second volume is taken up with the important writings of Laplace, Poisson, Gauss, Ivory, and Plana.

In the case of Laplace, who was, like some other writers, not in the habit of acknowledging his indebtedness to preceding authors, the result of the investigations is to restore his reputation to its proper eminence. "In the present history, and in that of Probability, I have gone over a third part of the collected mathematical works of Laplace; and to that extent the evidence of his great powers and achievements is, I hope, fully and fairly manifested."

Our work is now nearly done; were we to make use of all our notes, we might easily double and treble what we have written.* We have noted upwards of fifty articles which are interesting as sidelights: thus, § 227, Bouguer's remarks on what we now call the "personal equation;" § 710, transformation of variables in a triple integral, and many others. The value of the index would have been enhanced had reference been made to these; as it is, the index is much fuller than in the earlier volumes, and we have detected but few errors. This is no new feature, for all Mr. Todhunter's books are most carefully got up in this respect, and we have not met with a single important error in the mathematical work; the few mistakes we have come across are easily corrected.

There are indications here and there throughout the volumes that, should the writer be able to secure the requisite leisure, he will not want for subjects to exercise his special gifts upon. We sincerely hope that he will do so at an early date, and that it may be our lot to read the results. In the course of the preparation of the present work Mr. Todhunter has published, in the Royal and other Societies' proceedings, various papers which have grown out of his investigations in the history of our two subjects.*

R. TUCKER

* In our remarks we have preferred to treat the subjects from an historical rather than from a mathematical point of view.

* Possibly some further memoirs of use might be found in the "Librorum in Bibliothecâ Speculæ Pulcovensis, anno 1858, exeunte contentorum catalogus systematicus," by Otto Struve. Petropoli, 1860.

TRAINING

Training in Theory and Practice. By Archibald Maclaren. Second Edition. (Macmillan & Co., 1874.)

WHEREVER we go, "The Boat Race" is the topic of the hour. Opinions are freely expressed as to the relative merits of the rival crews; and the risks and dangers incurred during the process of training, sturdily insisted on by some, are as obstinately denied by others. The respective values of the slow and quick stroke—the American fashion of "sliding-seats," and a variety of kindred questions, are eagerly debated, occasionally by men who really understand what they are talking about, but more frequently in order to make conversation, or, as the phrase goes, "for the sake of something to say."

In such a state of the public mind, it has by a happy chance been so ordered that the second edition of Mr. Maclaren's well-known work on Training should appear, at a moment when all will take unusual interest in its contents.

From a perusal of this treatise everyone may gather information calculated to be of service to him, not only for the time being but for the future. The man who likes to know a little of everthing, may with advantage indulge his curiosity about a subject which has perhaps hitherto been out of his line; while the father with a son in "the eight," and whose mind has been disturbed by letters in the papers, hinting at dire mischief as the outcome of training, may find substantial comfort in its pages. All those who, either from necessity or for amusement, undergo much physical exertion, will do well to imbue themselves with the author's teaching; and the "man of ease" or of sedentary occupation will find hints for health or sanitary lessons of a description which, if honestly carried out, would cause the ruin of half the doctors in London.

Mr. Maclaren uses "rowing" as the peg on which to hang his theme, and the reason he gives for so doing is as follows:—

"It is the exercise most susceptible of being influenced by a judicious system of bodily preparation, being at once an act of considerable intricacy, demanding long and assiduous practice, and an exercise of considerable difficulty, involving the possession, although not in an equal degree, of both muscular and respiratory power, to promote which is the object of all training."

The question, what is training? and what is it meant to do? he answers thus:—"It is, to put the body with extreme and exceptional care under the influence of all the agents which promote its health and strength, in order to enable it to meet the extreme and exceptional demands upon its energies." There can be no doubt that the essence of this paragraph is contained in the words "extreme and exceptional care;"—for without such care, instances are furnished to alarmists of men "fainting by the way," and a highly valuable art is in danger of being brought into disrepute. We have no fear for the men engaged in a great race like that which takes place on Saturday—they have been carefully instructed in the proper method of training—all that experience has taught has been lavished on them, and the result is, as Dr. Morgan's statistics show, that they live as long as other portions of the civil community.

But on the other hand we do fear for the many persons who assert that they are in training for some race or other physical feat, but from whom on inquiry we learn that their notions and practice are so desultory, and so deficient in anything like scientific detail, that it is an abuse of the word training to apply it to their misdirected proceedings. They forget, or at least do not sufficiently consider, that the feat they are about to attempt will require of them, at some critical moment, a supreme effort; and that in the making of this effort, a lasting injury may be inflicted upon a frame that is only imperfectly prepared.

To them we say, "if a thing is worth doing at all it is worth doing well." Train thoroughly or not at all—you have no right to jeopardise your future by "extreme carelessness" when all bad results may be avoided by "extreme care."

In former times Diet was looked upon as of paramount importance among the agents of health in training; Mr. Maclaren, however, places exercises in the front rank, and justifies himself for doing so in the following logical and telling words:—"So long as men believe that the qualities which they covet are to be obtained from mere dietary regulations, they will neglect the real agencies which can alone bestow them; exercise, the one agent which gives, which can give, these qualities, both from its own nature and from the influence which it exerts upon all other agents of health, is, in a measure, neglected, nay, avoided, and to the imaginary virtues of diet men look for the longed-for acquisitions; they have yet to learn, they have yet to know, and to themselves realise, that power of muscle in trunk and limb, that freedom and capacity of heart and lung, that energy, stamina, strength and endurance, are not to be obtained from what they *eat*, but from what they *do*."

To each of these subjects, Exercise and Diet, a separate chapter is devoted; and each is treated in a masterly and exhaustive manner. Our space will not, however, permit of more than an allusion to them. Mr. Maclaren deprecates the error of confining the attention to any one form of exercise, on the ground that it must be insufficient to produce the desired result, that is to say, the increased action, and thereby the fuller development, of all the muscles of the body. He illustrates his meaning by the example of the man who, having one favourite author or favourite object of study, fails to cultivate or employ his *whole* mind. On the subject of Diet he is much less stringent and exclusive than is customary, and we think rightly so, recognising the fact that each one must be a law to himself; whence the truth of the trite saying, "What is one man's meat is another man's poison." His views on this point may be summed up in his own words: "I would only advocate the rational system of not suddenly breaking in upon a man's fixed habits, at the time you are asking for an effective display of his greatest bodily energies."

Another moot point, the question of the amount of sleep required by different persons under the same circumstances, is next discussed. No hard and fast rule is laid down, but the breadth of view that is one of the great charms of the book is again apparent. The opinion is expressed that the time to be devoted to the purpose should vary not only with the individual, but with the same individual at successive periods of life, and that the wants of the system, in this respect, are influenced by various causes, and by the action of the other agents of health, especially by exercise. With reference to this matter, we have frequently observed that tall persons require a longer period of recumbency than short, whose hearts are called upon for less powerful exertion, by reason of the smaller height of the column of blood that they have to sustain and propel.

The chapters next in order deal with the important but still subsidiary questions of air, bathing, and clothing. We cannot at present enter into detail with regard to any of these subjects, but we would add to Mr. Maclaren's observations on bathing, that one great secret of using a cold bath in the dressing-room without discomfort or injury, is to sit down in the water in the first instance, and to wash the upper part of the body, thus somewhat raising the temperature of the water before the feet are immersed.

The concluding portion of the book is devoted to very clear and practical directions for self-training for aquatic purposes, and in this, as in all other parts, it will be found a complete and trustworthy guide by those for whose use it is intended.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Herbert Spencer versus Thomson and Tait

A FRIEND has lent me a copy of a pamphlet recently published by Mr. Herbert Spencer, in which certain statements of mine are most unsparingly dealt with, especially in the way of attempted contrast with others made by Sir W. Thomson and myself. I am too busy at the present season to do more than request you to reprint one of the passages objected to (leaving it to your readers to divine to what possible objections it is open), and to illustrate by a brief record of my college days something closely akin to the mental attitude of the objector.

"Natural philosophy is an experimental, and not an intuitive science. No *à priori* reasoning can conduct us demonstratively to a single physical truth" (Tait, *Thermodynamics*, § 1).

One of my most intimate friends in Cambridge, who had been an ardent disciple of the late Sir W. Hamilton, Bart., and had adopted the preposterous notions about mathematics inculcated by that master, was consequently in great danger of being plucked. His college tutor took much interest in him, and for a long time gave him private instruction in elementary algebra in addition to the college lectures. After hard labour on the part of each, some success seemed to have been obtained, as my friend had at last for once been enabled to follow the steps of the solution of a question involving a simple equation. A flush of joy mantled his cheek, he felt his degree assured, and he warmly thanked his devoted instructor. Alas, this happy phase had but a brief duration; my friend's early mental bias too soon recovered its sway, and he cried in an agony of doubt and despair, "But what if x should turn out, after all, *not* to be the unknown quantity?"

Compare this with the following extract from Mr. Spencer's pamphlet:—

"... if I examine the nature of this proposition that '*the* properties of matter *might have been*' other than they are. Does it express an experimentally-ascertained truth? If so, I invite Prof. Tait to describe the experiments!" P. G. TAIT

Animal Locomotion

MY former letter on this subject was merely to show that, mechanically, Dr. Pettigrew's view of the forward motion or inclination of a bird's wing during the down stroke was less absurd than had been supposed, and even seemed necessary to flight. I did not profess to have made accurate observation or experiment on the point. I accept, therefore, the observation of the Duke of Argyll as to the vertical motion of the heron's wing; but as he expressly refers to its great concavity, that would give a vertical down stroke the effect of a somewhat forward stroke of a flatter wing. The proper inference would therefore seem to be, that in birds with less concave wings the stroke is slightly directed forwards. As to the last two paragraphs of his Grace's letter, he will see, if he refers again to mine, that he has quoted words I never used. I impute to Dr. Pettigrew the "merit of showing" that the "*slight upward angle* of the mean position of the wing plane is essential to secure horizontal forward motion *as* a general resultant," &c., and this is exactly what the Duke denies.

Mr. James Ward's elaborate analysis of the down stroke of a bird's wing simply shows (if correct) that in the position he ascribes to it (moving downward and backward) it would send the bird horizontally forward. Of course it would. But then what becomes of the bird during the up stroke in an opposite direction? The bird is then falling, and by the downward reaction of all the solid surface of the anterior margin of the wing, and of all the feathers, however obliquely turned, it is driven farther downwards; and as this takes place between every two down strokes, and approximately during an equal space of time, how is a horizontal average motion to be produced unless the down stroke alone produces, not a horizontal, but a highly-inclined upward motion? Mr. Ward's whole argument appears to me to ignore the great downward reaction, added to gravitation, during every up stroke, which requires that the down stroke should not merely support the bird, but raise it up vertically just as much as during the up stroke it has fallen vertically. The matter, however, is not to be settled by discussing theoretically, but by observation and experiment. I simply maintain that the results of Dr. Pettigrew's observations and experiments are not, as supposed, inconsistent with mechanical principles; and nothing in your correspondent's letter induces me to alter that opinion.

ALFRED R. WALLACE

The Newfoundland Cuttle-Fish (*Megaloteuthis harveyi* S. Kent)

MY right being questioned, through an anonymous paragraph in the *Globe* of the 11th inst., to institute a new generic title for the gigantic Cephalopod encountered off Newfoundland, and of which I communicated an account to the Zoological Society's meeting of March 3, I would briefly reply to my criticiser in these columns.

In the first place, it is a somewhat anomalous proceeding to raise objections on such a question before details of the grounds upon which it has been deemed advisable to establish such a title have appeared, as in the ordinary course of events they will, in the "Proceedings" of the Society. In these it will be found that ample reasons are given for the course that has been taken, as also due notice of both Prof. Steenstrup's and Prof. Verrill's researches in a similar direction. Had my assailant placed himself more thoroughly *au courant* with the details of the case, he would possibly have held back his emphatic assertion that Prof. Verrill had "actually identified the species from Newfoundland with those described by Steenstrup as belonging to his genus *Architeuthis*." This identification in Prof. Verrill's own language is entirely problematical, and must unfortunately remain so, since a beak only, an organ of no value in generic discrimination, has been preserved of the typical species *A. dux*. Respecting the second form, *A. monachus*, we have still less knowledge, the title being provisionally instituted by Prof. Steenstrup for the reception of two gigantic Cephalopods cast on the shores of Jutland in the years 1639 and 1790, and of which popular record alone remains.

In reference to the "imperfect evidence" asserted by my critic to be at my command, I may state that I received accounts of the examples, upon which I have proposed to base my new title of *Megaloteuthis*, direct from America as long ago as in the beginning of December last, supplemented by numerous fuller details since.

W. SAVILLE-KENT

Lord Lindsay's Expedition

THE expedition of Lord Lindsay for observation of the Transit of Venus at Mauritius (why will people still call it *the* Mauritius?) will afford a good opportunity for re-measuring the base line of Abbé de la Caille, made in 1753, and which, to the best of my belief, has never been since verified.

The small conical cairns which mark its extremities should still be found *in situ*. I saw one only of them in November 1864, when I had not time to search for the other. The base measured was 1,828 toises in length, and, I imagine, on the meridian. It was on a level plain at the south-west extremity of the island, close under the western slopes of the precipitous and noble "Morne du Brabant," which rises nearly 1,700 feet above the sea-level. By road the distance of this spot from Port Louis must be at least 30 miles, but it is much more easily reached direct by boat; or, as December is a bad time of year for boating outside the reefs, the best route would be from Black River by water inside the isle Bénitier. It is a glorious district, all that part of the island, and contains the finest scenery, including the Chamaral Falls.

S. P. OLIVER

Buncrana, near Londonderry, March 14

QUETELET

ON February 17 last Jacques-Adolphe-Lambert Quetelet died at Brussels, in the seventy-eighth year of his age, having been born on February 22, 1796, at Ghent. At the early age of 18 he was appointed Professor of Mathematics in the College of his native town. In July, 1819, the degree of Doctor of Science was conferred on him by the University of Ghent, then recently founded by King William. His dissertation on this occasion was so well received that he was shortly thereafter appointed to the Chair of Mathematics at the Royal Athenæum of Brussels; and in February following was elected a member of the Academy of Sciences and Belles-Lettres.

At this time he applied himself with ardour to the cultivation of literature and pure mathematics, thus laying a sure foundation for the world-wide fame he afterwards achieved as an exact investigator in many departments of physics, as an original thinker in applying methods of scientific treatment to the discussion of problems previously considered as belonging exclusively to moralists and divines, and as a clear and eloquent expounder of the truths he had demonstrated. The many-sidedness and fertility of his mind may be seen from his scientific memoirs enumerated in the Royal Society's Catalogue of Scientific papers, amounting at the close of 1863 to 220. He continued to write almost to the last, notwithstanding the mental malady, consisting in loss of memory, with which he was afflicted many years before his death, and it is noteworthy that even to the last his handwriting retained much of the rare grace and elegance for which it had been so remarkable.

The earliest of Quetelet's published memoirs, begun in 1820, were on geometrical subjects. The non-appreciation of these by the public determined him to devote himself to physical science and astronomy. On these subjects he lectured publicly with great success.

In 1823 he was sent on a mission to Paris with the view of preparing a report on the observatory of that city for the guidance of the Belgian Government in founding a similar observatory at Brussels. After some delay, the observatory was established, with Quetelet as director, and in 1833 began the long series of observations on astronomy, meteorology, and other physical inquiries, for which this observatory is so well known. The most important of his astronomical observations was the preparation of a catalogue, begun in 1857, of stars which seem to have appreciable motion. He also began, so early as 1836, systematically to observe and record the occurrence of meteors and shooting-stars. These observations came to be of great value thirty years later, when the true nature of these bodies was satisfactorily established.

The meteorological observations at this observatory have been particularly full and valuable, embracing hourly and bi-hourly observations, published annually *in extenso*, of atmospheric pressure, temperature, humidity, rain, cloud, &c. These have been exhaustively discussed by Quetelet in "La météorologie de la Belgique comparée à celle du globe," published in 1867. In this admirable treatise we have what must still be regarded as the fullest and best account of the meteorology of any single locality on the globe—the yearly, monthly, daily, and hourly march of the various meteorological elements being given. In the same volume are given *résumés* of the observations made at the other stations which began to be established at Alost, Ghent, Liége, &c., in 1835.

He was elected perpetual secretary of the Academy of Sciences and Belles-Lettres in November 1834, and was chiefly instrumental in adding a section on the Fine Arts in 1845. It is scarcely necessary to refer to the scientific contributions he made to the Fine Arts, by his extensive and minute investigations regarding the proportions of the human body, the results of which are given in his "Anthropométrie." In matters relating to the higher education, to the census, and other national questions, the Belgian Government wisely availed itself repeatedly of his wide knowledge and great experience.

His first paper on the subject of statistics was published in 1826; in 1835 appeared his "Physique sociale," and ten years later his "Lettres sur la théorie des probabilités appliquées aux sciences morales et politiques." In 1841 a Central Commission of Statistics was established by royal decree, of which Quetelet was made president, and of which he continued to be president to his death. He originated the idea of convening an International Congress of Statistics. The first was held in Brussels in 1853, and others have since been held at Paris, London, Berlin, Florence, the Hague, and St. Petersburg. It is in the field of statistics that Quetelet appears as a great discoverer, and his success in this department must be attributed to the clearness with which he saw that statistics occupy the ground in the development of the social and political sciences which observational data do in the development of such sciences as astronomy and meteorology, to the patient industry with which through long years he gathered together his facts, and to the mathematical skill he brought to bear on the discussion of the results. He was truly, as expressed by the Academy of Berlin in their congratulatory letter on the occasion of the centenary of the Belgian Academy, "the founder of a new science which proceeds from the firm basis of observation and calculation to discover and unfold those immutable laws which govern the phenomena, apparently the most accidental, of the life of man, down even to his most trivial actions."

SCIENTIFIC RESULTS OF THE "POLARIS" ARCTIC EXPEDITION

WE have received advanced sheets of the Report of the Secretary of the United States Navy, of the examination of those of the crew of the *Polaris* who were in the ship when she broke loose from the floe to which she was anchored, on October 15, 1872, leaving the nineteen persons on the sheet of ice which was their floating home, until picked up about six months after off the coast of Labrador (NATURE, vol. viii., p. 217). This report confirms the opinion we have already expressed that no Arctic expedition can be adequately conducted unless carried out under naval discipline. It was only on account of the good intentions and good nature of the crew, especially after their noble and enthusiastic captain's death, that things went on as smoothly as they did. Captain Buddington seems to have had no heart in the object of the expedition, and we cannot help thinking that had he not been with it much more would have been gained. It was in deference to his opinion that Captain Hall refrained from trying to push beyond his furthest point (82° 16′ N.) with the ship; all the other officers, though they do not seem to have been very well assorted, being of opinion that an attempt should be made to get further north, or at least not to lose ground by wintering further south.

We have already (vol. viii., p. 435) given details as to the rescue of those who were left in the *Polaris*, and of their being landed in Scotland by the *Arctic* and *Eric* whalers. The present report affords some idea of the scientific results of the expedition, a detailed account of which will no doubt by and by be published, although we regret to see that many of the records of the scientific results were lost in the confusion incident to the parting of the ship from the floe. Still much that is valuable has been brought home, from which many additions to a scientific knowledge of that part of the Arctic region will be obtained. Notwithstanding the want of perfect harmony among the officers, the scientific work of the expedition seems to have been diligently carried on, and the evidence of Dr. Bessels especially contains a great deal of value to Science. Geographers will be able to correct and extend their maps of the regions visited, and we hope that very soon the complete material for enabling them to do so will be in their hands. Constant and careful tidal observations were carried on, with the very valuable result of ascertaining that the tide of Thank-God Harbour, 81° 38′ N. is not produced by the Atlantic but by the Pacific tidal wave. "It was found," Dr. Bessels says, "that the co-tidal hour is about $16^h\ 20^m$. Rensselaer Harbour, being the northermost station, has its co-tidal hour at $18^h\ 04^m$, consequently the tide comes from the north, the rise and fall at spring-tides amounting to about 5 ft.; at neap tides $2\frac{3}{10}$. Most likely the two tidal waves meet somewhere in Smith Sound, near Cape Frazier. Kane and Hayes have both found a ridge of hummocks near Cape Frazier, and in drifting down we experienced that during some time, being abreast of Cape Frazier; we hardly made any headway, but we drifted both north and south."

The results of the expedition may be summed up briefly as follows:—(1) the *Polaris* reached 82° 16′ N., a higher latitude than has been attained by any other ship; (2) the navigability of Kennedy Channel has been proved beyond a doubt; (3) upwards of 700 miles of coast-line have been discovered and surveyed; (4) the insularity of Greenland has been proven; and (5) numerous observations have been made relating to astronomy, magnetism, force of gravity, ocean physics, meteorology, zoology, ethnology, botany, and geology, the records of which were kept in accordance with the instructions supplied by the National Academy, and some of the results of which we propose briefly to enumerate.

ASTRONOMY.—Great care was taken in determining a reliable meridian at Thank-God Harbour. Soon after entering winter-quarters an observatory was erected on the shore, thirty-four feet above mean sea-level, and the transit instrument stationed there. The longitude of this station was determined by the observation of 300 lunar distances; a number of moon culminations; a great number of star transits; a number of star occultations; a great number of altitudes of the sun on or near the prime vertical. Its latitude, by the observation of a great number of circummeridian altitudes of the sun, and a number of altitudes of stars. All of these observations were lost, but a number of the results have been preserved which are sufficient to establish the position of his station.

The instruments used in the above observations were a Würdemann transit and Gambey sextants divided to 10″. The expedition carried six box chronometers made by Negus, three of which indicated sidereal time, and four pocket chronometers by different English makers. These time-pieces were compared every day at precisely the same time, and the result entered in the chronometer-journal.

Besides the above-mentioned observations, twenty sets of pendulum experiments were made, which are saved, but the observations for time belonging to them are lost.

MAGNETISM.—The magnetic observations obtained were more complete than any others ever before made in the Arctic regions. The instruments supplied were:—one unifilar declinometer; one dip circle, with Lloyd's needles; one theodolite; and several prismatic compasses.

The observations on variation of declination were registered at Göttingen time, and were continued for five months: readings taken hourly. Besides that, three term days were observed every month, according to the Göttingen regulations, one of these term days corresponding with the day accepted by all the magnetic stations. Further, a number of observations were taken either with the theodolite or the prismatic compass. Whenever possible, the dip was observed, and several sets of observations on relative and absolute intensity and of the moment of inertia were obtained.

OCEAN PHYSICS.—Unfortunately there was not much opportunity for taking soundings. About twelve were obtained along the coast of Grinnell Land, which prove that the hundred-fathom line follows the coast at a distance of about 15 miles in Smith's Sound. One of these soundings (90 fathoms) proved highly interesting, containing an organism of lower type than the *Bathybius* discovered by the English dredging expedition. It was named *Protobathybius robesonii*.

A number of deep-sea temperatures were taken with corresponding observations on the density of the water. Following the coast of West Greenland the limits of the Gulf Stream were ascertained. Specimens of water from different depths were preserved in bottles, but were, unfortunately, lost.

As soon as the vessel was fairly frozen in, a tide-gauge was erected over a square hole cut in the ice-floe, and kept open continually; the pulley and rope were supported by a tripod of oars. A rope, to which a wooden scale, divided into feet and inches, was fastened, was carried through a block attached to the tripod. One end of the rope was anchored at the bottom by means of two thirty-two pound shot, and a counterpoise was attached to the other end to keep the rope properly stretched. This apparatus was tested by a series of scale readings with corresponding soundings, and proved to work very satisfactorily. The observations comprise eight lunations, the readings being taken hourly, half-hourly, and in some instances every ten minutes, in order to determine the precise moment of the turn of the tide.

METEOROLOGY.—After having entered winter-quarters meteorological observations, which up to this time had been made three-hourly, were made every hour, Washington time. The register contained observations on the temperature of the air, atmospheric pressure, psychrometrical observation, direction and force of wind, appearance of the sky, state of weather, and both solar and terrestrial radiation. Besides, all extraordinary meteorological phenomena were carefully noted.

For the registration of the temperature of the air mercurial thermometers were used for temperatures down to $-35°$ F.; for lower ranges spirit instruments being compared at intervals of 10°. As circumstances would permit, mercurial or aneroid barometers were used. As it was not supposed that psychrometrical observations could be favourably conducted at very low temperatures, the expedition was not supplied with the suitable instruments. For that reason two uncoloured spirit thermometers were selected and used, the readings of which agreed. As check observations the dew-point was determined by means of Regnault's apparatus. To measure the velocity of the wind, Robinson's anemometer usually served. The distance travelled by the wind was noted hourly, at the same intervals of time. The velocity of the wind was determined either by the same instrument or by means of Casella's current-meter. These observations on the winds, combined with those on moisture of the atmosphere, will form a valuable contribution to physical geography.

It was not thought essential to procure photographs of the clouds, as they do not differ in their general character from those in more southerly latitudes. The only remarkable fact to be noticed is that sometimes cirri could be observed at very low altitudes among stratus clouds, which, however, is not surprising if their mode of formation is taken into account.

Special attention was devoted to the aurora borealis, which occurred frequently, but rarely showed brilliant colours, never bright enough to produce a spectrum. Whenever necessary one observer was stationed at the magnetometer and the other out-doors, the former observing the motions of the magnets, while the other was watching the changes in the phenomenon and taking sketches. Although an electroscope and electrometer were set up, and the electrical condition of the atmosphere frequently tested, in no instance could the least amount of electricity be detected. The amount of precipitation was measured as carefully as the violent gales would permit, by means of a rain-gauge supplied with a funnel. In February, as soon as the sun re-appeared, observations on solar radiation were commenced, and continued throughout the entire summer. The instruments employed were a common black-bulb thermometer, and one *in vacuo*; both exposed on white cotton.

ZOOLOGY AND BOTANY.—The collections of natural history are almost entirely lost. With the exception of two small cases containing animals, minerals, and one package of plants, nothing could be rescued. The character of the fauna is North American, as indicated by the occurrence of the lemming and the musk ox. Nine species of mammals were found, four of which are seals. The birds are represented by twenty-one species. The number of species of insects is about fifteen, viz.: one beetle, four butterflies, six diptera, one bumble-bee, and several ichneumons, parasites in caterpillars. Further, two species of spiders and several mites were found. The animals of lower grade are not ready yet for examination.

The flora is richer than could be expected, as not less than seventeen phanerogamic plants were collected, besides three mosses, three lichens, and five fresh-water algæ.

GEOLOGY.—Although the formation of the Upper Silurian limestone, which seems to constitute the whole west coast north of Humboldt Glacier, is very uniform, some highly interesting and important observations have been made. It was found that the land is rising, as indicated, for instance, by the occurrence of marine animals in a fresh-water lake more than 30 feet above the sea-level and far out of reach of the spring-tides. Wherever the locality was favourable the land is covered by drift, sometimes containing very characteristic lithological specimens, the identification of which with rocks in South Greenland was a very easily accomplished task. For instance, garnets of unusually large size were found in latitude 81° 30′, having marked mineralogical characteristics by which the identity with some garnets from Fiskernaes was established. Drawing a conclusion from such observations it became evident that the main line of the drift, indicating the direction of its motion, runs from south to north.

THE COMMON FROG*

XII.

SO much for the circulation of the frog in its adult condition. Its larval, or tadpole stage, presents us with a series of changes which, though more familiar, are not less wonderful.

In the first place, however, it may be well to describe shortly the condition of the circulation in fishes, where the purification of the blood is effected, not by means of the exposure of the blood to the action of air taken into respiratory cavities of the body, but by its subjection in little plates of membrane, the gills, to the influence of air mechanically mixed up with and dissolved in the water in which those gills are bathed.

In fishes, moreover, unlike all air-breathing animals, none of the oxygenated blood is returned to the heart for propulsion, but is collected directly into the great dorsal aorta, whence it is distributed to the whole body, only being returned to the heart after such distribution, so that venous blood alone enters that organ.

This venous blood is sent out from the heart through a bulbous aorta, whence arise on each side a series of arteries which ascend the branchial arches, one on the outer side of each such arch, decreasing in size as it ascends.

Each branchial artery gives off small gill arteries, which run along one edge of each little membranous leaflet or gill, and supply it with minute branches ending in capillaries, in which the blood is purified. There the purified blood is taken up by minute veins which open into gill veins, one of which runs along the opposite edge of each gill to that occupied by the gill artery.

The gill veins pour their contents into branchial veins, one of which ascends the outer side of each branchial arch, increasing in size as it ascends. The branchial veins open into the great dorsal aorta, whence the blood is distributed over the body. Generally the branchial arteries are only connected with the branchial veins by the intervention of the capillary vessels of the gills. Sometimes, however (as *e.g.* in the mud-fish, *Lepidosiren*), the branchial veins are directly continuous with the branchial arteries.

In the tadpole, while the gills remain fully developed, a condition exists quite similar to that of fishes. Minute vessels, however, directly connect together, at the root of each gill, the branchial artery and branchial vein of each gill. Such a connecting vessel is termed a *ductus botalli.*

A minute vessel given off from the third branchial artery, is the incipient pulmonary artery.

As development proceeds, as the gills diminish by absorption, and as their respective arteries and veins decrease in size and importance, each *ductus botalli* increases until at last we have established the six great continuous vessels of the adult frog.

We have, then, in the life-history of the frog, a complete transition from the condition of the fish to that of a true air-breathing vertebrate, as regards its circulation. The various conditions herein referred to have, however, an important bearing on the question of the first origin of such structure.

All higher animals, even the very highest, have the great arteries, when they first appear, arranged substantially as in fishes.

From the common aortic bulb five vessels ascend each side of the neck, and more or fewer of these arteries abound in different classes, the permanent adult condition being arrived at by this circuitous route.

This argument has commonly been adduced as an argument in favour of the descent of air-breathing animals from more ancient gill-bearing forms, and it is not without weight.

Nevertheless it must be borne in mind that the primitive condition in Fishes is that of direct continuity between the branchial arteries and veins such as we have seen exists permanently in *Lepidosiren*. It is only as development proceeds that each primitive continuous arch

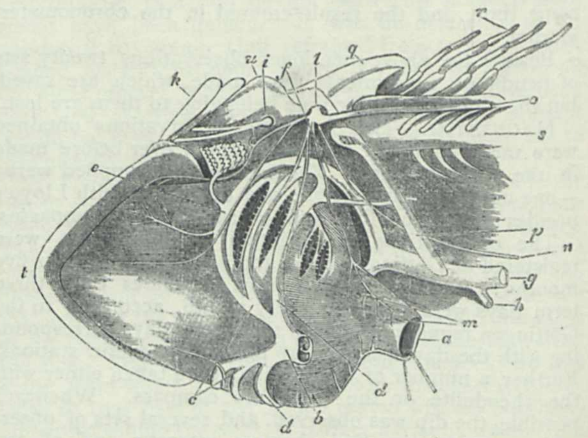

FIG. 78.—Infero-lateral view of Head and Aortic Arches of Lepidosiren (after Hyrtl). *a*, œsophagus; *b*, anterior end of bulbus aortæ; *c*, common roots of the first aortic arches; *d*, third aortic arch; *e*, first aortic arch; *f*, dorsal union of the first three aortic arches; *g*, aorta; *h*, cœliac artery; *i*, exit of the fifth nerve; *k*, part of operculum; *l*, exit of the nervus vagus from the skull; *m*, branches to œsophagus; *n*, nerve going to the rectus abdominis; *o*, nervus lateralis; *p*, first and hypertrophied rib; *q*, posterior part of the skull; *r*, segmented neural spines; *s*, chorda dorsalis; *t*, mandible; *u*, quadrate.

becomes broken up into an artery and a vein connected by a net-work of capillaries.

Now we can understand the series of unbroken arches in higher animals as the relics of ancestral vessels which divided for gill circulation and were therefore once of extreme functional importance and utility. But how can we understand the primitive unbroken series of arches in Fishes? Their utility was yet to come!

The frog when adult has, besides its skin, no breathing organs but the lungs. As has been said before, other members of the Frog's class retain gills and aquatic respiration during the whole of life, as for example *Menobranchus.*

Every one kind, however, whether provided permanently with gills or not, develops lungs, and it might easily be imagined that similarly every gilled-creature which has lungs is also a Batrachian.

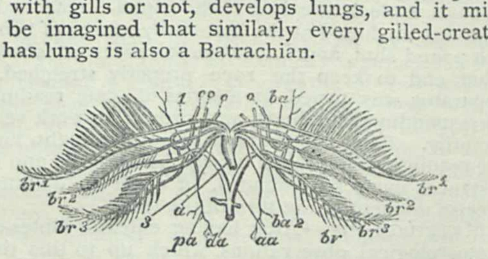

FIG. 79.—The Circulation of a Tadpole in its primitive stage, when nearly all the blood is distributed to the gills; the pulmonary arteries being quite rudimentary, and the vessel (or ductus botalli) connecting together the branchial artery and vein at the root of each gill being minute. *a*, bulbus aortæ; *b*, branchial arteries; br^1, br^2, $br3$, the three gills (or branchiæ of each side); *bv*, the branchial veins which bring back the blood from the gills—the hindermost pair of branchial veins on each side unite to form an aortic arch (*aa*), which again unites with its fellow of the opposite side to form *da*, the descending (or dorsal) aorta. The branchial veins of the foremost gills give rise to the carotid arteries, *cc*. *o*, artery going to the orbit; *pa*, pulmonary artery; 1, 2, 3, anastomosing branches connecting together the adjacent branchial arteries and veins.

This, however, would be a mistake.

The Mud-fish or *Lepidosiren*, already referred to more than once, is furnished with both gills and lungs throughout the whole of life. On this account it has been reckoned by some naturalists to be a Fish and not a Batrachian. Its fish-nature, however, has now been tho-

* Continued from p. 369

roughly established, and thus the probability of the existence of lungs within the class of fishes is also established.

But what is a lung?

A lung is a sac-like structure capable of being distended with air, supplied with venous blood direct from the heart and sending arterial blood directly to it. Generally the whole of the blood from the lungs goes back to the heart directly, but in one Batrachian—the celebrated *Proteus*—a portion of the blood from the lungs finds its way not

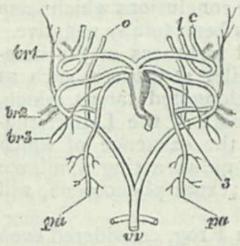

FIG. 80.

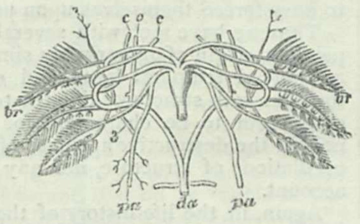

FIG. 81.

FIG. 80.—The Circulation in a Tadpole at a more advanced stage, when the gills have begun to be absorbed, the pulmonary arteries to increase, as also the connecting branches (at the root of the gills) between the branchial arteries and branchial veins.

FIG. 81.—The Circulation in a young Frog. Here the gills have been absorbed, and the blood passes directly from the heart to the head, the dorsal aorta, the lungs, and the skin,

into the heart but into vessels of the general circulation. When there is an air-sac which does not both receive blood directly from and return it directly to the heart—*i.e.* when there is no true *pulmonary* circulation—such an air-sac (whether single or double) is termed a *swim-bladder* and a structure of the kind is found in very many fishes. The swim-bladder of ordinary fishes neither receives blood directly from the heart by an artery like the pulmonary artery of higher animals, nor does it return blood directly to the heart.

The transition, however, from a lung to a swim-bladder is a graduated one. We have just seen that in *Proteus*,

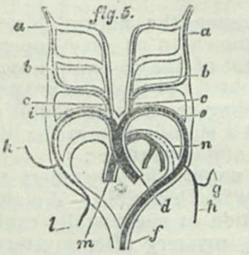

FIG. 82.

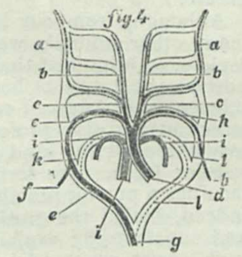

FIG. 83.

FIG. 82.—Diagram representing the main arteries of a Bird (fowl) with the changes induced on the primitive condition (after H. Rathke). *a, a*, internal carotids; *b, b*, external carotids; *c, c*, common carotids; *d*, root of main aortic arch (here right); *e*, arch of the same; *f*, right subclavian (which arises from the anastomosis of the first two right primitive aortic arches); *g*, commencement of the descending aorta; *h, h*, left subclavian; *i, i, i*, pulmonary arteries; *k*, right, and *l*, left, rudiments of the primitive aortic arches.

FIG. 83.—Diagram representing the main arteries of a Mammal with the changes induced in the primitive condition (after H. Rathke). *a, b, c*, carotids, as before; *d*, root of main aortic arch (here left); *e*, arch of the same; *f*, commencement of descending aorta; *g*, left vertebral artery; *h*, left subclavian; *i*, right subclavian; *k*, right vertebral artery; *l*, continuation of right subclavian; *m*, pulmonary artery; *n*, remnant of left primitive aortic arch.

though blood is returned from the lungs direct to the heart, yet that not all the blood is so returned. On the other hand in another animal, *Ceratodus*, though blood is not brought to its air-sac directly (which is therefore a swim-bladder and not a lung), yet for all that blood is sent *from* it direct to the heart.

Ceratodus (or as it is locally called "flat-head") is a fish of Queensland, closely allied to *Lepiaosiren*, and is a very noteworthy animal apart from and in addition to its peculiarly transitional structure as regards its air-sac.

It is, indeed, the last of an ancient race, a species of the same genus (known almost exclusively by its teeth) being found fossil in strata of oolitic and triassic date. It was discovered by the Hon. W. Foster, M.C.A. Mr. Gerard Krefft, F.L.S., Curator and Secretary of the Sydney Museum, first described and figured the animal in 1870,* and at once correctly referred it to the genus *Ceratodus*, which up to that time was supposed to be entirely extinct. Its further determination was effected by Dr. Günther.† He has conclusively shown that *Ceratodus* and *Lepidosiren* are closely allied, and thus finally brought the latter definitively within the class of Fishes, for that *Ceratodus* is a fish no one questions. It is an animal, however, of somewhat amphibious habits, as at night it leaves the brackish streams it inhabits, and wanders amongst the reeds and rushes of the adjacent flats. Vegetable substances constitute its principal food.

Ceratodus and *Lepidosiren* together afford the most remarkable evidence of the persistence of the same type of structure in the Vertebrate sub-kingdom. The group to which they both belong reaches back into the very earliest epoch, which has yet afforded us any evidence whatever of the existence of fishes; while the genus *Ceratodus* seems to have persisted unchanged from the period of the deposition of the triassic strata.

Summary.

Taking a rapid retrospect of the course we have pursued, we find that in seeking to decide as to "What is a Frog?" our inquiry into its absolute structure has made known to us an animal of peculiarly specialised and perfect organisation. This has been shown to us pre-eminently by the study of its skeleton. We have especially noted its skull, its wonderfully short vertebral column, its utterly anomalous pelvis, and its scarcely less anomalous foot. The flesh which clothes that skeleton has been seen to exhibit distinct muscles wonderfully like our own, those of the foot, indeed, exceeding ours in number, and being a very marvel of complexity. We have met with a nervous system ministered to by delicate organs of sense, and noted for the ready response to stimuli, made by even separated parts of it as evidenced by strikingly co-ordinated complex movements. We have found the circulation to be carried on by a heart which, at first sight, seems too structurally imperfect to distribute the venous and arterial blood in their respectively appropriate channels. Nevertheless, further examination has shown us that this heart is provided with a special arrangement of parts so delicately co-adjusted as to be able to act thus as efficaciously as does the heart of animals much higher in the scale. Respiration, too, we have seen provided for partly by an effective throat air-pump, partly by a peculiar activity of the cutaneous structures.

We have, moreover, found that this complex adult condition is arrived at by means of a rapid metamorphosis from an immature condition wonderfully different, indeed, but no less perfectly adapted to the life conditions of the tadpole state.

It remains now "to sum up the results" of our investigations through "a series of wider and wider comparisons" to answer, finally, as far as may be, the initial question of this little treatise.

We have, in the first place, seen that the frog belongs to an order far more distinct from cognate ordinal groups than is man's order from other orders of his class mammalia. We have also seen that the frog belongs to an order which is singularly homogeneous, and yet that the class which includes it is remarkably heterogeneous.

Again, we have found that the subordinate groups of the frog's order, families and genera, have very definite

* See "Proc. Zool." Soc. 1870, p. 22.

† See "Phil. Trans." 1871, p. 511; Plates xxx. to xlii.

relations to *space*, and that the order, as a whole, is, as far as yet known, remarkably restricted as regards geological *time*.

The comparisons instituted in our survey of the frog's anatomy will enable us now to sum up resemblances; first, as regards the orders of its class, and secondly, as regards the class itself.

1. Its own order, *Anoura*, has been seen to present singular resemblances to the *Chelonia* amongst reptiles. Such are the bony plates of the back of some forms, the bony covering of the temporal fossa in others, the mode of inspiration in the adult, the armature of the jaws in the young. On the other hand, the peculiar elongated tarsus has reminded us of certain mammals, and the median Eustachian opening of *Pipa* and *Dactylethra* has suggested an affinity to crocodiles and birds. It has been plain, however, that these several likenesses, however singular and striking, are not evidences of genetic affinity.

2. The order *Urodela* may well recall to mind the *Lacertilia* amongst reptiles, with which animals the *Urodela* were actually classed by Linneus. Moreover in both groups we find a series of different species, longer and longer in body and shorter and shorter in limb. We have also seen that in both these groups an analogous complication obtains in the muscles of the legs.

3. The order *Ophiomorpha*, as has been before observed, present a general resemblance to serpents, and a special resemblance to certain short-tailed ones; though it is rather to the Amphisbenian Saurians that they may most advantageously be compared. Here, again, however, we meet with the resemblances which, though striking, do not allow themselves to be interpreted as indices of any special relationship by descent.

4. The order *Labyrinthodonta* recalls to mind, as has been said earlier, the Crocodilia amongst reptiles, of which they may be deemed as the prophetic precursors, so to speak, though certainly not the direct ancestors.

Thus the class *Batrachia*, as a whole, presents a very interesting analogy and parallelism with the class *Reptilia*. It is a parallelism, moreover, which reminds us of that which exists between the various orders of Placental mammals and the great subdivisions of the pouched or Marsupial order of mammals. We have carnivorous, insectivorous, arboreal, aquatic, herbivorous, marsupial beasts, as we have carnivorous, insectivorous, arboreal, aquatic and herbivorous placental beasts. The harmonious variations of the placental and marsupial groups thus present us with excellent instances of affinities independently evolved and not due to hereditary influence.

In a similar way it seems probable that the subdivisions (orders) of the class Batrachia, mimic, as it were quite independently, the subdivisions (orders) of the class Reptilia.

The Frogs' class, as a whole, shows as many striking affinities to some or other fishes. It does so in the possession of gills and of a branchial apparatus during one time of life at the least; a large parasphenoid in the skull; the often persistently unsegmented terminal part of the notochord; the single ventricular cavity of the heart; the presence of a *bulbus aortæ*; the development of a *nervus lateralis*; the communication between the urinary canal and the oviduct, and certain other characters of less importance.

The class *Batrachia* agrees both with fishes and reptiles in having the blood cold, more than one aortic arch, and (except in crocodiles) in not having the distinct ventricles.

The class agrees with fishes, reptiles, and birds, in having no complete diaphragm, and no corpus callosum* in the brain, and no single aorta arching over the left bronchus.

We have now arrived at the end of those considerations seemingly best suited to enable us to answer the initial question, "What is a Frog?" The requisite definition might, of course, have been given much earlier, but these inquiries have seemed necessary to enable the reader to understand the technical terms of such definition—to give them, in his eyes, a real meaning.

The Frog is a tailless, lung-breathing, branchiate vertebrate, with four limbs typically differentiated, undergoing a complete metamorphosis, and provided with teeth along margins of the upper jaw.

The course of our inquiry into the nature and affinities of the Frog has not alone served to answer the question with which this memoir opened. Incidental bearings upon deep biological problems have come before us more than once in its course, nor have all the conclusions which seem to have forced themselves upon us been totally negative.

Thus we have met with several instances of the independent origin of remarkably similar structures, such as a shielded temporal fossa and elongated tarsus, which, together with structures like the tooth of the Labyrinthodon, seem to be characters for the existence of which neither the destructive agencies of nature acting on minute oscillations of structure, nor any sexual phenomena, will account.

Again, in the life-history of the Frog, considered even purely by itself, we find a remarkable example of spontaneous transformations due to innate powers and tendencies.

When, however, this process is considered in the light derived from the curious phenomena of transformation so enigmatically presented to us by the axolotl, we have very strongly brought before us the powerful action of internal tendencies lying dormant and latent till made manifest, through the advent of conditions so obscure that as yet they have evaded the most careful and anxious scrutiny of practised adepts.

It would seem to be a negligence not here to point out, that if new forms of life—new species—arise from time to time through congenital variation, not a few of the facts herein quoted point to the probability that such forms have arisen through the evolutions of implanted potentialities definite in nature, in other words, by "specific genesis."

Again, a general survey of the different kinds of relations which the Frog has brought before us, is well calculated to impress us with the overwhelming richness and fulness of nature.

Although, from our ignorance, the natural history of many other animals well known to us may appear less replete with interest than that of the common Frog may now be, yet it cannot be doubted but that the progress of science is capable of revealing to us facts as full of instruction and of as profound a significance in the life history of almost any kind of animal whatever.

Ever fresh, ever fertile, natural history offers to our faculties a pursuit practically inexhaustible. We are not, indeed, denied the gratification of successfully exploring and satisfactorily explaining mystery after mystery, but each secret wrested by our efforts brings before us other ever new enigmas, so that though refreshed by success we need never be wearied by monotony. While we need not regard any problem as absolutely hopeless, no dread of coming to the end of our inquiries need ever chill the warmth of our zeal in the scientific cause. Some may consider such reflections justified by the phenomena presented to them by the natural history of the Common Frog.

St. George Mivart

* As to this structure see Lesson in "Elementary Anatomy," pp. 367, 375.

THE HABITS OF BEES AND WASPS*

Sir John Lubbock, in a paper on the Social Hymenoptera (Bees, Wasps, and Ants), especially with reference to their habits, senses, and power of communication with one another, pointed out with regard to the latter, that the observations on record scarcely justify the conclusions which have been drawn from them.

* Being the substance of a paper by Sir John Lubbock, Bart., F.R.S., read before the Linnean Society on the 19th March, 1874.

Thus Messrs. Kirby and Spence say that ants have a language "not confined merely to giving intelligence of the approach or absence of danger, but co-extensive with all their other occasions for communicating their ideas to each other." The observations, however, on which this statement is based, scarcely seemed to him to be conclusive. The two Hubers, indeed, had clearly shown that ants and bees could make one another cognizant of their state of feeling, could communicate anger, danger, &c., but that was very different from the possession of a true language.

In support of the opinion that Ants and Bees possess a true language, it is usually stated that if one bee discovers a store of honey, the others are soon aware of the fact. Thus Huber says, "Wasps are also acquainted with the mode of imparting information to their companions. When a single wasp discovers a stronghold of sugar, honey, or other article of food, it returns to its nest, and brings off, in a short time, a hundred other wasps; but we are yet ignorant, if it be by visible or palpable signs, they are mutually informed of this discovery."*

This, however, does not necessarily imply the possession of any power of describing localities, or anything which could correctly be called a language. If the bees or wasps merely follow their fortunate companions, the matter is simple enough. If, on the contrary, the others are sent, the case would be very different. In order to test this, Sir John proposed to keep honey in a given place for some time, in order to satisfy himself that it would not readily be found by the bees, and then after bringing a bee to the honey, to watch whether it brought others or sent them,—the latter, of course, implying a much higher order of intelligence and power of communication.

In the first place, then, he kept some honey for some days at an open window in his sitting-room, and no bees came to it. He then brought a bee up from his hives in the garden in his hand, choosing one which was in the act of leaving the hive. He found it frightened the bees less to be brought in the hand than in a bottle, probably on account of the darkness. The bee thus brought up was then fed with honey, which it sucked with evident enjoyment for a few minutes, and then flew quietly away. But though it had given no symptom of alarm or annoyance, it did not return, nor did any other bee come to the honey. This experiment he repeated eight times, with a like result. He therefore procured one of Marriott's observatory hives, which he placed in his sitting-room. The bees had free access to the open air, but there was also a small side, or postern door, which could be opened at pleasure, and which led into the room.

This enabled him to feed and mark any particular bees, and he recounted a number of experiments from which it appeared that comparatively few bees found their own way through the postern, while of those which did so, the great majority flew to the window, and scarcely any found the honey for themselves.

Those, on the contrary, which were taken to the honey, passed backwards and forwards between it and the hive, making, on an average, five journeys in the hour. In these cases it is obvious that the bees which had found the honey did not communicate their discovery to the others; and the postern being small and on one side, few of the bees found it out for themselves. If the honey had been in an open place, no doubt the sight of their companions feasting would have attracted other bees, but in this case the honey was rather out of sight, being behind the hive entrance; and was, moreover, only accessible by the narrow and winding exit through the little postern door.

Sir John had, also, in a similar manner, watched a number of marked wasps with very similar results.

No doubt when one wasp has discovered and is visiting a supply of syrup, others are apt to come too, but he believed that they merely follow one another. He argued that if they communicated the fact, considerable numbers would at once make their appearance, but he has never found this to be the case. The frequent and regular visits which his wasps paid to the honey put out for them proves that it was very much to their taste. Yet they did not bring their companions with them. For instance, on September 19, when a marked wasp paid more than forty visits to some honey, only one other specimen came to the honey during the whole day. Both these wasps returned on the 20th, but not one other. The 21st was a hot day, and there were many wasps about the house; his honey was regularly visited by the marked wasps, but during the whole day only five others came to it.

From these and other observations of the same tendency he concludes that even if bees and wasps have the power of informing one another when they discover a store of good food, at any rate they do not habitually do so, and this seemed to him a strong reason for concluding that they are not in the habit of communicating facts. If they do not, he argues, discuss among themselves the incidents of the day, their adventures in search of food, their success and fortunes in hunting, is it not a fair inference that they have no power of doing so?

Without in any way regarding the facts now recorded as sufficient or conclusive, he thought they indicated that their communications were confined to the feelings, and that there was no power of transmitting information as to matters of fact.

When once wasps had made themselves thoroughly acquainted with their way, their movements were most regular. They spent three minutes supplying themselves with honey, and then flew straight to the nest, returning after an interval of about ten minutes, and thus making, like the bees, about five journeys an hour. During September they began in the morning at about 6 o'clock, and later when the mornings began to get cold, and continued to work without intermission till dusk. They made therefore rather more than 50 journeys in the day. In fact they were just as industrious as bees, and kept longer hours, as they began earlier in the morning. He believed that the wasps which seemed to be idling in our rooms had simply lost their way. He gave also a number of observations tending to show the difficulty which bees have in finding their way. For instance, he put a bee into a bell glass 18 inches long with a mouth $6\frac{1}{2}$ inches in diameter, turning the closed end to the window. The bee buzzed about for an hour, when, as there seemed no chance of her getting out, he released her.

Although, as everyone knows, wasps are easily startled and very much on the alert, still they are very courageous.

On one occasion one of his marked wasps had smeared herself with honey and could not fly. When this happened to a bee it was only necessary to carry her to the alighting-board, when she was soon cleaned by her comrades. But he did not know where this wasp's nest was, and could not, therefore, pursue a similar course with her. At first he was afraid she was doomed. He thought, however, that he could wash her, fully expecting, indeed, to terrify her so much that she would not return again. He therefore caught her, put her in a bottle half full of water, and shook her up and down well till the honey was washed off. He then transferred her to a dry bottle and put her in the sun. When she was dry he let her out, and she at once flew to her nest. To his surprise, in thirteen minutes she returned as if nothing had happened and continued her visits to the honey all the afternoon. The next morning she was the first to arrive.

He also had made some experiments on the behaviour of bees introduced into strange hives, which seemed to contradict the ordinary statement that strange bees are always recognised and attacked.

Another point as to which very different opinions have

* Huber, "Nat. Hist. of Ants," p. 374

been established is the use of the antennæ. Some entomologists have regarded them as olfactory organs, some as ears; the weight of authority being perhaps in favour of the latter opinion. In experimenting on his wasps and bees Sir John, to his surprise, could obtain no evidence that they heard at all. He tried them with a shrill pipe, with a whistle, with the violin, with all the sounds of which his voice was capable, doing so, moreover, within a few inches of their head, but they continued to feed without the slightest appearance of consciousness.

Lastly he recounted some observations to show that bees have the power of distinguishing colours. The relations of insects to flowers imply that the former can distinguish colour, but there had been as yet but few direct observations on the point.

THE CAVENDISH LABORATORY

THIS Laboratory, in which every facility is furnished for the prosecution of physical research, is the munificent gift of William Cavendish, Duke of Devonshire, K.G., Chancellor of the University, who has intimated his intention of presenting it complete to the University.

The building, which is now finished, was erected from the designs of W. M. Fawcett, M.A., of Jesus College, at an expense of about 10,000*l.*

The ground-floor contains a set of rooms for operations requiring great steadiness, such as the measurements of length, time, and mass, and of heat, electricity, and magnetism. A store-room, a workshop, and a battery-room are also provided on the ground-floor.

The first floor contains a spacious lecture-room with a preparation-room, a large apparatus-room, a private room for the professor, and a large working laboratory, fitted with tables standing on beams of their own, so as to be independent of the vibrations of the floor. All the tables in the building are supported in the same way, and there are in every floor small trap-doors, by means of which bodies may be suspended over the tables in the room beneath, and through which electric and other communicaions may be made.

The upper rooms are intended for acoustics, radiant heat, optics, electricity, and the graphic reduction of observations. There is also a dark room for photographic preparations. The air in the electric room will be kept dry by a contrivance due to Mr. Latimer Clark, and the electric machine worked in this room may be made to furnish electricity for experiments in the lecture-room.

In the tower will be erected an iron tube, which may be filled with mercury so as to measure the greater pressures to which gases and vapours are subjected in the heat-room on the ground-floor. There is also an arrangement by which the electric potential of the air at the top of the tower may be measured either in the lecture-room or in the electric-room.

The laboratory is open daily from 10 A.M. till 6 P.M. under the superintendence of the Professor of Experimental Physics, for the use of any member of the University who may desire to acquire a knowledge of experimental methods, or to take part in physical researches.

NOTES

A REUTER's telegram from Aden, of March 23, states that the steamer *Calcutta* arrived there from Zanzibar on the previous day with the body of the late Dr. Livingstone. We fear this must be regarded as final, and as shutting out any further hope; we can only now do all possible honour to those remains which the doctor's faithful servants have so religiously preserved. A letter recently received from Zanzibar, by Mr. R. A. Laing, states that the body, after having been exposed to the sun for a month to dry, and then packed in a hollowed tree, was wrapped round with cloth, and the natives carrying it supposed it a bale of cloth, or kaniki.

H.M.S. *Challenger* arrived at Melbourne on the 17th inst.: all well. On her voyage from the Cape of Good Hope, she reached the Antarctic Circle between E. long. 70° and 80°.

IN connection with our leading article this week we see with pleasure that Mr. Mundella gave notice in the House of Commons on Monday, that "at an early day he would call attention to the Report of the Science Commissioners on National Museums, and move that, in the opinion of the House, steps should be taken to render National Museums and Galleries of Art more available for instruction for the purposes of Science and Art." We sincerely hope Mr. Mundella's motion will lead to some decided step in advance.

WE are sorry to have to announce the death of Johann Heinrich Maedler, the distinguished German astronomer, at Hanover, on March 14, at the advanced age of eighty. One of his best-known works is a Map of the Moon, of which he was the joint author with M. Beer. He was appointed Professor of Astronomy and Director of the Observatory at Dorpat in Russia about 1840, and was also the writer of various astronomical treatises:—"Popular Astronomy," Berlin, 1849; "The Existence of a Central Sun," Dorpat, 1846; "Lectures on Astronomy," Mittau, 1845–47, &c.

WE are informed that the Royal Belgian Academy has resolved to place the bust of Quetelet in the hall where its meetings are held. We believe no successor to the Directorship of the Brussels Observatory has yet been named.

AT the last meeting of the Royal Irish Academy, the Rev. Prof. Jellett resigned the office of president, and Wm. Stokes, M.D., D.C.L., F.R.S., was elected in his stead. Dr. Sullivan also resigned the secretaryship of the Academy on being made president of the Queen's College, in Cork, and Dr. E. Perceval Wright, F.L.S., was elected to the post. Dr. R. McDonnell, F.R.S., was also elected to the secretaryship of Foreign Correspondence in the place of Sir W. Wilde.

THE Professorship of Astronomy in the University of Dublin, the holder of which is also Astronomer Royal of Ireland, is now vacant by the resignation of Dr. Francis Brünnow. Since its foundation this professorship has been held by Dr. Henry Ussher (1783), Dr. John Brinkley (1790), Sir William Hamilton (1827), and Dr. Brünnow (1865). The election will be held on April 18. Rumour in Trinity College points to Prof. R. Ball, LL.D., F.R.S., as the most likely successor to Brünnow, a distinguished graduate of the University of Dublin in both pure mathematics and experimental physics. Dr. Ball acquired an extensive knowledge of astronomy during the several years that he acted as the late Lord Rosse's assistant at the Observatory at Parsonstown.

AT a numerously attended meeting of the Fellows of the Royal College of Surgeons, Ireland, held in the College Hall, Dublin, on the 13th inst., it was resolved, by a large majority, that it is not expedient for this college to take part in the proposed conjoint scheme for the examination of medical graduates in Ireland. The conjoint scheme had already been approved of by the Council of the College, by the Medical Professors and Board of Trinity College, Dublin, by the King and Queen's College of Physicians, Ireland, and by the Governors of the Apothecaries Hall.

THE circular of the Board of Trade, respecting Storm Warnings, which appeared in our last, appears to require a few additional remarks by way of explanation. The circular speaks of an explanatory pamphlet which is now before us, and we see from it that the intention of the present system of signals is to give an indication of the *direction* of the wind to be apprehended in every case. The drum is never to be used without the cone. Its signification in Admiral FitzRoy's time was "dangerous winds from nearly opposite quarters successively;" and it accordingly gave no indication of direction *by itself*. Experience has shown that there is a much greater degree of certainty in foreseeing the *direction* than the *force* of a coming strong wind. Furthermore an attempt is made to give a degree of numerical definitiveness to the warnings, which at once admits that they are not infallible. The Committee say:—"Hitherto it has been found that at least three out of five signals of approaching storms (force upwards of 8 Beaufort scale, a 'fresh gale,') and four out of five signals of approaching strong winds (force upwards of 6 Beaufort scale, a 'strong breeze') have been fully justified." We may fairly consider this as a step in the direction of treating weather indications by the laws of exact science.

WE are glad to learn that Prof. Alluard, of Clermont-Ferraud, has at last succeeded in surmounting the various obstacles which he met with in the establishment of his proposed observatory on the Puy de Dome, at an elevation of about 1,660 metres above the surrounding country (NATURE, vol. vii. p. 481). The chief difficulty arose from the opposition of the peasant proprietors to the invasion of their rights by the construction of a road and erection of the building. M. Alluard announces that the observatory will be ready to be inaugurated in September next, and has invited his meteorological friends to visit Auvergne on that occasion.

A LARGE deposit of Moa bones has lately been discovered in a swamp at Hamilton, in Otago. Besides *Dinornis*, the swamp contains bones of *Aptornis*, *Harpagornis*, &c. The whole have been secured by the curator of the Otago Museum.

The Sedgwick Geological Prize (Cambridge) has been adjudged to J. J. Harris Teale, B.A., St. John's College. The subject for the next prize will be—"The post tertiary deposits of Cambridgeshire and their relation to deposits of the same period in the rest of East Anglia."

THE French Society of Geography has decided upon holding an International Geographical Congress at Paris in 1878. Rules and programmes will be issued shortly.

THERE has recently been concluded in connection with the Liverpool Free Public Library and Museum, a carefully arranged and excellent course of Free Lectures. This is the ninth winter course in connection with the same institution, from which we are glad to infer that these free lectures have been a success. We should like to see similar courses inaugurated in all our large towns; we believe the results would be in the highest degree beneficial. The following is a summary of the Liverpool course:—Eight Lectures on Art, by Mr. W. J. Bishop; Three Lectures on Natural History, by Mr. T. J. Moore; Six Lectures on the Chemistry of Salt, and of the Manufactures depending on it, illustrated with Specimens, Experiments, Diagrams, &c., by Mr. Edward Davies, F.C.S., &c.; Three Lectures on Geology, by Mr. G. H. Morton, F.G.S., F.R.G.S.I.; Two Lectures on Mineralogy and Mining, by Mr. F. P. Marrat, M.L.G.S.; Six Lectures on Navigation and Astronomy, by Mr. J. T. Towson, F.R.G.S.; Two Lectures on Art and Antiquities, and one on Town Window Gardens, by Mr. Charles T. Gatty; Three Lectures on the Constitutional History of England, illustrated with Historical Maps, by Mr. James Birchall; Four Readings, by Mrs. H. J. Gorst.

THE Austrian amateur navigator, Count Wilczek, writing in the *Neue Freie Presse*, says that there is no ground for apprehension as to the fate of the Austrian Polar Expedition which sailed in the *Tegetthoff*, in the year 1872, and that news will probably be received from the expedition in October or November next. Letters for members of the expedition will be despatched by the Austro-Hungarian Government by means of whaling and other vessels bound for the Arctic seas.

MESSRS. TRÜBNER and Co. have in the press and will shortly publish a treatise on "Valleys, and their Relation to Fissures, Fractures, and Faults," by G. H. Kinahan, M.R.I.A., F.R.G.S.I. This work will be dedicated by permission to His Grace the Duke of Argyll.

MR. F. C. S. ROPER, F.L.S., has published a "Supplement to the Fauna and Flora of Eastbourne, together with a list of Eastbourne Cretaceous Fossils."

MESSRS. S. W. SILVER & Co. have just published a "Handbook for Australia and New Zealand," containing a large amount of varied and useful information about the various colonies in that quarter of the world. It is accompanied by a "Seasons'-Chart of the World."

THE additions to the Zoological Society's Gardens during the last week include two Palm Squirrels (*Sciurus palmarum*) from Ceylon, presented by Capt. Forster; a Sonnerat's Jungle Fowl (*Gallus sonnerati*) from South India, presented by Mrs. White; two Tench (*Tinca vulgaris*) British, presented by Mr. W. Arnold; a Black-eared Marmoset (*Hapale penicillata*) from Brazil, presented by Mr. F. Graham; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Colonel Carington; two Boat-bills (*Cancroma cochlearia*) from South America, deposited.

*CELESTIAL CHEMISTRY**

IT now and then happens in the history of the human race upon this planet, that one particular generation witnesses the most stupendous advancement of knowledge, this advancement generally coming from what one might consider an exceeding small germ of thought. You will at once call to mind several such instances. You will recollect how once a Dutchman experimenting with two spectacle-glasses produced the Telescope; and how the field of the known and the knowable has been enlarged by the invention of that wonderful instrument. Again, you recollect how once Sir Isaac Newton was in a garden and saw an apple fall, and how the germ of thought which was started in his mind by that simple incident fructified into the theory of universal gravitation. You will also acknowledge that each step of this kind has more firmly knit the universe together, has welded it into a more and more perfect whole, and has enhanced the marvellous beauty of its structure.

I think that future times will say that either this generation, or perhaps the next, is as favoured a one as that which saw the invention of the telescope or the immortal discovery of Newton: for as by the invention of the telescope the universe was almost infinitely extended; as from Newton's discovery we learned that like forces were acting in like manner everywhere; so in our time does the wonderful instrument called the Spectroscope show us that like matter is acting in like manner everywhere; so that if matter and force be not identical, then these two, namely, matter and force, may be termed the foundation stones of the universe in which we dwell.

My present object is to bring before you as well as I can some first notions which are to be got out of this general examination of all matter beyond our own planet, in its chemical relations; this examination having been rendered possible by the spectroscope.

In the first instance, before I attempt to deal with chemical ideas in relation to the heavenly bodies, I have two things to do. I must first refer to our earthly notions of chemistry, not of course in their generality, for that would be impossible in the time at my disposal, but to that side of them which touches most intimately what I shall have to say by and by; and I must also refer to the results which we have already obtained with regard to the constitution, so to speak, of terrestrial matter, as it is brought before us by the spectroscope.

* Revised from short-hand notes of a Lecture delivered at the Quebec Institute, on Tuesday, December 16, 1873.

First, then, with regard to chemistry. What is chemistry? It is a science which deals with the matter which surrounds us, and of which the whole planet and we ourselves are built up. We see everywhere around us an enormous number of apparently perfectly distinct things, some of them having vital properties, some of them lifeless, motionless; but out of this apparently infinite diversity chemistry presents us with an almost perfect simplicity. It tells us that everything which exists here is really made up of only sixty-three different things; that the whole of the animal kingdom, the vegetable kingdom, the mineral kingdom—everything—is made up of only sixty-three different substances. That is a wonderful simplification, and science always simplifies.

Now we may look upon those sixty-three elements in two distinct points of view. We may consider them in their physical relations, or we may regard them in a more purely chemical aspect. If we look upon them in relation to their physical conditions, we find that amongst them are fifty-six solids, two liquids, and five gases. If we look upon them chemically, dropping all distinctions between solids, liquids, and gases, we say that some of them are metals, some metalloids; and of some, it may be truly said that it is very difficult to place them exactly—to determine whether they are on the side of the metals or on the side of the metalloids—in the same way as the biologist finds it absolutely impossible to put his finger upon any particular part of the organic world and say, Here the vegetable, or here the animal, kingdom begins. All these chemical distinctions, then, are quite independent of physical conditions. For instance, I shall have to show you that amongst the most metallic of the metals is a gas. Again, among the metals we have a liquid—mercury; so that we have a complete chain of gas, liquid and solid among the metals, although popularly the term metal is often imagined to apply only to such solids as gold, silver, and iron. On the metalloid side, again, we have gases among them the familiar oxygen and nitrogen; we have the liquid bromine, and so on, added to other unmistakable metalloids, such as phosphorus, sulphur, carbon, and iodine, generally thought of in their solid form.

Now what are the chemist's tools by which he has brought about this marvellous simplicity, what the processes by which he carries on his operations? I answer, in the main *vibrations*. He finds the world composed of molecules in millionfold complexities, combinations, and sizes, and he acts upon these molecules by vibrations. For gross molecules he finds in heat most that he wants, but when the molecules are more delicate, then electricity is called in, and electricity does for these what heat did for the others.

Let me here endeavour to make my meaning clear. I want you to assume a long series of vibrations, long at one end of the series and short at the other. We know that heat consists of vibrations, we know that light consists of vibrations. I will also ask you to think of electricity as connected with vibrations, and I ask you further to assume these vibrations to be short. We get heat from the sun, and among these vibrations are some to which our eye is tuned. We get an immense vibration of heat from the oxyhydrogen flame, a flame the heat of which is due to the formation of the gaseous molecules of water, but we get, practically speaking, no light. Many of the electrical phenomena with which we are acquainted take place unseen, and without heat, showing they are not long-wave phenomena; others are exquisitely visible to us, because the vibrations are within our ken; but, to get associated heat, we want pressure, and with pressure we can render the oxyhydrogen flame luminous. In fact—and here let me be perfectly frank with you—I call your attention to the "*as if*"—it is *as if* we have long heat-waves at one end of a long scale, and short electricity-waves at the other, each with different functions, heat giving us with solids and liquids *visible* phenomena, because of added shorter waves, electricity giving us visible phenomena with gases and vapours, because of added longer waves; heat passing invisibly through gases, electricity passing invisibly through solids; heat bringing about chemical changes in solids and liquids, electricity bringing about similar changes in the case of gases.

Now, this being so, let us assume, for the purposes of the present statement, that the mode of motion heat, with its long waves, chiefly affects the larger molecules, that is, compound bodies, and the mode of motion electricity, whatever electricity may be, chiefly affects the smaller molecules, that is, the atoms of simple substances. We shall find, in accordance with this assumption, that if a chemist wishes to reduce the millions of compound molecules in that very compound molecule a piece of ice, he applies heat, and he gets a physical simplification, but not a chemical one, when water is produced; a still further, and exactly similar, stage is reached when this water takes the form of steam, but it is not till an enormous temperature, with its added short vibrations, or electricity, is employed, that the compound molecule breaks up into the simple things oxygen and hydrogen, unless another vibration is superadded of a molecule of another simple thing (or element) which shall aid in shaking them apart.

As instances of the action of heat, I will show you one or two experiments to indicate that in a great deal of chemical action the heat vibration requisite to bring about that simplification by means of which the simple bodies have been determined to exist as such is supplied by the chemical action itself; it is the heat of arrested motion. In other cases we have to supply the heat artificially; but also bear this in mind, that whenever we apply artificial heat the heat is none of our making. It also is the result of a chemical combination. For instance, if I take some potassium and throw it into water, that potassium will instantly burst into flame. You will see that we have a perfectly cool metal put into perfectly cool water, and, as you see, it at once takes fire in consequence of the heat of combination which has been brought about by the attraction between the potassium and the water. And if I had time I could show you that as the result of that heat-vibration thus introduced the water has been simplified, one of its constituent simple things, hydrogen, has been liberated, and I might have collected it in a bell jar.

Another illustration is to be got from a mixture of water and sulphuric acid. I have, in a test-tube, some ether, and I have the water. When I pour the water into a glass you will see that the ether in the test-tube placed in the glass will remain as if nothing had happened. But now I will pour some sulphuric acid into the water, and what happens? We get an attraction between these two things: we get a heat vibration as the result of chemical combination; and, as the result of the heat vibration produced in that manner, the water gets hot and the ether boils, the boiling point of ether being below that of water.

Here is another experiment, and I have chosen these out of many others which might have been brought before you, to show the changes brought about by heat vibrations. Here we have some bichromate of potassium, and on the application of heat it will be instantly reduced. When I say instantly reduced, probably a few seconds will be required in order to allow the heat vibration to act, and you will then have a change of colour in the solution brought about by the application of heat, artificial, so to speak, in this case, although, as I have already cautioned you, the heat of the Bunsen burner which we employ is really an effect of chemical combination.

But not only have we heat with its long waves to bring about chemical action and its result, simplification, but, as I have said, we have another agent, electricity. I have here two tubes filled with water, and a battery, and in each tube connected with this battery is a strip of platinum. The instant that the circuit is made complete you see that the water is decomposed, bubbles rise from the platinum foil, which bubbles in the one case are bubbles of hydrogen, and in the other case bubbles of the other constituent of the water—oxygen. Here you see, by means not of the long waves of heat, but by means of electricity, we bring about a complete dissociation or a complete separation of the elements of the water which originally was in these two tubes. And if we were to allow the experiment to go on a little longer, you would see that not only is there the evolution of gas in each of the tubes, but that the evolution will be greater in one tube than in the other, for this reason, that in the water there are two equivalents of hydrogen to one of oxygen.

These then are instances of simplification brought about by heat and electricity. I quit this part of the subject by the remark that the ultimate particles of an element are called atoms; that agglomerations of atoms are termed molecules; elementary molecules when the atoms are alike; compound molecules when the atoms are dissimilar. The heat-waves generally help us to get at the molecule, and electricity helps us to get at the atom; and mark, I only say *generally*. It might be universally true if all elementary atoms were alike; but on that point we must be content to say that we do not know. But I might place much evidence before you which indicates that they are vastly different. We can only study them by their vibrations;

for, as Sir Wm. Thomson has calculated, the atoms in a drop of water are so small that if the drop of water were magnified to the size of the earth, the atoms would then be seen not larger than cricket-balls or not smaller than shot.

It must be clearly understood that I here refer to the true atom and not to the atom of the chemists, the weight of which they give as the "atomic weight." It may probably turn out that this is often a molecule, sometimes a complicated one, which great heat or electricity can divide, the latter sometimes more than once. It is clear that if this be so, then the vapour densities as referred to the atomic weight will be "anomalous," because the true atom and not the chemist's atom is in question at these high temperatures.

It is now time for us to pass to the action of the spectroscope. The spectroscope, as you know, is the instrument which enables us to deal with either the refraction or the diffraction of light; that is to say, by means of refraction or diffraction we sort out the rays of any beam which we may choose to use into a spectrum, and we then study by means of that spectrum the nature and conditions of the substance which gave us the light.

And there is more than this. Not only can we deal with the giving out of light as light is being given out by this lamp, or that flame, or that gas before me, but we can equally use the absorption of light by various substances, thus studying the nature and conditions of these substances. You know very well that if this lamp, instead of having a shade of ground glass had a red one, the light that would reach your eye would be red. That simply results from the fact that the red glass stops in the main all light but the red, and allows the red to reach your eye. That then is a case of absorption, as the giving out of light by the wick of the lamp is a case of radiation.

What, then, does the spectroscope tell us with regard to the physical differences in matter? It tells us that if we have matter in a solid state, that is matter the molecules of which are large and are near together, agitated by the waves of heat, or by electricity, we get a spectrum from it of a particular kind, called a "continuous spectrum," because the spectrum is absolutely continuous, the red, yellow, orange, green, blue, violet, are all there, as you see them in the rainbow; whereas, if we deal with a gas or vapour not too dense, that is with a substance the atoms or molecules of which are smaller and further apart than in the former case, similarly agitated by electricity or, in some cases, by heat, you find that instead of having what is called a continuous spectrum, you have a spectrum in which the light is not continuous, but broken. The result of this broken condition is that we have light as it were only here and there in the spectrum. We have in fact bright lines representing a few images of the slit, instead of a rainbow band, complete from the red to the violet, representing continuous images of the slit. This you see at once enables the spectroscope to tell us the difference between the rare and the dense states of matter quite independently of what that matter may be, and whether we use radiation or absorption as the test; since a substance with a certain molecular arrangement absorbs precisely the same undulations as it gives out with the same molecular arrangement. No matter what it is, the spectroscope at once tells us whether this matter is in a gaseous or vaporous state, in which case we have lines or bands; or in a state in which the molecules are nearer together, when we get a more or less complete continuous spectrum. This at once partly explains why the almost invisible long waves of the oxyhydrogen flame soon fill a mass of the most refractory metal with waves of all lengths, until it shines out almost like the sun. It would appear that molecules or atoms, when once set vibrating by either long or short waves, perform *all* the vibrations proper to them under the conditions present.

How then about the chemical differences? Here the information afforded by the spectroscope is of a much closer character. In the first place it tells us that if you take any substance whatever in a state of gas or vapour, not only do you get bright lines, which tell you that you are dealing with a gas, but you get different bright lines for every substance, so that you not only know that you are dealing with a gas or vapour, but you know at the same time what particular gas or what particular vapour. This is qualitative spectrum analysis, as the effects depend upon the quality of the atoms or molecules present. Further, we see a change in the spectrum from simplicity to complexity, by which I mean that the lines increase in number and broaden, and that the bands become more complete and their channelled structure, where it exists, comes out better, as we pass from a low to a high pressure. This is quantitative spectrum analysis, the change depends upon the quantity of the atoms or molecules present.

Again, the spectroscope at once enables us in the main (and I say in the main, because I have already referred to the borderland between the metals and the metalloids) to differentiate quite as sharply between metals and metalloids as it does between solids and gases.

A metallic spectrum is always a line spectrum when we employ electricity to produce the vapour. Only certain metals give us line spectra at low temperatures: these are mostly monad metals which vaporise easily.

A metalloidal spectrum is only a line-spectrum when we employ electricity. Long heat-waves in their action upon the molecules only produce bands and channelled spaces. Thus the vapour of sulphur has three spectra, two to be obtained by heat, the line spectrum only being obtained by electricity..

Nor is this all. As we can distinguish the spectrum of a metal from the spectrum of a metalloid by the appearance of the spectrum, so also does the spectroscope enable us to see a difference between the spectrum of a compound molecule and an elemental molecule. Let me explain what I mean:—If we are dealing with a metallic element, we get a spectrum of a particular kind so sharply defined that when any one has once seen it, he always knows that an atom of a metal is being dealt with. In the same way when we are dealing with metalloids, the spectrum is generally so entirely distinct from the spectrum of a metal, that when you have once seen the spectrum of a metalloid produced by the long heat-waves, you will always be able to tell it again, there is no possibility of mistaking it for the spectrum of a metal. So far we have been dealing with the elemental molecules, or perchance atoms of metals and metalloids, but we can take a compound molecule. Let us take the combination between metalloids and metals, such as some of the salts of strontium—the chloride of strontium, iodide of strontium, and so on: here we have compound molecules, that is, molecules no longer built up of one substance, but of two; and the long heat-waves, although they can set them vibrating and therefore make them radiate light, do not shake them asunder as high tension electricity does.

We find that the spectroscope is perfectly competent to separate such spectra from all others, so that when we have once seen the spectrum of, say, iodide of strontium, we shall for ever afterwards know that such spectra are given by such a compound molecule as iodide of strontium. The same remark applies to the compound molecules in which oxygen enters as one of the substances. Such spectra closely resemble the spectra of the metalloids, but the bands are farther apart and lie nearer the violet as a rule, so that it is not difficult to distinguish them.

Now when we have to do with a compound molecule, that is to say, with an association of two molecules or atoms of two different chemical substances, we shall at once see that this question of vibrations instantly comes into play; for if the function of vibration, whether we deal with large molecules and long heat-waves, or small molecules and electricity, is to render more simple what in the first instance was compound, then we ought to get spectroscopic differences.

Let us again take the iodide of strontium; the spectroscope is perfectly capable of letting us see the exact effects, not of every degree of temperature which we employ, but of any great differences of temperature. We can follow each increase of temperature by observing the lines or bands which disappear, or which begin to be visible, as the case may be, as the temperature is increased. And similarly, if we have a mixture at a temperature of dissociation, and gradually bring the temperature down until association takes place, then also the spectroscope is just as competent to help us as it was before when we were dealing with an increasing temperature. We find that as the temperature decreases in the latter case, the peculiar compound spectrum to which I have already referred gets more and more visible at the same time as the elemental spectrum gets less and less visible: the order being one of strict law absolutely capable of prediction the moment you know what are the elemental lines, and the lines of any particular compound which longest resists the action of pressure.

Now this is extremely important in its bearing upon the celestial side, so to speak, of this inquiry, and therefore if you will allow me I will still further enlarge what I have said about this distinction between the metals and the metalloids.

If I take sodium vapour at a very low temperature and at the highest temperature that I can get on the earth either by the long heat-waves or by means of electricity, I find that there is absolutely no difference whatever in the molecular arrangement of that sodium vapour at the extreme points. Spectroscopically it is absolutely the same.

Then if I take, not sodium but another element, such as iron, I find it excessively difficult, by means of the heat-waves, to shake asunder the molecules of iron and the diatomic or polyatomic molecules of iron vapour at all. But we know that by electricity non-atomic iron vapour can be got; and then we may say, at all events so far as the lines in the spectrum are concerned (I do not mean their position, but their general nature), that we get a spectrum from the vapour of iron, similar in character to that of the vapour of sodium; but the spectrum has become more complicated as we pass from the monad metal to one with a higher atomicity.

Suppose that, instead of taking a monad metal like a sodium, with its few-lined spectrum, or a metal like iron, with its high atomicity and its many-lined spectrum, we take a *metalloid*; then we find that those conditions no longer hold good. It is not too much to say that in the case of the metalloids every change of even low temperature brings about a change in the spectrum. It is perfectly true, as I have said before, that by means of electricity we can get a line-spectrum from most of the metalloids. But from the ordinary temperature to the electric spark in the case of a metalloid, instead of getting the perfect similarity that we did in the case of sodium vapour, we get an equally perfect and equally beautiful dissimilarity; so that whilst we say that in the case of sodium we only know of but one spectrum, in the case of sulphur, to take one case, we certainly know of four.

You must let me again remind you that when we employ electricity the spectra of the metalloids present exactly the same appearance as the spectra of the metallic elements, such as iron and sodium, and that it is only when we employ heat-waves that those other changes to which I have referred take place.

One word more, too, on the fundamental difference between the spectrum of a metalloid and the spectrum of a metal on the one hand, and the spectrum of a compound on the other. The metalloid has a spectrum of channelled spaces or bands, sometimes to be found in the central part, that is to say, in the green part, or thereabouts, of the spectrum, whereas in the case of the vapour of metals such as iron, and so on, we get bright lines only, not bands; and these lines increase in number generally toward the violet, while in the case of the compound molecules, such as iodide of strontium, to which I referred, we get a something which is half channelled spaces and bands, and half lines, but in all the cases I have examined, excluding oxides, they are limited to the red end of the spectrum.

Let me attempt briefly to summarise what I have stated. With electricity in the case of all elements we obtain line spectra; as we are here dealing with the most complete simplification of matter that we can attain, let us call this the *atomic spectrum*.

With heat we can obtain a continuous spectrum, from solids, liquids, and some vapours; with electricity we can even obtain a similar spectrum from dense gases. Let us call this the *molecular spectrum*.

In the case of many of the metalloids we get, between these extremes, a channelled space spectrum. Let us term this the *sub-atomic spectrum*.

In the case of some compound molecules, we get by heat in some cases, and by electricity in others, a spectrum which is dissimilar from all these. Let us call this the *compound atomic spectrum*.

J. NORMAN LOCKYER

(*To be continued.*)

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 19.—Preliminary Notice of Experiments concerning the Chemical Constitution of Saline Solutions, by Walter Noel Hartley, F.C.S., Demonstrator of Chemistry, King's College, London.

The author has been engaged in investigating the above subject during the last eighteen months, and his experiments being still in progress, he thinks it desirable to place the following observations on record:—

In the examination of the absorption-spectra, as seen in wedge-shaped cells, of the principal salts of cerium, cobalt, copper, chromium, didymium, nickel, palladium, and uranium, to the number of sixty different solutions, it was noticed that the tinctorial properties of the substances could be ascertained by noticing the absorption-curves and bands, so that, provided water be without chemical action, it could be foreseen what change would occur on dilution of a saturated solution.

The Effect of Heat on Absorption-spectra

When saturated solutions of coloured salts are heated to 100° C. there are (1) few cases in which no change is noticed; (2) generally the amount of light transmitted is diminished to a small extent by some of the more refrangible (the less refrangible), or both kinds of rays being obstructed; (3) there is frequently a complete difference in the nature of the transmitted light. Anhydrous salts not decomposed, hydrated compounds not dehydrated at 100° C., and salts which do not change colour on dehydration, give little or no alteration in their spectra when heated.

Solutions of hydrated salts, and most notably those of haloid compounds, do change; and the alteration is, if not identical, similar to that produced by dehydration and the action of dehydrating liquids, such as alcohol, acids, and glycerine, on the salts in crystals or solution.

A particular instance of the action of heat on an aqueous solution is that of cobalt chloride, which gives a different series of dark bands in the red part of the spectrum at different temperatures, ranging between 23° C. and 73° C. Band after band of shadow intercepts the red rays as the temperature rises, till finally nothing but the blue are transmitted. Drawings of six different spectra of this remarkable nature have been made. The changes are most marked between 33° and 53°, when the temperature may be told almost to a degree by noting the appearance of the spectrum. Though to the unaided eye cobalt bromide appears to undergo the same change, yet, as seen with the spectroscope, it is not of so curious a character, the bands being not so numerous.

With cobalt iodide a band of red light is transmitted at low temperatures; this moves towards the opposite end of the spectrum with rise of temperature until it is transferred to such a position that it consists of green rays only. In this instance the change to the eye is more striking when seen without the spectroscope, because the mixtures of red, yellow, and green rays, which are formed during the transition, give rise to very beautiful shades of brown and olive green. Thus a saturated solution at 16° C. was of a brown colour, at - 10° C. it became of a fiery red and crystals separated, at + 10° reddish brown, at 20° the same, at 35° Vandyke brown, 45° a cold brown tint with a tinge of yellowish green, at 55° a decidedly yellowish green in thin layers and yellow brown in thick, 65° greenish brown, thin layers green, 75° olive-green. An examination of this cobalt salt has shown that there are two distinct crystalline hydrates; the one formed at high temperatures has the formula $CoCl_2.2H_2O$, and is of a dark green colour; the other, which contains a much larger proportion of crystalline water, is produced at a low temperature, and its colour is generally brown, in cold weather inclining to red.

The action of heat on solutions of didymium is characterised by a broadening of the black lines seen in the spectrum, more especially of the important band in the yellow; and in the case of potassio-didymium nitrate, this is accompanied by the formation of a new line. In the case of didymium acetate, which decomposes with separation of a basic salt, the lines thickened on heating.

Thermo-chemical experiments

Regnault (Institut, 1864; "Jahresbericht," 1864, p. 99) has shown that on diluting a saturated solution of a salt, as a rule there is an absorption of heat, but in one or two cases he noticed that heat was evolved. The change in colour that takes place on the dilution of saturated solutions of cobalt iodide, cupric chloride, bromide and acetate is very remarkable. There is every likelihood that this phenomenon is due in each case to the formation of a liquid hydrate. It is impossible of belief that accompanying such a circumstance there should be no measurable development of heat; and the author's experiments have proved that in the above cases, at any rate, the heat disengaged is very considerable, amounting, for instance, on the part of cupric chloride, at least to 2,565 "units when 1 gram molecule of the crystalline salt is displaced in its minimum of water at 16° C. and brought into contact with sufficient to make the addition of 40 Aq." These numbers only roughly approximate the truth. On diluting a solution of cobalt iodide till the red colour appears, the thermal effect must be much greater, as not only does it register several degrees on an ordinary thermometer, but it may be perceived by the hand.

The conclusions indicated by these results are obvious, but it is beyond the scope of this paper to refer to them. The writer hopes before long to complete his experiments with the view of having them communicated to the Royal Society.

Spectroscopic Observations of the Sun, by J. Norman Lockyer, F.R.S., and G. M. Seabroke, F.R.A.S.

Note on the Intracellular Development of Blood-corpuscles in Mammalia, by Edward Albert Schafer.

Linnean Society, March 19.—Dr. G. J. Allmann, F.R.S., in the chair.—The following papers were read:—Observations on Bees and Wasps, by Sir John Lubbock, Bart., F.R.S. (for an abstract of which see another column), followed by an interesting discussion in which the president, Mr. Robert Warren, Major-General Strachey, Mr. A. W. Bennett, Prof. Newton, Prof. Thiselton Dyer, Mr. D. Hanbury, Mr. Elliot of New York, and others, took part.—On *Oniscigaster wakefieldi*, a singular insect from New Zealand, belonging to the family Ephemeridæ, with notes on its aquatic conditions, by R. M'Lachlan.

Zoological Society, March 12.—Prof. Newton, F.R.S., in the chair.—The Secretary called the attention of the meeting to an important addition that had been made to the Society's collection on the 7th inst., by the acquisition of a young male Javan rhinoceros (*Rhinoceros sondaicus*) from Batavia, believed to be the first example of this rhinoceros that had ever been brought alive to Europe.—A letter was read from the Rev. S. J. Whitmee, resident at Samoa, stating that he had forwarded, through Dr. G. Bennett, of Sydney, a *Didunculus* and two curlews for the Society's collection, and giving interesting particulars concerning the habits of this bird, and another peculiar Samoan species, *Pareudiastes pacificus*.—An extract was read from a letter addressed to the Secretary by Dr. George Bennett respecting a *Didunculus*, and other birds, he had received from the Rev. Mr. Whitmee, of Samoa, intended for the Society's collection.—Dr. Günther, F.R.S., gave some details concerning the recent introduction into this country, by Lord Arthur Russell, of the Ide (*Leuciscus melanotus*, var. *orfus*).—Prof. Huxley read a memoir upon the structure of the skull and of the heart of *Menobranchus lateralis*, describing the structure of the bony skull in the osteo-cranium, and giving a full account of the primordial skull or chondrocranium, which has not hitherto been noticed. The chondrocranium was compared with that of *Proteus*, and that of larval frogs and tritons, and its essentially embryonic character was indicated. The chondrocranium was further shown to be formed by the coalescence of three distinct classes of elements which were termed *parachordal*, *pleural*, and *paraneural*. The heart was described, and the septum of the auricles was shown to be an open network allowing of free communication between the right and left auricular chambers. The structure of the *Truncus arteriosus* was compared with that observed in other amphibians.—Mr. R. B. Sharpe communicated the descriptions of two new species of birds recently procured by Mr. H. T. Ansell, of Gaboon; these were proposed to be called *Centropus anselli*, and *Dryoscopus coronatus*.

Chemical Society, March 19.—Prof. Odling, F.R.S., president, in the chair.—On Dissociation, by Prof. Dewar. The lecturer premised that as he had but little that was new to tell, he must content himself with condensing and epitomising the results of others. After briefly referring to the theories of Priestly and Hutton, he described the famous experiments of Sir James Hall, who obtained a substance identical with marble by fusing carbonate of lime under pressure. He next noticed Grove's discovery that water was decomposed at a temperature lower than that produced by the union of oxygen and hydrogen, and then explained the masterly researches of Deville on the effect of heat in causing the dissociation of carbonic anhydride, carbonic oxide, water, &c. After this the lecturer showed that in dissociation the tension of the vapour evolved is constant for a given temperature and independent of the mass, illustrating it by Debray's experiments on the decomposition of carbonate of lime at a regulated heat, and the evolution of water from certain hydrated salts. The lecture, which was illustrated with diagrams of various curves of tension, concluded with some remarks on the dissociation of the compound of hydrogen and palladium, and with a description of an apparatus devised by the speaker for ascertaining the temperature produced by the explosion of a mixture of oxygen and hydrogen under various pressures.

Meteorological Society, March 18.—Dr. R. J. Mann, president, in the chair.—Mr. R. H. Scott, F.R.S., read a paper On an attempt to establish a Relation between the Velocity of the Wind and its Force (Beaufort scale), with some remarks on anemometrical observations in general. The author stated that he considered that the existing scales of wind force were unsatisfactory. The highest pressure corresponding to force 6 of the land scale was 36 lbs. per square foot, whereas pressures of above 40 lbs. had frequently been registered. He further brought forward proofs of the irregularity in the distribution of such high pressures. He then spoke of the Beaufort scale, and pointed out some of its defects, but stated that speaking generally it might be considered to be a rough classification of the wind force, exact enough for practical purposes, and proceeding by nearly equal degrees. He had recently made experiments at Holyhead and at Yarmouth to test the velocity recorded by the anemometer at each station at the hours when the several figures of the Beaufort scale were reported. The result was a scale which agreed very closely with that given by Schott, as a deduction from theory in his discussion of the observations made by Sir F. Leopold M'Clintock in the *Fox*, and published by the Smithsonian Institution. Inasmuch as the accordance of practice with theory was very great, he proposed this scale for general adoption—

Force.	Miles per hour.	Force.	Miles per hour.
0	2·5	7	40·5
1	8	8	48·5
2	13	9	56·5
3	18	10	65
4	23	11	75
5	28	12	90
6	33·5		

The paper then went on to point out from experience gained at Holyhead, Yarmouth, and Falmouth, the very serious discrepancies which had been proved to exist in the records of velocity for the various points of the compass, especially at Yarmouth, and which showed that the influence of local situation, not only as to the contour of the country, but even the very shape and height of the observatory and the adjacent buildings, exercised a most serious influence on the correctness of the data afforded by the instruments. It therefore seemed very dangerous to reason as to the mean motion of the air over the British Isles from the anemometrical records of one or two stations, as has been done by Dove.—The next paper read was by Mr. G. J. Symons, On the Sensitiveness of Thermometers, in which he gave the results of a series of comparisons of the speed with which thermometers with bulbs of various sizes took up the true temperature to which they were exposed. Three series of thermometers were used, a set with spherical bulbs filled with mercury, and varying in diameter from a quarter to three-quarters of an inch. The result was that the small bulb took up the true temperature in about three minutes, while the large bulb took three times as long; a second set were similar in form, but filled with spirit; they were more sluggish, but the small spirit ones were more prompt than large mercurial ones. Lastly, the new patterns of spirit minimum thermometers introduced by Mr. Casella and Mr. Hicks were tested and found as sensitive as ordinary mercurial thermometers. The instruments were all examined by the Fellows at the close of the meeting.—The last paper was by Mr. R. Strachan, On the Weather of Thirteen Autumns.

Royal Astronomical Society, March 13.—Prof. Adams, F.R.S., president, in the chair.—On an occultation of Neptune observed at Walthamstow on April 24, by Mr. Talmage. The planet was seen to skirt along the moon's limb, and was only occulted for a few seconds. The occultation was also watched for at Greenwich by Mr. Criswick, and although the difference of latitude only amounts to a few miles, the planet was never lost sight of.—On a remarkable structure visible upon the photographs of the solar eclipse of December 12, 1871, by Mr. Ranyard. In viewing the photographs by transmitted light a minute partially transparent spot can be traced at a height of about 9′ from the eastern limb on all the negatives of Lord Lindsay's series, and on four out of the six negatives of Col. Tennant's series. It appears to occupy identically the same place with regard to the dark details of the corona in all the photographs, and cannot therefore be due to any reflection within the camera, for the position of the corona is shifted upon the different plates. On first making the discovery, he had been inclined to think that it must be due to a star seen through the corona, but on further reflection he had been obliged to abandon that idea, for a star would have been represented by a dark or opaque point, whereas this must be due to an object darker than the corona, apparently hiding or cutting out some of its light. On a closer examination of the negatives, with suitable lights, three partially transparent circular arcs concentric with the bright point were detected above it. Such forms are totally different from the corona structure visible on other parts of the plate, and there seemed no alternative but to suppose that they were due to some partially opaque body situated between us and the sun, cutting out or partially intercepting the light of the corona. The structure is similar to that which has often been observed in the nuclei and concentric comæ of comets, and Mr. Ranyard thought that it did not seem unreasonable to suppose that this was really a photograph of a faint though large comet near to perihelion. Mr. Christie said that he had examined the negatives and he did not think there could be any doubt about the existence of the structure. It was distinctly to be traced on Lord Lindsay's series, and also on those taken 120 miles away at Ootacamund by Col. Tennant.

Entomological Society, March 16.—Sir Sidney Smith Saunders, president, in the chair.—Mr. Champion exhibited specimens of *Euryporus picipes* taken near Chatham.—Mr. Edward Saunders exhibited a box of *Buprestidæ* collected by Prof. Semper in the Philippine Islands; and read some notes and descriptions of the new species.—A paper was communicated by Prof. Westwood on several additional species of *Lucanidæ* in the collection of Major F. J. Sidney Parry.

Geologists' Association, March 6.—Prof. Morris, F.G.S., vice-president, in the chair.—On the geology of the Nottingham district, by the Rev. A. Irving, F.G.S. The district under aonsideration comprises coal-measures, Permian, Bunter, Keuper, and Lias rocks—a border-land between the Palæozoic and Mesozoic epochs. No apparent unconformability exists between the Permian and Triassic series here; while that between the Permian and coal-measures is enormous. (1) *Coal Measures.* There are seven seams of coal at present workable in this field, with many more of inferior quality. The enormous unconformability between the coal-measures and the Permian is shown by the fact that at the Shire Oak Colliery near Worksop, 1,300 ft. of coal-measures are passed through before the "top-hard" is reached, whilst at Stretley, twenty miles to the south, the magnesian limestone rests directly (according to Mr. G. Fowler, C.E.) upon the "top-hard" seam. (2) *The Permian.*—The great unconformability between the Permian rocks and the coal-measures is rendered more significant by the absence of the Lower Red Sandstone (Rothliegende), whilst there are clear proofs of continuous deposition of the Permian and Lower Bunter. In this area stratigraphical evidence points to the Permian and Bunter as but portions of one great unbroken sequence of rocks deposited upon highly disturbed and denuded coal-measures. (3) The *Bunter.*—The Lower Mottled Sandstone is nowhere more than 100 ft. thick. The Himlack stone exhibits the junction of the Lower and Middle Bunter. It is marked by unconformability. A bed of calcareous grit and breccia forms the basement of the pebble beds, or Middle Bunter. This is evidently a shore formation. The author concluded, from its composition and from the general prevalence of current bedding, that it occupied an area of deposition subject to shifting currents, but protected from the open ocean. (4) *Keuper.*—Two sections were given where the "water-stones," consisting of alternating beds of sandstone and marls, are seen resting upon the eroded surface of the bunter. In each case the junction is marked by a bed of highly calcareous breccia; and there is unconformability between the two formations. Footprints of *Cheirotherium* have been observed at Castle Donnington, and recently by the author at Colwick, near Nottingham. Ripple marks, &c., are also commonly met with. (5) The *Rhœtic beds.*—The black paper shales were discovered by Mr. Etheridge a short time ago at Elton; there also the author has found a portion of the bone-bed. (6) The *Lias* may be observed capping the hills on the south side of the Trent Valley. Belvoir Castle crowns an escarpement of the Middle Lias (marlstone), abounding in *Rhync. tetrahedra* and *Ter. punctata.* (7) *Drift and Alluvium.*—The greater part of the former appears to have been long since carried down into the valley of the Trent, where extensive gravel-pits are worked, as *e.g.* at Saveley and Beeston.

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Academy of Sciences, March 16.—M. Bertrand in the chair. The following communications were read:—Note on the employment of flexible laminæ for the tracing of arcs with curvature of large diameter, by M. Resal.—Researches on symmetrical isomerism and on the four tartaric acids, by MM. Berthelot and Jungfleisch. The authors have determined the heat of solution of dextro-tartaric acid, lævo-tartaric acid, racemic acid, and inactive tartaric acid. The authors think it probable from their researches that water decomposes the inactive acid into its two active constituents during the act of solution.—On the crystalline hydrates of sulphuric acid, by M. Berthelot; also a thermo-chemical communication.—Experimental researches leading to a determination of the sun's temperature: a letter from P. Secchi to the perpetual secretary. The author has compared the solar radiation with that of the electric arc from a battery of 50 Bunsen's elements, using for this purpose his "thermo-heliometer." After making necessary corrections for atmospheric absorption the result obtained is 133780°, but the author considers this number only an approximation, and considers it possible that it may have to be raised to 169680°.—Report of the geodesic work relating to the new determination of the French meridian, by M. Elie de Beaumont.—Memoir on the swim-bladder from the point of view of station and locomotion, by M. A. Moreau.—On an application of the theory of substitutions to linear differential equations, by M. C. Jordan.—On the heat of combustion of different varieties of red phosphorus; a note by MM. Troost and Hautefeuille.—On the conditions which determine the movements of chlorophyll granules in the cells of *Elodea canadensis*; note by M. E. Prillieux. By a microscopical examination the author has sought to distinguish clearly in the example chosen the movements which are affected by light from those produced by lesion of the tissues during the act of preparation for microscopic examination.—The blocks and rolled flints in the Red Sandstone ot the drift of Saint-Brieuc; note by M. T. Héna. These flints appear to have been brought from Erquy, 24 kilometres to the north-east of Saint-Brieuc by means of floating ice.—On the laws of the plane distribution of pressures in the interior of the isotropic bodies in the state of limited equilibrium; note by M. J. Bossinesq.—On the friction of glaciers and the erosion of valleys, by M. C. Grad. The author expresses his belief that neither the Alpine valleys, the Italian and Swiss lakes, nor the fjords of Norway and Greenland owe their origin to glacial erosion.—Chemical nature of the sulphide of iron (troïlite) contained in meteoric irons, by M. S. Meunier. A reiteration of the view, formerly expressed by the author, that this substance is a variety of pyrrhotine (Fe_7S_8) and not simply a ferrous sulphide (FeS).—On a phosphate of cerium containing fluorine, by M. F. Radominski. This mineral contains cerium, lanthanum and didymium, calcium, magnesium, iron, fluorine, phosphoric acid and traces of water. It was found near Fahlun in Sweden. During the meeting M. Gosselin was elected into the section of medicine and surgery to supply the vacancy caused by the death of M. Nelaton.

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