THURSDAY, AUGUST 1, 1895.

## LINEAR DIFFERENTIAL EQUATIONS.

*Handbuch der Theorie der linearen Differentialgleichungen.* Von Prof. Dr. Ludwig Schlesinger, Privatdocenten an der Universität zu Berlin. Erster Band. (Leipzig: Teubner, 1895.)

DE MORGAN is reported to have said of the subject of differential equations, that it illustrated the proverb that he who hides knows how to find. This was true enough at a time when the sole aim of the analyst was to "solve" differential equations by reducing them to quadratures, or to construct ingenious puzzles for the benefit of undergraduates. Integration by series was known, of course; but this was regarded as a mean device, useful indeed for purposes of calculation, especially to the physicist, but unworthy of the serious attention of the pure mathematician.

A new era began with the foundation of what is now called function-theory by Cauchy, Riemann, and Weierstrass. The study and classification of functions according to their essential properties, as distinguished from the accidents of their analytical forms, soon led to a complete revolution in the theory of differential equations. It became evident that the real question raised by a differential equation is not whether a solution, assumed to exist, can be expressed by means of known functions, or integrals of known functions, but in the first place whether a given differential equation does really suffice for the definition of a function of the independent variable (or variables), and, if so, what are the characteristic properties of the function thus defined. Few things in the history of mathematics are more remarkable than the developments to which this change of view has given rise. Leaving out of account the theory of partial differential equations, which is still beset with many and serious difficulties, it is not too much to say that in the course of less than half a century the theory of ordinary linear differential equations alone has attained a degree of extent and importance which makes it comparable with almost any of the main branches of analysis.

The landmarks of the new departure are the memoir of Briot and Bouquet in the *Journal de l'École Polytechnique* (cap. 36), Riemann's paper on the generalised hypergeometric series, and Fuchs's memoir in the sixty-sixth volume of *Crelle's Journal*. Since the publication of this last work, more especially, the progress made has been exceedingly rapid: the general principles of the subjects have been permanently established, so as already to admit of methodical treatment, and numerous particular applications, all of great interest and beauty, have attracted and continue to invite the attention of mathematical explorers. Thus there is the problem of discovering whether a given equation has an algebraic integral, and, if so, of finding it; there is the theory of equations with doubly periodic coefficients; and there is the theory of differential invariants. Each of these is associated with some of the most brilliant discoveries of modern analysis, and each offers abundant opportunity for further research.

The wide extent of the subject, and the immense number of memoirs relating to it, have created an urgent need for systematic treatises to serve as an introduction to the theory, and presenting its main outlines in a proper perspective. Fortunately this want seems likely to be supplied before long; various excellent works, dealing wholly or in part with linear differential equations, have recently appeared or are in course of publication, and among these the book now under review will take an honourable place.

Dr. Schlesinger's work, to be completed in two volumes, is intended to give a coherent and comprehensive account of the theory in the light of its most recent developments. This first volume is divided into eight sections, exclusive of two introductory chapters, one historical, the other treating of the existence of an integral, and the general nature of the singular points. Of the eight sections, the first contains the first principles of the theory, mostly after Fuchs; the second discusses systems of independent integrals, reduction when particular integrals are known, Lagrange's adjoint equation, non-homogeneous equations, and Frobenius's theorems on irreducibility; the third relates to the fundamental equation; the fourth to unessential singular points; the fifth to equations of the "Fuchsian" class, that is to say, of which the coefficients are rational functions of $x$ and all the integrals are regular; the sixth treats of the development of integrals within a circular annulus; and, finally, the seventh and eighth contain the general theory of equations with rational coefficients.

The treatment is entirely analytical, and is based principally on the methods of Weierstrass as expounded by Fuchs, Frobenius, Hamburger and others; thus the integrals are obtained in the form of power-series valid within a certain region of the plane of the complex variable, and no use is made of geometrical diagrams such as those employed by Schwarz, Klein, and Goursat. Moreover, except in the fifth section, which contains a brief discussion of Riemann's P-function and of the hypergeometric series, the author confines himself to the *general* theory, and does not consider special cases, or particular applications. The demonstrations, for the most part, are concise, and free use is made of the sign of summation and double suffixes. For these reasons the book is perhaps hardly suitable for those who are approaching the subject for the first time; but any one who has read, let us say, Goursat's thesis on the hypergeometric series, or Klein's lectures on linear differential equations of the second order, and is moderately familiar with the Weierstrassian function-theory, will be able to consult it with advantage. To those who are engaged in research, Dr. Schlesinger's treatise will be of great value, because those parts of the subject which are included within the author's plan are discussed with sufficient thoroughness, with a consistent notation, and in logical order; while the analytical table of contents gives references to the original sources in direct connection with the articles of the book which are based upon them. It is rather a pity, by-the-by, that the dates have not always been given in these references; the reader may very possibly wish to know the date of a paper, and not be able to consult the volume of the journal in which it appeared.

Mathematicians will look forward with interest to the appearance of the concluding volume of the treatise, which will contain, *inter alia*, a discussion of the group of an equation, and of the classification of equations according to the nature of the groups belonging to them. Until the work is complete, it is premature to express an opinion as to the degree of success with which the author has attained the object he has in view; but there can be no doubt of the valuable service which is rendered to science by the composition of a methodical treatise like this. So far as we are able to judge, account has been taken of all the most important researches which come within the scope of the present volume; the three last sections, in particular, include an account of the recently published papers of Helge von Koch, Poincaré, and Mittag-Leffler.

The proof-sheets appear to have been very carefully revised, so that the book is happily free from the crowd of misprints with which mathematical text-books, otherwise excellent, are not unfrequently disfigured. G. B. M.

## *THE RESEARCHES OF TESLA.*

*Inventions, Researches, and Writings of Nikola Tesla.* By Thomas Commerford Martin. (New York: *The Electrical Engineer*, 1894.)

WE have here an account of Nikola Tesla, his scientific inventions and work, by a devoted admirer. Mr. Martin is not a Boswell, and from the nature of the case his book could hardly have about it all that human interest which pervades the life and achievements of a veteran discoverer in science. Mr. Tesla is a young man whose career has been somewhat romantic, and whose ingenuity is such as to rank him very high indeed among the electrical workers and discoverers of the day. Born in Austro-Hungary, educated at the Realschule at Carstatt and the Polytechnic at Gratz, and professionally first in the Government Telegraph Department, and afterwards in Paris, his career as an engineer really began when he arrived in America little more than ten years ago.

In two or three years from the day on which he took off his coat in the Edison Works, Tesla motors had attracted attention, and he leaped at once to a position as a successful experimenter and inventor, which his subsequent work has only secured and made more important. His researches on the effects of alternating currents of high potential and frequency, in particular, though they had the misfortune to be made the subject of the speculations of the ordinary journalist, are of great scientific interest, and continued by Mr. Tesla himself and the army of enthusiastic workers we now have, cannot fail to yield theoretical results and practical applications which will more than fulfil the anticipations of those who took a sober and rational view of their possibilities. None of those who listened to Mr. Tesla at the Royal Institution will soon forget the almost marvellous experiments performed, their clear exposition in what was to the lecturer manifestly a foreign language, and the enthusiasm which the results displayed excited in those present who were best able to judge of their scientific interest and importance.

Mr. Martin's account of Mr. Tesla's work is interesting, and yet perhaps it might have been in some respects better than it is. He has had excellent materials, such as the various lectures delivered by Mr. Tesla on his researches generally, the papers read from time to time to scientific societies on particular inventions and points of interest, and apparently the specifications of Mr. Tesla's patents. Our complaint, if we have one, is that this material has hardly been sufficiently worked up. Many of the lectures and papers were, as was inevitable, hurriedly composed, and the expression of Mr. Tesla's theoretical views contained in them is not always so clear and complete as it might have been made by one not so rapidly carried forward by the stream of discovery. A great inventor can hardly be expected to spend time weighing words and phrases, at any rate he has a title to be excused from doing so, which others who expound him do not possess. As it is, Mr. Martin's book is on the whole a reproduction of articles which appeared from time to time in the *Electrical Engineer* (of New York), and all we wish is that he could have spared the time and trouble necessary to cast the matter into a more homogeneous and symmetrical form.

For the lectures which are reproduced we are very grateful. They give Mr. Tesla's own description of his inventions, and his views on points of theory—views, which if not always orthodox, and sometimes expressed in language which appears strange, are always fresh and suggestive. The unavoidable repetitions of the same ideas, and recurring descriptions of the same apparatus, are not without some advantage, though they interfere with the unity of Mr. Martin's book, as they enable the lecturer's meaning to be made out more completely than would otherwise be possible.

The book is divided into four parts: Polyphase currents; Tesla effects with high frequency and high potential currents; miscellaneous inventions and writings; early phase motors and the Tesla oscillators. The two first parts are of course much more interesting than the remaining two, which have to do with such things as oil condensers, anti-sparking dynamo brushes, unipolar generators, the Tesla exhibit at the World's Fair, and the Tesla mechanical and electrical oscillators.

The discussion of polyphase currents, which occupies the first 115 pages of the book, has more unity of treatment about it than the second part, which consists mainly of the lectures Mr. Tesla delivered in this country and America. After a short introductory and biographical chapter, Mr. Martin proceeds to expound the principle of the rotating magnetic field and the construction of synchronising motors. A paper by Tesla, on a "New System of Alternate Current Motors and Transformers," is reproduced in this connection, and contains the foundation on which is based the remaining twenty-one chapters which make up Part i. These contain numerous modifications of the original idea, many of them exceedingly ingenious. A motor "depending on 'magnetic lag' or hysteresis" is described in Chapter xii. The peculiarity of this is stated in an introductory paragraph to be "that in it the attractive effects or phases, while lagging behind the phases of current which produce them, are manifested simultaneously and not successively." This statement itself seems to want some little exposition, though the arrangement is really very simple. An iron disc is pivoted within a fixed coil, wound just large enough to admit the diameter of the disc one way, and a little more than its thickness the other. The coil carries two pole-pieces, one at each end, which project from opposite sides a little way round the disc. Thus opposite poles are stretched out as it were from the coil round the disc in the same direction. An alternating current passed round the coil magnetises both these pole-pieces and the disc, and the repulsion between the adjacent similar polarities of the disc and pole-pieces produces the rotation, the polarities of both being of course reversed with the current. The disc is wound with closed coils, so that the induced currents augment the turning couple developed. This arrangement is further developed into a "multipolar motor"; but in neither case is there any clear statement of how the action depends on hysteresis.

In connection with these and similar devices it would have been interesting to have had some estimate of efficiency, but generally speaking, in no part of the book is there any discussion of this most important question. Indeed, when the word energy is used it seems to bear a somewhat peculiar sense. For example, at p. 81 we have a statement as to the "energies" of the field and the armature, and the importance of these being equal if for a given sum the motor is to have the greatest efficiency. This passage is a little difficult of interpretation, if the word energy is to be taken as it ought to be in its technical sense throughout, though it is not very hard to make out the idea intended.

By far the most interesting portion of the book to a student of electricity generally is Part ii. The alternator of high frequency which Mr. Tesla used is fully described, and the arrangements for using it explained in the first of the lectures already referred to. The phenomena produced are set forth in the remaining chapters with numerous illustrations which render the descriptions very easy to follow. The whole subject of high frequency phenomena is very intimately connected with the researches of Hertz on the one hand, and the work of Mr. Crookes on the other, and forms a most inviting field of research for experimentalists who possess the necessary equipment. Whether always the theoretical view taken by Mr. Tesla is correct, is matter for legitimate difference of opinion. For one thing, we do not think that there is any difference at all between electric force produced by what is properly called electrostatic action and that produced by electro-magnetic action. The distinction is only mathematical—the former force can be derived from a potential function, the latter cannot—and in a sense only expresses our ignorance of the mode of production of the force. But perhaps we are mistaken in supposing that Mr. Tesla regards the electric forces in these two cases as different in nature.

To every physical inquirer the perusal of these lectures cannot but be of the greatest benefit. It will again remind him that the field of research is unlimited, and quicken his scientific enthusiasm, if not to taking part in the work of this particular part of it, to at least prosecuting with renewed vigour the inquiry, whatever it is, which lies ready to his hand.

It was reported a few weeks ago that all the apparatus and machinery belonging to Mr. Tesla had been destroyed by fire. Every reader of his researches must sincerely sympathise with Mr. Tesla in his loss of valuable appliances and still more valuable time. That he at once set himself to repair the loss is only what was to be expected from his character; let us hope that it may result in such improvements of his means of experimenting as may, in some measure at least, make up for his disappointment, if it is not, what is perhaps too much to suppose, turned into a blessing.

A. GRAY.

## OUR BOOK SHELF.

*An Introduction to Chemical Crystallography.* By Andreas Fock, Ph.D., translated and edited by William J. Pope, with a preface by N. Story-Maskelyne, M.A., F.R.S. Pp. 189 and xvi. 8vo. (Oxford: Clarendon Press, 1895.)

THIS little book is issued by the Clarendon Press as a companion volume to Maskelyne's "Morphology of Crystals," which was recently reviewed in these columns. It is far from being a mere translation of Fock's "Einleitung in die chemische Krystallographie," which was published in 1888. That book contained a useful summary of the leading facts known about the origin and growth of crystals, and the general relations between their chemical composition and other properties, especially as regards isomorphism and the properties of mixed crystals. All this is contained in the present volume, which is, moreover, less sketchy than the earlier book, and the somewhat numerous inaccuracies which disfigured the German edition have been corrected. But it is in the additional matter that the chief alteration is to be found. About fifty pages have been introduced, containing a survey of those important contributions to our knowledge of crystals which have recently been made from the side of physical chemistry; the remarkable theoretical researches of Van t'Hoff and Willard Gibbs, and the quite recent experimental investigations of Bakhuis Roozeboom, to which they gave rise, are here very happily summarised and brought within the reach of the English elementary student.

In order to give a comprehensive survey of the origin and growth of crystals, it is necessary to take into account the properties of the solutions from which they separate, and several chapters are accordingly devoted to such subjects as the relations between osmotic pressure and concentration, the separation of double salts and those containing water of crystallisation, the conditions of equilibrium in a solution containing various *solutes* (to employ a convenient word suggested by Prof. Maskelyne in his preface as a term for the substances dissolved), and the resulting variations in the isomorphous mixtures which crystallise from such solutions; all these are subjects of great importance, which have up to the present time met with no adequate treatment in English text-books.

A treatise which merely summarises without criticism loses much piquancy and interest, and also some value as a guide to students. This objection may fairly be urged against Fock's book, which appears to accept without question all the observations reported by the author. It would have been better, for example, to indicate the insecure nature of some of the evidence which rests only upon microscopical observation, such as that of Lehmann and Vogelsang.

This book remains, nevertheless, an excellent survey of chemical crystallography, brought fully up to date, and one which will, we hope, open the eyes of English chemists to a new field of work.

Mr. Pope's translation is both fluent and accurate; he is further responsible for some of the new matter introduced into this edition. The book is lucid, readable, and interesting, and is one which does credit to the Clarendon Press.

*Laboratory Exercises in Botany.* By Prof. Edson S. Bastin, A.M. (Philadelphia: W. B. Saunders, 1895.)

FOR a laboratory manual this book is of great extent, for it includes more than 500 octavo pages, with no less than 87 plates. Yet it is more remarkable for what is omitted than for what is contained in it.

The first half of the book is devoted to organography, and consists of descriptions of the gross structure of a number of types of flowering plants, fully illustrated in the first 37 plates. This part of the book seems to us decidedly well done.

The second half, with 50 plates, is on vegetable histology. Strange to say, it deals simply and solely with the *vegetative* structure of phanerogams and vascular cryptogams. This branch of the subject is illustrated in great detail, and the anatomical work is sound, if not quite up to the highest modern standard.

Not a word, however, is said as to reproduction, development, or life-history. The words *pollen-tube*, *ovule*, *embryo-sac*, *archegonium*, *antheridium*, and *growing-point*, are sought in vain in the index, nor have we found any reference to them in the text, except that ovules are of course mentioned in the descriptive part. In fact, just those subjects which are most important in a scientific course of laboratory work are entirely passed over. The utter absence of any account of the lower cryptogams is also astonishing, for there is no indication that a second volume may be looked for.

The author is professor at a pharmaceutical college, and this fact may help to account for the extraordinary unevenness with which he has treated his subject. Students of pharmacy in America are no doubt required to have some acquaintance with the external characters of the higher plants, and some anatomical training may also be expected of them, with a view to the identification of drugs. Beyond this it would appear that their botanical education is not meant to go. The author has expended great pains on his work, but its manifest one-sidedness renders it quite valueless as a scientific guide to laboratory botany. Students of pharmacy in England are happily accustomed to a very different system of botanical teaching.

D. H. S.

*The Source and Mode of Solar Energy.* By I. W. Heysinger, M.A., M.D. (Philadelphia: J. B. Lippincott and Co., 1895.)

ON the strength of an acquaintance with popular astronomical literature, in many cases not up to date, the author of this work offers a theory which he states to be capable of interpreting all the phenomena presented to us in the heavens. Briefly, we are asked to believe that all interstellar space is filled with attenuated water vapour, and that this vapour is decomposed into its constituents by the electricity generated by the movements of planetary bodies; the oxygen remains on the planets, while the hydrogen goes to maintain the incandescence of the central suns. The author deals very ingeniously with many of the apparent difficulties, such, for example, as the absence of an atmosphere from the moon; but his anxiety to leave nothing unexplained, has occasionally demanded other assumptions, and led to self-contradictions. Thus, in regard to comets, it is necessary to suppose, from the repulsion of the tails, that when they enter our system, they do not behave electrically as planets do, but like suns, and so they should have hydrogen atmospheres; on the other hand, since carbon is assumed to be a "planetary" element (p. 69), they should not contain carbon. This is in complete contradiction with the facts. The author is so much behind the times in spectroscopic matters as to imagine that nebulæ abound in free nitrogen, and possibly oxygen, and that free nitrogen and hydrogen are characteristic of comets. It would serve no good purpose to discuss a theory based on such misconceptions.

## LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

### The Huxley Memorial.

I TRUST you will allow me through the medium of your columns to make it known that at the meeting of the Provisional Committee, which was held at the rooms of the Royal Society on Tuesday afternoon, it was announced that a large number of acceptances had already been received to the invitation which was issued a few days ago to a number of gentlemen to serve on the General Committee which it had been decided to form to inaugurate a National Memorial to the late Right Hon. T. H. Huxley, F.R.S.

A list of the Committee will shortly be published.

Owing to the lateness of the season, it has been decided to defer until after the autumn recess the meeting of the General Committee, at which the proposals of the Provisional Committee with regard to the form which the National Memorial shall take may be discussed and decided.

With a view of assisting the Provisional Committee in arriving at some general ideas on the subject, it is suggested that those who propose to contribute to the fund might be willing to inform the Treasurer of the probable amount of their subscriptions.

Subscriptions will be received and acknowledged by advertisement in *The Times* by the Treasurer, Sir John Lubbock.

J. D. HOOKER,
Chairman of the Provisional Committee.

July 30.

### The Kinetic Theory of Gases.

WE shall all agree with Dr. Boltzmann's views as expressed in NATURE of July 4, that if in a system of elastic sphere molecules the free paths be very long, and if at the same time the system be of unlimited extent, condition A will always be satisfied. The system will go on till it attains Nirvana in the Maxwell-Boltzmann distribution.

It is only for a finite system that it appeared to me that occasional disturbances from the outside were necessary to produce this result. I agree with Mr. Bryan that contact with the refrigerator or with the reservoir, such as is supposed to take place in thermodynamics, is for this purpose a disturbance.

But it is this very length of free path, and condition A which follows from it, that restricts our kinetic theory to the limiting case of a rare gas.

We have, as I maintain, to abandon condition A altogether if we wish to present our theory in a form applicable to dense media. We must consider, not single spheres, but groups of spheres to begin with. Given that there are at this instant $n$ spheres, and no more within a spherical space S, but nothing is known of their position within S, what is the chance that their component velocities shall at this instant be

$$u_1 \ldots u_1 + du_1 \quad \ldots \quad \ldots \quad \ldots \quad w_n \ldots w_n + dw_n ?$$

I assume that chance to be

$$C\epsilon^{-hQ} du_1 \ldots dw_n,$$

in which $Q = a\Sigma(u^2 + v^2 + w^2) + b\Sigma\Sigma(uu' + vv' + ww')$, the summation including the $n$ spheres and every pair of them. The coefficient $b$ excludes condition A.

But whatever be the values of $a$ and $b$, this distribution of velocities remains undisturbed by collisions. And by suitably choosing $a$ and $b$, we can satisfy all other necessary conditions.

The same thing can be done for two sets of spheres of unequal masses $m$ and $m'$. In that case we must put Q in the form

$$Q = a\Sigma(u^2 + v^2 + w^2) + a'\Sigma(u'^2 + v'^2 + w'^2)$$
$$+ b\Sigma\Sigma(u_p u_q + v_p v_q + w_p w_q) + b'\Sigma\Sigma(u'_p u'_q + v'_p v'_q + w'_p w'_q)$$
$$+ \beta\Sigma\Sigma(uu' + vv' + ww'),$$

in which the accents $'$ refer to the $m'$ set, and $\Sigma\Sigma u_p u_q$, &c., means summation over all pairs of spheres $m$, &c.

Here we have five coefficients, $a$, $b$, $a'$, $b'$, $\beta$. But the condition for permanence, notwithstanding collisions between $m$ and $m'$, requires

$$2am' - 2a'm + \beta(m' - m) = 0$$

$$b = \frac{m}{m'}\beta \qquad b' = \frac{m'}{m}\beta,$$

three conditions reducing the five coefficients to two independent ones. It will be found that $m\overline{u^2} = m'\overline{u'^2}$, as in the ordinary theory.

I doubt not that Boltzmann's minimum theorem can with some modification be applied to this system, at all events if he will take up the theory of dense gases himself.

S. H. Burbury.

## On Skew Probability Curves.

In a memoir, entitled "Contributions to the Mathematical Theory of Evolution. II. Skew Variation in Homogeneous Material" (*Phil. Trans.* 186, A, pp. 343–414), and noticed in your columns by Mr. Francis Galton (January 31, 1895), I have dealt with four types of skew frequency curves.

Last Tuesday, Prof. Edgworth drew my attention to the fact that a portion of my results has been anticipated by Mr. E. L. De Forest in vols. vi., ix., and x. of *The Analyst*, an excellent American mathematical journal, the acquaintance of which, I am ashamed to say, I have only to-day made for the first time.

So far as Mr. De Forest's priority is concerned, it covers the special class of curve I have in my memoir termed Type III. He has fully worked out the geometry of this type, and I consider his deduction of it, if somewhat more lengthy than mine, to have the advantage of greater generality. So far as my own memoir is concerned, a knowledge of Mr. De Forest's memoir would not have led me to rewrite pp. 373–6 of mine, which deal with this type, because my discussion there is only a branch of my general treatment of a series of skew frequency curves. I should, however, have referred to Mr. De Forest's priority and the excellency of his work. In particular I should have cited the whole of his numerical table iii. x. p. 69, which gives the values of the frequency in excess and defect of the mode, and the probable errors in excess and defect, for a considerable range of values. These results are only given by algebraic or empirical formulæ in my paper. The statisticians among your readers, who may be proposing to deal with skew frequency, would find a copy of Mr. De Forest's Table III. of considerable service should they come across a curve of Type III.

Karl Pearson.

University College, London, July 24.

## Evolution, or Epigenesis?

In the English translation of Prof. Hertwig's book "The Cell," it is stated (p. 295), "When the female gamete of the Alga *Ectocarpus* comes to rest, for a few minutes it becomes receptive. If the egg is not fertilised at this time . . . parthenogenetic germination begins to make its appearance . . . It may be accepted as a *law of nature* (italics mine) for mammals, and for the majority of other organisms, that their male and female sexual cells are absolutely incapable of development by themselves." Thus, what occurs in the lower organisms is no criterion of what occurs in the higher, and *vice versâ*. Then why does Hertwig remark (p. 348), "It is quite sufficient for our purpose to acknowledge, that in the plants and lower animals, all the cells which are derived from the ovum contain *equal quantities of the hereditary mass*. . . . All idioblasts must divide and must be transmitted to the daughter-cells, in *equal proportions both as regards quality and quantity*" (italics mine). According to the above, it is "quite sufficient" for Hertwig's purpose of discrediting Weismann's contention for differentiated distribution of hereditary elements among somatic cells, to show that there is undifferentiated distribution in the case of plants and lower animals. But, reverting to the earlier quotation, if it is not sufficient to prove sexual reproduction in the case of the higher organisms, in order to disprove parthenogenesis in the case of the lower organisms, why should it be "quite sufficient," in order to disprove distribution through germ-cells, in the case of the higher organisms, to show that, in plants and the lower animals one cell contains the same hereditary constituents as another? It is permissible to infer that differentiation in regard to germ-cells, in the higher animals, is no more disproved by the assumed demonstration that, in plants and the lower animals, there is no such differentiation, than that asexuality in lower is disproved by sexuality in higher organisms. Weismann, in my opinion, has proved to rational satisfaction that differentiation of germ from other cells must occur in the higher organisms, and he has offered a rational explanation, conformable with the theory of germ-plasm, of the apparently summational distribution of hereditary elements through somatic cells. Until Weismann's position is seriously undermined, which, so far, is not even a likely contingency, we must decline to accept Hertwig's assumed demonstrations in regard to plants and lower animals as invalidating the theory of germ-plasm. Similarly, that environment may affect the hereditary character of a primitive organism is no more evidence that it may so affect a mammal, than sexuality in the latter is evidence against parthenogenesis in the former. On page 348 we are told: "Johannes Müller has raised the question, 'How does it happen that certain of the cells of the organised body, although they resemble both other cells and the original germ-cell, can produce nothing but their like, *i.e.* cells which are (in-?) capable of developing into the complete organism? Thus epidermal cells can only, by absorbing material, develop new epidermal cells, and cartilage cells only other cartilage cells, but never embryos or buds.' To which he has made answer: 'This may be due to the fact that these cells, even if they possess the power of forming the whole, have, by means of a particular metamorphosis of their substance, become so specialised, that they have entirely lost their germinal properties, as regards the whole organism, and when they become separated from the whole, are unable to lead an independent existence.'" The above is simply a restatement of Weismann's doctrine regarding the origin of germ-cells. All cells which have not, as Müller states, "lost their germinal properties, as regards the whole organism," are Weismann's germ-cells.

So far as regards the essential question of heredity, Hertwig agrees with Weismann. Special units (idioblasts) are the bearers of hereditary qualities. This is "evolution," and no superstructural epigenetic thesis attributing modifying effects by environment, as the cause of a somatic cellular development, can affect the point that differentiation, through hereditary units, is the fundamental condition of morphological development. To accept "hereditary units," in my opinion, excludes "hereditary effect through environment," never mind to what matter-system the latter assumption be applied, whether the systems be, for instance, unicellular organisms or somatic cells. On the other hand, if we accept "hereditary extraneous influence," we need not trouble ourselves with "hereditary units." If "extraneous influences" have hereditary effect, "hereditary units" have no logical existence. All we then need for a theory of heredity are primordial homogeneous matter and environment. Mr. Herbert Spencer's earlier hypothesis, in which he attributed all variation to extraneous influence, would have been logical had he excluded "physiological units." With these, it became illogical. For this reason: if all organic variability depended on the effect of extraneous influences, why should such influences not have produced the differentiations called physiological units? Why should the only logical "unit" not be homogeneous *primordium?* That the conception "hereditary unit" shall be logical, involves that the "unit" shall be as unchangeable as an "atom." If, on the contrary, we have a variable "unit," it is not a genuine "hereditary unit," but merely the equivalent of any later variable "unit." Hertwig's "hereditary units," or "idioblasts" (p. 340), "are the smallest particles of material into which the hereditary mass or idioplasm can be divided, and of which great numbers and various kinds are present in this idioplasm. They are, according to their different composition, the bearers of different properties." They are not indivisible, like atoms, but assimilate food, grow and divide, as do Weismann's "biophors," from which they appear to differ only to the extent that they are complex organisms. The hereditary factor in Weismann's theory which corresponds with these "idioblasts" of Hertwig appears to be the "determinant." All the functions of the latter seem to be performed by the former. These "idioblasts" (p. 343) "must evolve in regular sequence during the process of development." As sentences are formed from words, so are organisms formed from these "idioblasts." We can attain a clear conception of the formation of sentences from words, but Hertwig does not enable us to apprehend how organisms can arise from "idioblasts." As he very truly observes (p. 344), "this portion of the theory is the most difficult to understand."

Hertwig, like Spencer, takes his stand on epigenesis. It may be asked, wherein is the epigenetic character of his (Hertwig's) theory? Unlike Spencer's "physiological units," Hertwig's "idioblasts" are intrinsically differentiated organisms with specific tendencies. Now, for a genuine epigenetic theory, hereditary units must merely compose a plastic mould to take the impress of environment, whereas these "idioblastic" cells are composed of elements with predetermined peculiarities. Accordingly they must function in a predetermined manner, and thus the products of their activity must issue through evolutionary processes; what they will become after millions of generations must be determined so soon as these "idioblasts" combine as the first cell. If, however, we are to assume that the hereditary qualities of "idioblasts" can be eliminated by environment—as we must assume if we attempt to combine evolution with epigenesis—I reply, as in my earlier proposition, we have no need for "idioblasts" or any other "hereditary unit." All we then need for a theory of heredity is some plastic *primordium* and environment. Then, as such *primordium* would have no hereditary predisposition, there would be no room for predeterminism, and it would remain for ingenious theorists in love with epigenesis and the tape-measure system of estimating the cosmos, to explain the persistence of types under variable environment, and the differentiation of types under identical environment.

I can appreciate the eagerness of the "mechanical school" to welcome any loophole of escape from predeterminism. A genuine epigenetic theory is, no doubt, their great desideratum. If they "won't be happy till they get it," I venture to predict that they are doomed to a lengthened spell of dumps! The main issue raised by Hertwig in "The Cell," is: evolution or epigenesis? He tries to accept both, basing epigenesis on evolution. Thereby, in my opinion, he stultifies both doctrines. All biologists, so far as I am aware, start their theories from the basis of differentiated units. Equally they all evade the attempt to account for the differentiation. This omission I have endeavoured to rectify in "Rhythmic Heredity" (Williams and Norgate). H. CROFT HILLER.

### A Sound-producing Insect.

IN your issue of June 13, Mr. S. E. Peal speaks of a lepidopterous insect in Assam which makes a tapping noise when flying. His description so closely resembles an insect in Gorakhpur, that I think it must be identical or closely allied.

The alar expanse is about three inches. The wings are broad, not indented, of a very dark chocolate-brown colour on both sides, with one small yellowish-brown blotch on the costa of fore-wing on upper surface. The body is thin, like a butterfly, but the antennæ are not clubbed. It is apparently a Geometer or slender-bodied Bombyx. It flies in the darkest parts of woods, just as twilight is settling into night, and is very hard to see when standing up. By lying down, so as to get the sky as a background, it is easily visible. It cannot be netted in the ordinary way, as the eye cannot follow it, but by standing still till one is heard near, and then striking in the direction of the sound, one may sometimes be successful. I first succeeded in striking one down with my "solah topi"; afterwards I netted two, and brought them home alive, to see how the noise was made. The sound is a sort of clicking, which may be fairly imitated by striking the nails of the thumb and fore-finger together. From the thorax, between the bases of the wings, a stiff bristle (like a pig's) projects about a quarter of an inch. The noise is made by this bristle catching in the hind-margin of the fore-wings and the costal margin of the hind-wings. I fancy it must be of a warning character, as if the insect is eatable, it would help to enable bats and birds to find it. I think I have noticed that the insect is attracted by imitating the clicking sound with the nails, but could not satisfy myself on this point. J. R. HOLT.

## *A FEW MORE WORDS ON THOMAS HENRY HUXLEY.*

TWO scenes in Huxley's life stand out clear and full of meaning, amid my recollections of him, reaching now some forty years back. Both took place at Oxford, both at meetings of the British Association. The first, few witnesses of which now remain, was the memorable discussion on Darwin in 1860. The room was crowded though it was a Saturday, and the meeting was excited. The Bishop had spoken; cheered loudly from time to time during his speech, he sat down amid tumultuous applause, ladies waving their handkerchiefs with great enthusiasm; and in almost dead silence, broken merely by greetings which, coming only from the few who knew, seemed as nothing, Huxley, then well-nigh unknown outside the narrow circle of scientific workers, began his reply. A cheer, chiefly from a knot of young men in the audience, hearty but seeming scant through the fewness of those who gave it, and almost angrily resented by some, welcomed the first point made. Then as, slowly and measuredly at first, more quickly and with more vigour later, stroke followed stroke, the circle of cheers grew wider and yet wider, until the speaker's last words were crowned with an applause falling not far short of, indeed equalling, that which had gone before, an applause hearty and genuine in its recognition that a strong man had arisen among the biologists of England.

The second scene, that of 1894, is still fresh in the minds of all. No one who was present is likely to forget how, when Huxley rose to second the vote of thanks for the presidential address, the whole house burst into a cheering such as had never before been witnessed on any like occasion, a cheering which said, as plainly as such things can say: "This is the faithful servant who has laboured for more than half a century on behalf of science with his face set firmly towards truth, and we want him to know that his labours have not been in vain." Nor is any one likely to forget the few carefully chosen, wise, pregnant words which fell from him when the applause died away. Those two speeches, the one long and polemical, the other brief and judicial, show, taken together, many of the qualities which made Huxley great and strong.

Among those qualities perhaps the most dominant, certainly the most effective as regards his influence on the world, were on the one hand an alertness, a quickness of apprehension, and a clear way of thinking, which, in dealing with a problem, made him dissatisfied with any solution incapable of rigid proof and incisive expression, he seemed always to go about with a halo of clear light immediately around him; and, on the other hand, that power of foreseeing future consequences of immediate action which forms the greater part of what we call sagacity. The former gave him his notable dialectic skill, and mark all his contributions to scientific literature; the latter made him, in addition, an able administrator and a wise counsellor, both within the tents of science and beyond. These at least were his dominant intellectual qualities; but even more powerful were the qualities in him which though allied, we distinguish as moral; and perhaps the greater part of his influence over his fellows was due to the fact that every one who met him saw in him a man bent on following the true and doing the right, swerving aside no tittle, either for the sake of reward or for fear of the enemy, a man whose uttered scorn of what was mean and cowardly was but the reciprocal of his inward love of nobleness and courage.

Bearing in mind his possession of these general qualities, we may find the key to the influence exerted by him on biological science in what he says of himself in his all too short autobiographic sketch, namely, that the bent of his mind was towards mechanical problems, and that it was the force of circumstances which, frustrating his boyish wish to be a mechanical engineer, brought him to the medical profession. Probably the boyish wish was merely the natural outcome of an early feeling that the solution of mechanical problems was congenial to the clear decisive way of thinking, to which I referred above, and which was obviously present even in the boy; and that it was not the subject-matter of mechanical problems, but the mode of treating them which interested him, is shown by the incident recorded by himself, how when he was a mere boy a too zealous attention to a post-mortem examination cost him a long illness. It is clear that the call to solve biologic problems came to him early; it is also clear that the call was a real one; and, as he himself has said, he recognised his calling when, after some years of desultory reading and lonely irregular mental activity, he came under the influence of Wharton Jones at Charing Cross Hospital. That made him a biologist, but confirmed the natural aptitude of his mind in making him a biologist who, rejecting all shadowy intangible views, was to direct his energies to problems which seemed capable of clear demonstrable proof. In many respects the biologic problems which lend themselves most readily to demonstrable solutions capable of verification are those which constitute what we call physiology; and if at the time of his youth the way had been open to him, Huxley would probably have become known as a physiologist. But at that time careers for physiologists were of the fewest. His master, Wharton Jones, a physiologist of the first rank, whose work in the first half of this century still remains of classic value, had been driven to earn his bread as an ophthalmic surgeon, and an even greater physiologist, William Bowman, was following the same course. There was no opening in physiology for the young student at Charing Cross, and he was driven by stress of circumstances to morphological rather than to strictly physiological problems; but it was not until long after, when he had achieved eminence as a morphologist, that he finally abandoned his old wish to hold a physiological chair.

Looking back on the past, we may now be glad that circumstances were against his wishes; for (though in every branch of science there is need at all times of a great man) there was at the middle of the century, in the early fifties, a special need in morphology for a man of Huxley's mould. Richard Owen was then dominant, and it is an acknowledged feature of Owen's work that in it there was a sudden leap from most admirable detailed descriptive labour to dubious speculations, based for the most part on, or at least akin to, the philosophy of Oken. Of the "new morphology" in which Johannes Müller was leading the way, and the criteria of which had been furnished by the labours of von Baer, there was then but little in England save, perhaps, what was to be found in the expositions of Carpenter. Of this new morphology, by which this branch of biology was brought into a line with other exact sciences, and the note of which was not to speculate on guiding forces and on the realisation of ideals, but to determine the laws of growth by the careful investigation, as of so many special problems, of what parts of different animals, as shown among other ways by the mode of their development, were really the same or alike, Huxley became at once an apostle. His very first work, that on the Medusæ, wrought out amid the distractions of ship life, written on a lonely vessel ploughing its solitary way amidst almost unknown seas, away from books and the communion of his fellow workers, bears the same marks which characterise his subsequent memoirs; it is the effort of a clear mind striving to see its way through difficult problems, bent on holding fast only to that which could be proved. This is not the occasion to insist in detail on the value of the like morphological work which he produced in the fifties and the sixties, or to show how he applied to other forms of animal life, to echinoderms, to tunicates, to arthropods, to molluscs, and last though not least to vertebrates, the same method of inquiry which guided the work on the Medusæ. Nor need I dwell on the many valuable results which he gained for science by attacking in the same spirit the problems offered by the remains of extinct forms. Moreover, he strengthened the effect of his own labours by admirable expositions of the results of others. Further, unlike his great predecessor who formed no school and had few if any disciples, it was Huxley's delight to hold out his hand to every young man whom he thought could profit by his help, and before many years were over his spirit was moving in the minds of many others. Thus it came about that during the latter half of this century, owing largely to Huxley's own labours and to the influence which he exerted not only in England but abroad, there has been added to science a large body of morphological truths, truths which have been demonstrated and must remain, not mere views and theories which may be washed away.

The excitement of the Darwinian controversy, with its far-reaching issues, has been apt to make us forget how great has been the progress of animal morphology during the past half century. Undoubtedly the solution of special problems touching animal forms, and the great theory of Natural Selection through the Struggle for Existence have been closely bound together: the special learning has furnished support for the general theory, and the general theory, besides strongly stimulating inquiry, has illumined the special problems. But the two stand apart, each on its own basis; and were it possible to wipe out, as with a sponge, everything which Darwin wrote, and which his views have caused to be written, there would still remain a body of science touching animal forms, both recent and extinct, acquired since 1850, of which we may well be proud. In the gaining that knowledge Huxley, as well by his own labours as by his influence over others, stands foremost, Gegenbaur being almost his only peer; and had Huxley done nothing more, his name would live as that of one of the most remarkable biologists of the present century.

As we all know, he did much more; his influence on England and on the world went far beyond that of his purely scientific writings. But when we reflect that a hundred years hence the image of the man as he went to and fro among men, so bright and vivid to-day, will have become dim and colourless, a shadow as it were, and that then the man will be judged mainly by the writings which remain, we must count these writings as the chief basis of his fame. And, though we may think it possible that the world of that day, much that is unwritten having been forgotten, may find it in part difficult to understand how great a power Huxley was in his time, the lapse of years will, we may be sure, in no way lessen, it may be will heighten, the estimate of his contributions to exact science.

As we all know, he did much more. To the public outside science he first became known as the bold, outspoken exponent and advocate of Darwin's views, and indeed to some this is still his chief fame. There is no need here to dwell on this part of his work, and I speak of it now chiefly to remark that the zeal with which he threw himself into this advocacy was merely a part of the larger purpose of his life. Science, or, to use the old phrase of the Royal Society, Natural Knowledge, had a two-fold hold on Huxley. On the one hand he felt deeply all the purely intellectual, and if we may use the word, selfish joys of fruitful progressive inquiry after truth. This was dominant in his early days, and to it we owe the long list of valuable researches, of which I just now spoke, and which followed each other rapidly in the fifties and the sixties. On the other hand, feeling deeply, as he did, his duties as a citizen of the world, science laid hold of him as being the true and sure guide to conduct man in all his ways; and this latter working of science in him, evident even in early days (witness his Address to Working Men at St. Martin's Hall in 1854), grew stronger and stronger as the years went on, until at last it took almost entire possession of him. To him, indeed, it may be said, science was all in all. He saw, as others see, in science a something which is broadening and strengthening human life by unceasingly bending nature to the use of man, and making her resources subservient to his desires; he saw the material usefulness of science, but he saw something more. He saw also, as others see, in science a something in which the human mind, exercising and training itself, makes itself at once nimble and strong, and dwelling on which is raised to broad and high views of the nature of things; he saw in science a means of culture, but he saw something more. He saw in science even as it is, and still more in science as it will be, the sure and trustworthy guide of man in the dark paths of life. Many a man of science goes, or seems to others to go, through the world ordering his steps by two ways of thinking. When he is dealing with the matters the treatment of which has given him his scientific position, with physical or with biological problems, he thinks in one way; when he is dealing with other matters, those of morals and religion, he thinks in another way; he seems to have two minds, and to pass from the one to the other according to the subject matter. It was not so with Huxley. He could not split himself or the universe into two halves, and treat the one and the other half by two methods radically distinct and in many ways opposed; he applied the one method, which he believed to be the true and fruitful one, to all problems without distinction. And as years came over him, the duty of making this view clear to others grew stronger and stronger. Relinquishing, not without bitter regret, little by little, the calm intellectual joys of the pursuit of narrower morphological problems, he became more and more the apostle of the scientific method, driven to the new career by the force of a pure altruism, not loving science the less but loving man the more. And his work in this respect was a double one; he had to teach his scientific brethren, at least his biologic brethren, the ways of science, and he had to teach the world the works of science. It was this feeling which, on the one hand, led him to devote so much labour to the organisation of biologic science in order that his younger brethren might be helped to walk in the straight path and to do their work well. It was this feeling, on the other hand, which made him urgent in the spread of the teaching of science. It was this, and no vain love of being known, which led him to the platform and the press. The zeal with which he defended the theory of Natural Selection came from his seeing the large issues involved; to him the theory was a great example of the scientific method applied successfully to a problem of more than biologic moment; while the fierceness of his advocacy was a natural expression of resentment on the part of one who saw a scientific conclusion, gained with unstinted pains and large reasoning, judged contemptuously by men who knew nothing of science according to methods in which science had no part.

Science, under this aspect, is a part of what is sometimes called philosophy; and though Huxley felt, in common with others, and felt deeply the pleasures of the intellectual wrestler, struggling with problems which, seemingly solved and thrown to the ground, spring up again at once in unsolved strength, it was not these pleasures alone which led him, especially in his later years, to devote so much time and labour to technical philosophic studies. He hoped out of the depths of philosophy to call witnesses to the value of the scientific method. Indeed, nearly all the work of the latter part of his life, including the last imperfect fragment, written when the hand of disease which was to be the hand of death was already laid upon him, and bearing marks of that hand, was wrought with one desire, namely to show that the only possible solutions of the problems of the universe were such as the scientific method could bring. This was at the bottom of that antagonism to theology which he never attempted to conceal, and the real existence of which no one who wishes to form a true judgment of the man can ignore. He recognised that the only two consistent conceptions of man and the universe were the distinctly theologic one and the scientific one; he put aside as unworthy of serious attention all between. He was convinced that the theologic conception was based on error, and much of his old age was spent in the study of theologic writings whereby he gathered for himself increasing proof that there was no flaw in the judgment which had guided his way from his youth upward. Not only so, but he was no less convinced that, owing to what he believed to be the essential antagonism of the theologic and the scientific methods, the dominance of the former was an obstacle to the progress of the latter. This conviction he freely confessed to be the cause of his hostile attitude; he believed it to be the justification of even his bitter polemics.

But while on the objective side his scientific mode of thought thus made him a never-failing opponent of theologic thought of every kind, a common tie on the subjective side bound him to the heart of the Christian religion. Strong as was his conviction that the moral no less than the material good of man was to be secured by the scientific method alone, strong as was his confidence in the ultimate victory of that method in the war against ignorance and wrong, no less clear was his vision of the limits beyond which science was unable to go. He brought into the current use of to-day the term "agnostic," but the word had to him a deep and solemn meaning. To him "I do not know" was not a mere phrase to be thrown with a light heart at a face of an opponent who asks a hard question; it was reciprocally with the positive teachings of science the guide of his life. Great as he felt science to be, he was well aware that science could never lay its hand, could never touch, even with the tip of its finger, that dream with which our little life is rounded, and that unknown dream was a power as dominant over him as was the might of known science; he carried about with him every day that which he did not know as his guide of life no less to be minded than that which he did know. Future visitors to the burial-place on the northern heights of London, seeing on his tombstone the lines—

> "And if there be no meeting past the grave,
> If all is darkness, silence, yet 't is rest.
> Be not afraid ye waiting hearts that weep,
> For God still 'giveth his belovèd sleep,'
> And if an endless sleep He wills,—so best"—

will recognise that the agnostic man of science had much in common with the man of faith.

There is still much more to say of him, but this is not the place to say it. Let it be enough to add that those who had the happiness to come near him knew that besides science and philosophy there was room in him for yet many other things; they forgot the learned investigator, the wise man of action, and the fearless combatant as they listened to him talking of letters, of pictures, or of music, always wondering which delighted them most, the sure thrust with which he hit the mark whatever it might be, or the brilliant wit which flashed around his stroke. And yet one word more. As an object seen first at a distance changes in aspect to the looker-on who draws nearer and yet more near, features unseen afar off filling up the vision close at hand, so he seemed to change to those who coming nearer and nearer to him gained a happy place within his innermost circle; his incisive thought, his wide knowledge, his sure and prompt judgment, his ready and sharp word, all these shrunk away so as to seem but a small part of him; his greater part, and that which most shaped his life, was seen to be a heart full of love which, clinging round his family and his friends in tenderest devotion, was spread over all his fellow men in kindness guided by justice.

M. FOSTER.

## *DR. FRIEDRICH TIETJEN.*

AT a time when astronomical knowledge is being extended at so rapid a rate, and in so many directions, as has been the case during the last few years, it is natural and right that the highest honour should be paid to those astronomers to whose genius and industry are due discoveries possible on account of original suggestion or ingenious execution. But at the same time, and on the other hand, there is no small danger that we may fail to give proper recognition to those other astronomers whose lives, unmarked by brilliant achievements, have been devoted to labours which are none the less valuable because they have been accomplished while quietly pursuing recognised lines, and are therefore devoid of conspicuous originality. In particular, the work of computation and arithmetical reduction of observations, without which the observations themselves either cannot be made or must remain almost entirely useless, is apt to fall into disrepute, as being wholly mechanical and unenterprising. This is certainly to be regretted; for just as a victorious general marching forward in the enemy's country must depend for his very safety on the fidelity and capacity of those officers who hold the conquered territory, so our scientific knowledge is liable to become disconnected and fragmentary unless we have capable men ready to perform the task of computing from the observations, and co-ordinating the results achieved in more exciting spheres of scientific work. If the pursuit of such unostentatious work lead to the effacement of the worker, our gratitude should be even all the greater for the self-denial exhibited and practised. Of such a man we have recently had to lament the loss, owing to the sad death of Dr. Tietjen, of Berlin.

Friedrich Tietjen was born in Oldenburg, in the year 1834; we therefore lose his services at the comparatively early age of sixty-one. He studied mathematics and astronomy at Göttingen, and subsequently at Berlin, with which latter city he has been continuously connected. In 1861, he became attached to the staff of the Berlin Observatory, and in one or other capacity this connection remained unbroken till the time of his death. He was appointed Professor of Astronomy in the University of Berlin, and Director of the *Recheninstitut*, allied to the Berlin Observatory. In his earlier career, Dr. Tietjen occupied himself with the observations of comets and asteroids, discovering in this way the asteroid *Semele*. To his activity and devotion the pages of the *Astronomische Nachrichten* abundantly testify. He is also known as the calculator of several cometary orbits, and also of the orbits and ephemerides of many asteroids. Some twelve years later, Dr. Tietjen became superintendent of the *Berliner Astronomisches Jahrbuch*, and his reputation in that capacity is not less assured than that of Dr. Powalky, who had preceded him in that office. As official director he paid great attention to shortening the labour of the necessary calculations as far as possible. Some of his methods have been published, others are not so well known, ill-health having prevented him from giving them to the world. Of the value and of the accuracy of this publication under the superintendence of Dr. Tietjen it is unnecessary to speak here, for it is sufficiently well known. Probably his most useful work was that done in superintending the preparation of the ephemerides of the small planets, the continual and rapid increase in the number of which, while it enormously increased his work, had likewise the effect of lessening the interest in this class of discoveries. While the national almanacks of other countries practically discontinued the publication of this class of ephemerides, Dr. Tietjen loyally struggled to supply sufficient information to ensure the observation of the small planets. Those who have attempted the determination of the mass of Jupiter from the perturbations of these bodies, and similar kinds of work, know how to appreciate the labours of Dr. Tietjen, by which the continuous observation from opposition to opposition has been rendered possible.

This skilled mathematician and remarkably facile computer died at Berlin, on June 21, deeply lamented by his numerous friends, and regretted by many who have profited by the devotion of his quiet unambitious life to the service of astronomy.

## THE MAXIM FLYING MACHINE.

ON Friday, July 5, a large party of scientific men paid a visit, by invitation of Mr. Hiram Maxim and Mr. Brodrick Cloete, to Baldwyns Park, Bexley, to witness a trial of the celebrated flying machine, and the latest development in the direction of mechanical flight.

The invitations were carefully distributed among those who were competent to judge of the magnitude of the task to be attempted, and who were prepared to examine closely the ingenious mechanical details by which it was clearly demonstrated that the machine had ample power to lift itself off the ground, carrying with it a supply of fuel and water, and a crew for the navigation.

An unscientific crowd of spectators might have become unmanageable, and might have developed iconoclastic tendencies (like the Weser boatmen with Denis Papin's original steam vessel) when the machine did not take to flight immediately and disappear from their astonished gaze.

> "As lewed people demeth comunly
> Of thinges that ben maad more subtilly
> Than they can in her lewednes comprehende
> They demen gladly to the badder ende——"

But the Bexley machine is purposely designed of extreme size, with the intention of thoroughly testing and elaborating the details of the mechanism, and of measuring the lifting power, within immediate reach of a workshop and skilled mechanics, more than of actually taking to the air; this will probably be first attempted with a much smaller machine, capable of lifting one man of jockey-like proportions, and mounted on a boat on a lake, so that short flights, like those of a flying fish, can be attempted for initial practice.

The lifting force of the machine is measured automatically as it runs along a railway track about half a mile in length, as shown in the accompanying illustration (Fig. 1), and the machine is prevented from taking to flight by wheels running underneath the outer wooden rails, seen in the figure; for much yet remains to be done in the way of practice in vertical steering before taking leave of the earth; the chief difficulties of the Aviator beginning when he wishes to descend and alight on the ground again.

Chaucer did not realise the difficulties of the problem when describing so jauntily the Bronze Horse in the Squieres Tale:—

> "This same stede shall bere yow ever-more
> With-outen harm, til ye be ther yow leste,
> Though that ye slepen on his bak or reste;
> And turne ayeyn, with wrything of a pin."
>
> "But whan yow list to ryden any-where,
> Ye moten trille a pin, stant in his ere—"
> "Bid him descend, and trille another pin,"
> "Trille this pin, and he wol vanishe anon."

The "wrything of a pin" is not inapt in describing the dominating gyrostatic brain of the Aviator, designed by Mr. Maxim to perform the vertical steering automatically.

The Bexley machine, complete with the water, naphtha fuel, and crew of three men on board, weighs 8000 lb.; and running at forty miles an hour with a pressure of 275 lb. per square inch, the engines develop 360-horse power, the thrust of the screws is 2000 lb., and the lifting effect of the aeroplanes and wings, 4000 square feet in area, is 10,000 lb.

A thrust of 2000 pounds at 45 miles an hour gives 240 thrust horse-power; or, with a speed of advance of the screw of 60 miles an hour, 320 indicated horse-power.

The total projected disc area of the screws is 500 square feet, each screw being nearly 18 feet in diameter, with a pitch of 16 feet; and thus requiring 330 revolutions a minute to give a speed of advance of 60 miles an hour.

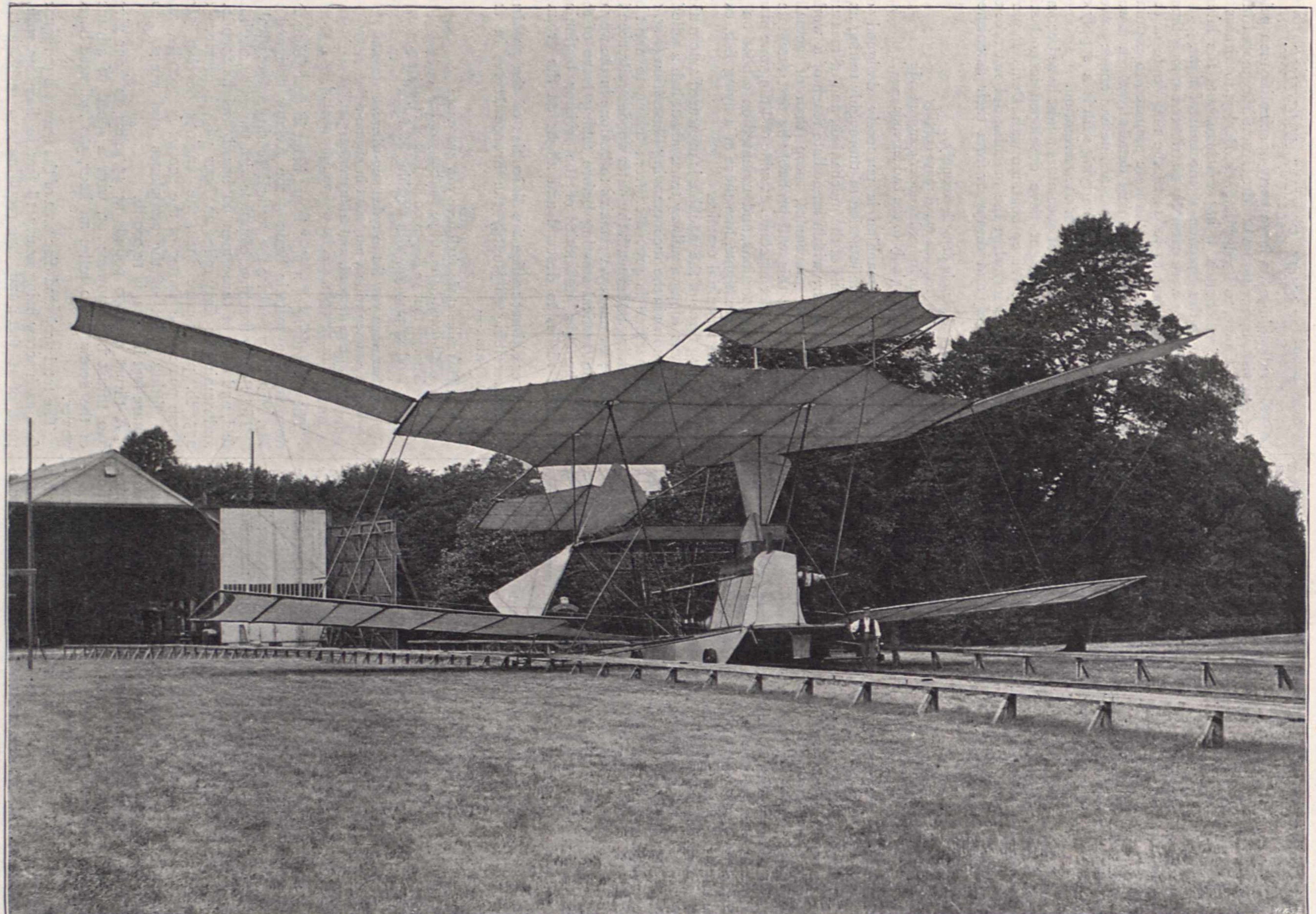

FIG. 1.—General view.

FIG. 2.—One of the screws, showing the connection with the engines.

Mr. Maxim calculates that, after making all allowances, he can at present lift 28 pounds per horse-power; but that, with improvements, he hopes to raise this figure to 50 or 60 pounds, and then a machine could take a flight of 500 or 600 miles.

When the machine is perfected, Mr. Maxim claims that the railway track may be dispensed with; and that a short run over a moderately level field will enable it to attain the velocity necessary to rise. As far as landing is concerned, he says that the aerial navigator will touch the ground while moving forward, and the machine will be brought to rest by sliding on the ground for a short distance. In this manner very little shock should result, whereas if the machine is stopped in the air and allowed to fall directly to the earth without advancing, the shock, though not strong enough to be dangerous (?) to life or limb, might be sufficient to disarrange or injure the machinery.

These numbers are taken from Mr. Maxim's lecture on "Experiments in Aeronautics," before the Society of Arts, November 28, 1894, where a full account of the mechanical details will be found. Each engine is a two-cylinder compound, with the cranks set at 180°; in this way the inertia stresses are self-contained, and racking of the framework is avoided; a similar arrangement is adopted by Mr. Thornycroft in his recent torpedo boats. A photograph showed Mr. Maxim lifting with ease one of these engines, from which 180-horse power can be developed. The boiler is, if possible, a still more wonderful miracle of lightness for its power, weighing only 1000 lb., and providing 360-horse power; the fire is given by a steel burner with 14,000 jets, made from the naphtha vapour delivered from an automatic gas generator. For details the reader must be referred to Mr. Maxim's lecture; but the chief result arrived at may be summarised as a performance of one-horse power for every 11 lb. of weight in the motor complete.

At this rate a 10-horse-power motor can be produced, which will weigh considerably less than an ordinary man; so that when Mr. Maxim can spare a little leisure from this fascinating problem of flight, he can beat easily the performance of the steam carriages recently competing in France, and carry off, we hope, the prize of £1000 offered in this country by the proprietors of the *Engineer;* and some day we may see his motor utilised for purposes of military traction, and galloping round the smartest battery of artillery on Woolwich Common.

Mr. Maxim eschews the gas bag of balloons and the use of vertical screws for securing levitation, and he relies entirely on the upward thrust on the aeroplane and wings, mounted at a slope of about 1 in 8, due to the currents of air rushing past them.

These surfaces are formed of canvas, stretched on a skeleton framework of hollow steel rods for the struts and thin steel wire for the ties; the large central aeroplane is composed of two parallel canvas surfaces, with a space between, and in this way the shape is preserved better; and the general set of the wings, smooth like cardboard, should excite the envy and stimulate the imitation of our sailmakers for yacht racing. The front and rear wings are shown pivoted about a horizontal axis, so as to act as rudders in a vertical plane.

The machine is started from the position in the photograph, being tied up to the indicator post shown in its rear; the propellers are then set in motion, and soon drive a gale of wind in their wake; when the pull of the rope has reached a definite amount, say 2000 lb., a hook is released, and the machine starts on its journey along the track. Mr. Maxim can now carry out his original notion of experiments with a model machine, tied to a post in a gale of forty miles an hour, to be found every afternoon in the cañons of California, in an artificial gale produced in the wake of his propellers. Dynamometers register simultaneously the thrust of the propellers, so that much interesting information concerning the dynamics of screw propulsion can be obtained here, especially if Mr. Maxim will stretch a wire carrying ribbons across the axes of the propellers, in front and in rear, to measure the direction of the air currents. The speed in air Mr. Maxim deals with is about double the speed of the torpedo boat in water; but the effect of "cavitation" in water, which is beginning to trouble the naval architects, is one which will not concern the propeller working in air.

Now that the main mechanical difficulties of construction have been overcome, a longer track is required for the purpose of practice in vertical steering while the machine is off the ground, but bearing upwards against the outer rails. It is unfortunate that difficulties should have been thrown in the way of making an extension of the present track beyond the domain of Baldwyns Park; so another practice ground, perhaps a sheet of water, must be found, not too far from headquarters or from skilled assistance.

During a short interval of delay, caused by a refractory pump, an adjournment was made to a gravel-pit close by, to witness a performance of the Maxim automatic gun.

Ancient and mediæval mythology is full of references to flying machines, from Dædalus and his son Icarus, and Archytas of Tarentum, to

> "The story of Cambuscan bold
> . . . And of the wondrous horse of brass
> On which the Tartar king did ride"

of Chaucer's Squieres Tale; and to Johnson's "Rasselas," Peter Wilkins, Baron Munchausen, and Auber's opera "le Cheval de Bronze."

"Rasselas," chapter vi., "A Dissertation on the Art of Flying," is so curiously apposite that some extracts may well find a place here.

"Among the artists that had been allured into the Happy Valley, to labour for the accommodation and pleasure of its inhabitants, was a man eminent for his knowledge of the mechanic powers, who had contrived many engines, both for use and recreation." "This artist was sometimes visited by Rasselas, who was pleased with every kind of knowledge, imagining that the time would come when all his acquisitions would be of use to him in the open world. He came one day to amuse himself in his usual manner, and found the master busy in building a sailing chariot. He saw that the design was practicable upon a level surface, and with expressions of great esteem solicited its completion. 'Sir,' said the master, 'you have seen but a small part of what the mechanic arts can perform. I have long been of opinion that instead of the tardy conveyance of ships and chariots, man might use the swifter migration of wings, that the fields of air are open to knowledge, and that only ignorance and idleness need crawl upon the ground." "'The labour of rising from the ground will be great,' said the artist, 'as we see it in the heavier domestic fowls; but as we mount higher the earth's attraction and the body's gravity will be gradually diminished, till we arrive at a region where man shall float in the air without any tendency to fall; no care will then be necessary but to move forward, which the gentlest impulse will effect.' 'Nothing,' replied the artist, 'will ever be attempted if all possible objections must be first overcome. If you will favour my project I will try the first flight at my own hazard. I have considered the structure of all volant animals, and find the folding continuity of the bat's wings most easily accommodated to the human form. Upon this model I will begin my task to-morrow, and in a year expect to tower into the air beyond the malice and pursuit of man.'" "The Prince visited the work from time to time, observed its progress, and remarked many ingenious contrivances to facilitate motion and unite levity with strength. The artist was every day more certain that he should leave vultures and eagles behind him, and the contagion seized upon the Prince. In a year the wings were finished, and on a morning appointed the maker appeared, furnished for flight, on a little promontory; he waved his pinions awhile to gather air, then leaped from his stand, and in an instant dropped into the lake. His wings, which were of no use in the air, sustained him in the water, and the Prince drew him to land half dead with terror and vexation."

These extracts show that Dr. Johnson had realised to some extent the difficulty of the problem to be solved; although Herr von Lilienthal's experiments, recently attempted by Prof. Fitzgerald, have to a certain extent falsified the universal application of his final catastrophe.

But, viewed with the cold calculating eye of mechanical science, the poetical descriptions are seen to be hopelessly absurd and impossible; now that Mr. Maxim has taken up the subject, and proved to demonstration the enormous power required, out of all proportion to the size, if man is ever to emulate the birds.

A. G. GREENHILL.

## NOTES.

THE Organising Committee of the third International Zoological Congress, to be held at Leyden, September 16-21, has sent us a copy of the provisional programme. The programme contains some details with reference to the work proposed, not given in our previous notes on the forthcoming Congress. At the first general meeting, a discourse will be delivered by Dr. Weismann; Mr. Haviland Field's scheme for bibliographical reform will be reported upon by M. E. L. Bouvier; and a report on the prize instituted in 1892, at the Moscow meeting, will be made by M. Blanchard. At the second general meeting, Prof. Milne Edwards will give a discourse, and Dr. F. E. Schulze will propose the nomination of a commission of three members to draw up, in three languages, the code of zoological nomenclature. Dr. John Murray will address the third general meeting. With regard to the sections: up to the middle of July, the first section had been promised a communication on Weismanism, by M. A. Giard; on cellular theory, by Mr. A. Sedgwick; on Plankton studies, by Prof. Victor Hensen; and a paper by Dr. S. Apathy. Dr. Bowdler Sharpe will address Section II. upon the classification of birds; and there will be papers on the origin of the lacustrine fauna of European Russia, by Prof. N. Zograf (Moscow); on the fauna of Borneo, by J. Buttikofer; and on *Pithecanthropus erectus*, by Dr. E. Dubois. In the third section, Prof. W. Leche (Stockholm) will read an odontological paper, and there will also be papers by Prof. R. Semon (Jena) and Prof. O. C. Marsh. In the fourth section, papers referring to the classification of living and fossil invertebrates, and bionomy, will be read by Dr. V. Salensky, Dr. C. W. Stiles, M. Blanchard, and Prof. S. J. Hickson. The section of entomology has received papers by M. E. de Selys-Longchamps, Father E. Wasmann, Dr. A. Fritze, and Prof. G. Canestrini. In Section VI., papers on the comparative anatomy and embryology of invertebrates will be read by A. de Korotnev, M. E. Perrier, Prof. J. W. Spengel, and Prof. Herdman. We understand that up to now the following delegates have been officially announced by the respective foreign Governments:—Belgium, Prof. Ed. van Beneden, Prof. Ch. van Bambeke, Prof. Gilson, and Prof. Lameere; France, Prof. Milne Edwards. MM. R. Blanchard, E. Bouvier, A. Certes, J. de Guerne, H. Filhol, Ch. Schlumberger, and L. Vaillant; Great Britain, Sir W. H. Flower, Prof. Sydney J. Hickson, Dr. J. Anderson, Dr. St. George Mivart, and Dr. P. L. Sclater; Sweden, Prof. F. A. Smith; Switzerland, Prof. Th. Studer, and E. Jung; United States (Department of Agriculture), Dr. C. W. Stiles.

A DESIRE is widely felt among the pupils of Prof. Leuckart that the occasion of the fiftieth year of his doctorate should not pass without some durable mark of recognition from those who have known and valued his inspiring influence. It is proposed that the memorial should take the form of a marble bust, and an appeal for contributions is being circulated as widely as possible. There is naturally some difficulty in obtaining the addresses of all old pupils; and it is hoped that those who receive the appeal will make it generally known. Contributions should be sent to Herr Carl Graubner (C. F. Winter's Verlag, Leipzig, Johannes-gasse 8), who has consented to act as treasurer of the memorial fund.

IT is proposed to honour Sir Joseph Lister by presenting his portrait to the Royal College of Surgeons for England, to be placed by the side of the portraits of John Hunter and other great surgeons of the past. On Tuesday last, in the presence of a large company, Sir Joseph was presented with a testimonial, in the form of a portrait of himself, subscribed for by his past colleagues and pupils, as a mark of esteem and admiration, on his retirement from the chair of clinical surgery at King's College Hospital.

THE sixty-third annual meeting of the British Medical Association was opened on Tuesday, when Dr. E. Long Fox retired from the presidential chair, and Sir J. Russell Reynolds was installed as his successor. Dr. Ward Cousins, in moving the report of the Council, said that when they last met in London, in 1873, they numbered only 1500, whereas now their membership exceeded 16,000. The financial position of the Association is most satisfactory, the assets exceeding the liabilities by more than £60,000. In his opening address, Sir Russell Reynolds dwelt chiefly upon the great advances that have been made, during the past twenty years, in the elucidation of both structure and function—such, for example, as in the researches upon the thyroid, the adrenal bodies, the spleen, and the liver; the advance of bacteriology; the function of the axis-cylinder of nerves; and the development of a new field of therapeutics in the serum-treatment of disease.

THE death is announced of Prof. H. Witmeur, Professor of Mineralogy and Geology in the University of Brussels, and of Prof. Josef Loschmidt, at Vienna.

SIR JOHN TOMES, F.R.S., died at Caterham on Monday, at eighty years of age. He was elected into the Royal Society in 1850, after carrying out valuable work referring to dental physiology and surgery. In 1883, with the late Prof. Huxley, he was elected an honorary fellow of the Royal College of Surgeons; and three years later the honour of knighthood was conferred upon him, in recognition of his services to his profession.

WE have already noted that an international conference for the protection of birds useful in agriculture, by helping to destroy injurious insects, has recently been held in Paris. Most of the countries in Europe were represented at the conference; and it was agreed that various measures should be taken to preserve useful birds, and to protect their nests and eggs from destruction. A list of birds considered useful has now been published by the Commission, and as this includes a number of our caged friends as well as other birds at present ruthlessly sacrificed for ornamental purposes, the trade in birds in various directions will naturally be curtailed. We learn from the *Révue Scientifique* that a period of three years is to be accorded to the different countries of Europe to allow them to arrange their laws in accordance with the principles agreed upon by the International Commission.

THE prospectus is issued of a proposed complete directory of living botanists of all countries, inclusive of the officers of botanic gardens, institutes, and societies, as also of their works and the botanical papers issued by them. Any communication should be made to Herr J. Dörfler, III Barichgasse 36, Vienna, of the botanical section of the Imperial Museum of Natural History.

MR. F. T. COVILLE, the honorary curator of the Department of Botany of the United States National Museum, issues an appeal for information on the aboriginal uses of plants by the natives of North America, accompanied by instructions as to the collecting of specimens, and the arrangement of the information under various heads.

WE learn from the *Botanical Gazette* that the Division of Vegetable Physiology and Pathology in the United States Department of Agriculture has had under cultivation during the past year over 1000 varieties of wheat and oats. The grains have been collected from nearly all parts of the world, and have been grown chiefly for the purpose of obtaining information upon their rust-resisting qualities. Numerous crosses have been made, and material and facts obtained which will be used in further work.

A VALUABLE memoir on the earthquakes of the Philippine Islands has recently been published by P. Miguel Saderra Masó, the director of the seismic section of the Observatory of Manila. The work consists of 122 quarto pages, and is illustrated by 48 plates, representing the instruments used in the observatory, the disturbed areas and isoseismal lines of sixty-one important earthquakes, and copies of some of the seismographic records, one of them somewhat resembling a bank manager's signature. With a seismological observatory so well equipped as that of Manila, a network of seismic and meteorological stations already established over the country, an energetic and capable director, and numerous shocks, the Philippine Islands promise to become as important a district for studying earthquakes as the neighbouring empire of Japan.

SOME beautiful enlargements of phonograph traces are given by Dr. John G. McKendrick in the *Journal of Anatomy and Physiology*, illustrating his paper "On the Tone and Curves of the Phonograph." The accuracy of the phonograph records is strikingly exemplified by the traces of four Koenig tuning-forks, giving 64, 128, 256, and 512 vibrations per second respectively. In each case, the length of indentations is half of that of the previous set, and they are of the same character. Traces of the sounds of a violin, flute, organ, military band, and human voice, singing and speaking, are reproduced. But these traces do not show the exact motion of the vibrating disc. To exhibit this, the phonograph traces were converted into curves by a lever arrangement. The lever ended in a fine point of a hard needle, which translated the up-and-down motion of the reproducing style into a to-and-fro wave motion. To get rid of all disturbing vibrations due to the needle itself, the latter was firmly fixed in a lead block to which the reproducing style was attached, and the phonogram cylinder was turned so slowly that its motion was almost imperceptible to the eye. By this contrivance the uniform curves due to a tuning-fork, the smooth notes of a piccolo, the strong undulations of a bassoon, and the highly over-toned ripples of an old English coach horn were very effectively made visible to the eye.

A RECENT number of *Modern Medicine and Bacteriological Review* contains an article on Prof. Bunge's important paper on the therapeutic value of iron, read at the German Congress of Internal Medicine last spring. An interesting table is quoted showing the amount of iron found in various food substances. Spinach contains considerably more iron than the yolk of eggs, whilst the latter, again, is superior in this respect to beef, next in order coming apples, lentils, strawberries, white beans, peas, potatoes, wheat, &c., and almost at the bottom of the list we find cow's milk. That this article of food, of such great importance in infant life, should contain so small a quantity of iron, led Prof. Bunge to conduct a series of experiments on animals, to ascertain in what quantity iron was present in the system of animals of different age. The interesting fact was established that younger animals contain a much greater quantity of iron than adult animals, that the body of a rabbit or a guinea-pig, for example, one hour old, was found to contain more than four times as much iron as that of similar animals two and a half months old. Prof. Bunge is of opinion that a long-continued exclusive milk diet for infants is not advantageous, but should be supplemented by the addition of wheat preparations. Strawberries and apples, however, become invested with fresh attractions by the light of these investigations. The writer of the article suggests that reform is required in the administration of iron, and that the immense quantities of iron in the shape of tonics, which custom prescribes for patients, may very possibly, in a large number of cases, only serve to increase the discomfort of the invalid by the disturbance caused in the digestive functions of the body. In conclusion the hope is expressed that Prof. Bunge's valuable results will "set physicians to thinking more of materia alimentariæ, and less of materia medica"!

THE *American Naturalist* for July contains a statement of the advantages offered for scientific study by the Missouri Botanical Garden at St. Louis, and by the Hopkins Seaside Laboratory, situated at Pacific Grove on the coast of California, and maintained by the Leland Stanford Junior University.

QUAIN'S "Elements of Anatomy" (Longmans, Green, and Co.) is now in its tenth edition. The second part of the third volume, which has just been published, comprises the descriptive anatomy of the cerebro-spinal and sympathetic nerves, and their ganglia. It is by Prof. G. D. Thane, who, with Prof. Schäfer, edits the edition.

WE have received the first part of a new monthly microscopical journal, the *Zeitschrift fur angewandte Mikroskopie*, edited by G. Marpmann, and published by Thost, of Leipzig. It will be especially concerned with technique and methods. The present number contains papers on a new species of *Scenedesmus*, by P. Richter; on modern imbedding materials, by the editor; on the fixing of spores and pollen in glycerin, by H. Reichelt; and a number of reviews and notes.

THE Central Meteorological Institute of Finland has just issued vol. xii. (new series) of its observations for the year 1893. This service is one of the oldest, having been established about 1844, and reorganised, under the superintendence of the Society of Sciences of Finland, in 1882. Among its earlier publications there is a series of eye observations taken at twenty minutes interval, from March 1848 to December 1856, before the establishment of self-registering instruments, a labour which is probably without a parallel. The present volume contains hourly observations for Helsingfors, particular attention being paid to the character and motion of clouds, and to atmospherical electricity.

THE eighth volume of the late Prof. Cayley's "Collected Mathematical Papers" has just appeared. The volume contains seventy papers, numbered from 486 to 555, published for the most part in the years 1871–73, and runs into 570 pages. In a prefatory note, Dr. A. R. Forsyth, the editor of this and the remaining volumes, says that Prof. Cayley had himself passed the first thirty-eight sheets for press, and prepared one note. The actual manuscript of this note, which was one of the last of Cayley's writings, is reproduced in fac-simile in the present volume, upon the kind of paper which he regularly used during his mathematical investigations. The remaining papers will appear without notes and references. The long biographical notice of Cayley, contributed by Dr. Forsyth to the *Proceedings* of the Royal Society, is reprinted in the volume just published.

THE sixth annual report of the Missouri Botanical Garden bears witness that useful work was accomplished during last year.

In addition to the necessary routine work, several researches were carried out, and the results of some of these investigations are embodied in the report. Mr. M. A. Brannon, who occupied the Garden's table at the Wood's Holl Marine Biological Laboratory, has his studies on *Grinnellia* nearly ready for publication. The Director, Mr. W. Trelease, has made a large collection of the flora of the Azores, and is now working at it. The collection fully represents the flora of those islands, and adds somewhat to what is known of the distribution of species through the group. The papers included in the present report are:—"Revision of the North American Species of Sagittaria and Lophotocarpus," by Mr. J. G. Smith, who also describes a few new or little known species; "*Leitneria Floridana*," by Mr. Trelease. "Studies on the Dissemination and Leaf Reflection of *Yucca aloifolia* and other Species," by Mr. H. J. Webber; and "Notes on the Mound Flora of Atchison County, Missouri," by Mr. B. F. Bush. The report is illustrated by sixty excellent plates.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Captain Fitzgerald; a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mrs. Florence Cowlard; a Serval (*Felis serval*), a White-necked Stork (*Dissura episcopus*), a Vociferous Sea Eagle (*Haliætus vocifer*), an Antarctic Skua (*Stercorarius antarcticus*) from Mozambique, presented by Mr. W. A. Churchill; a Cardinal Grosbeak (*Cardinalis virginianus*) from North America, a Lazuline Finch (*Guiraca parellina*) from Mexico, presented by Miss E. A. Krumbholz; an Orbicular Horned Lizard (*Phrynosoma orbiculare*) from California, presented by Miss Mabel Baker; a Frilled Lizard (*Chlamydosaurus kingi*) from Roebuck Bay, West Australia, presented by Mr. Saville-Kent; an Orang-outang (*Simia satyrus*, ♀) from Borneo, three Pratincoles (*Glareola pratincola*), European, an Eyed Lizard (*Lacerta ocellata*) from North Africa, a Brazilian Tortoise (*Testudo tabulata*), a Black Tortoise (*Testudo carbonaria*) from Brazil, deposited; two Plumed Ground Doves (*Geophaps plumifera*), bred in the Gardens.

## *OUR ASTRONOMICAL COLUMN.*

TERRESTRIAL HELIUM.—The discovery by Messrs. Runge and Paschen of the duplicity of the bright yellow line seen in the spectrum of the gas obtained from cleveite, and of its apparent non-coincidence with the solar $D_3$ line, as announced in NATURE of June 6, has naturally led to the re-observation of the solar line.

Mr. Lockyer informs us that on June 14, observing in the fourth order spectrum of a grating having 14,438 lines to the inch, he found the $D_3$ line in the chromosphere to have a considerable breadth with rather uncertain indications of doubling, while in the spectrum of a prominence the line was much better defined, and was distinctly double, the less refrangible component being the fainter, as in the case of the gas from cleveite.

Writing under date June 25 (*Ast. Nach*. 3302), Prof. G. E. Hale gives a preliminary account of the observations he has made with the powerful spectroscope of the Kenwood Observatory. To eliminate the effect of the sun's rotation in displacing the lines, observations were made of the chromosphere at the sun's north and south poles.

On June 19 and 20 the chromospheric line was found to be 0·54 tenth metres broad, the wave-length of the middle being determined as 5875·924. In the spectrum of each of two prominences observed on June 20 and 21, an inconspicuous bright line was detected on the less refrangible side of $D_3$, both lines being narrow and sharp, and the distance between them being 0·357 tenth metres. The absence of metallic lines, other than H and K, indicated that the fainter line was probably not due to the accidental proximity to $D_3$ of a faint metallic line. Further observations on June 24 showed that the broad line in the chromosphere was also divisible into two parts, and it became evident that the wave-length of the $D_3$ line determined on June 19 and 20, as well as that determined by Rowland, must be affected by an error on account of the presence of the faint line on the less refrangible side. So far, Prof. Hale has not succeeded in obtaining a measure of the wave-length of the more refrangible and brighter of the solar $D_3$ lines, considered as a separate line.

The results so far obtained may be stated as follows:—

| | |
|---|---|
| λ of solar $D_3$ line (Rowland) . . . . . . . . . . | 5875·982 |
| ,, ,, ,, (Hale) . . . . . . . . . . . . | 5875·924 |
| ,, brightest component of terrestrial line (Runge and Paschen) . . . . . . . . . . | 5875·883 |
| Distance apart of components of terrestrial line (Runge and Paschen) . . . . . . . . . | 0·323 |
| Distance apart of components of solar $D_3$ (Hale) | 0·357 |

The wave-length of the brighter component of the solar $D_3$ line remains to be determined before the question of the identity of the solar and terrestrial gas can be regarded as completely set at rest.

The announcement that the yellow line of the gas from cleveite was double, also led Dr. Huggins to observe the chromospheric line. In his first attempts he failed to see the line double (*Chemical News*, No. 1855), but he now states that he clearly saw the line to be double on July 10, 11, and 13, the less refrangible line being the fainter, and the distance apart of the lines being about the same as that of the lines in the cleveite gas according to Runge and Paschen (*Ast. Nach*. 3302).

It is worth recalling that Belopolsky observed the solar $D_3$ line to be double in May 1894, and ascribed the appearance to the superposition of a telluric line upon the bright line. Prof. Hale's observations demonstrate very clearly that Belopolsky's explanation cannot possibly account for the doubling of the line as observed by him.

EPHEMERIS FOR BARNARD'S COMET, 1884 II.—The following search ephemeris for the return of this comet is due to Dr. Berberich (*Ast. Nach*. 3301):—

| | R.A. h. | m. | s. | Decl. ° | ′ |
|---|---|---|---|---|---|
| Aug. 2 | 2 | 23 | 9 | + 12 | 29 |
| 6 | 2 | 29 | 7 | 13 | 19·5 |
| 10 | 2 | 34 | 30 | 14 | 6·8 |
| 14 | 2 | 39 | 16 | 14 | 50·9 |
| 18 | 2 | 43 | 23 | 15 | 31·9 |
| 22 | 2 | 46 | 48 | 16 | 9·7 |

The positions are for Berlin midnight, and are computed on the assumption that the comet will pass through perihelion on June 3. On June 30, Swift discovered a nebulous object in R.A. 20°, decl. + 2° 55′, which was missing on July 4, and was thought to be a possible return of the comet for which the ephemeris is given above. Dr. Berberich states that the observation by Swift does not fit closely into the orbit.

THE AUGUST METEORS.—Shooting stars from various radiants appear during the month of August; but the most important shower is that of the Perseids. These are visible for a considerable period, with a maximum on August 10. According to Mr. Denning, the radiant point exhibits an easterly motion among the stars; on the 10th it is situated in R.A. 45°, decl. 57° N.; on August 2 it is in R.A. 36°, decl. 55°, and on August 16 in R.A. 53°, decl. + 58°. The density of the shower varies but little from year to year, the number of meteors seen by one observer on the morning of August 11 being from sixty to eighty. Unfortunately the moon rises about nine o'clock on the 10th, so that this year only the brighter meteors will be visible.

## *THE SUN'S PLACE IN NATURE.*[1]

### IX.

IN most of the earlier attempts which were made to explain the origin of new stars, the leading idea was that of a single body being suddenly disturbed in some way, with the possible result that the heat of its interior became manifested at the surface. Thus Zöllner, in 1865, suggested that the phenomena might be

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 255).

produced by the bursting of the crust which had just formed on the surface of a star approaching extinction. Again, in connection with the new star in Corona, I pointed out in 1866 that all that seemed necessary to get such an outburst in our own sun was to increase the power of his convection currents, which we know to be ever at work. Dr. Huggins at that time believed that the appearances were due to gaseous eruptions in a single body, and that "possibly chemical actions between the erupted gases and the outer atmosphere of the star may have contributed to its sudden and transient splendour."

Though Zöllner's theory was further advocated by Vogel and Löhse in 1877, the idea that such outbursts can be produced in a single body, without external influence, is now almost universally abandoned.

The alternative hypotheses mostly have to do with the possible action between two bodies—an idea first suggested by Newton—and, as I have already pointed out, the evidence that two bodies were engaged in the case of Nova Aurigæ, at least, is conclusive. Even Dr. Huggins has found it necessary to suppose the existence of two bodies, in order to explain the phenomena observed in this case; and Dr. Vogel, who made some most admirable observations during the appearance of this new star, states most distinctly that we can no longer regard the assumption of a single body as sufficient in any explanation of the occurrence.

Notwithstanding the general agreement as to the presence of at least two bodies in the outburst of Nova Aurigæ, there remain considerable differences of opinion as to the nature of the separate bodies, and of the kind of interaction between them.

One explanation which has been suggested ascribes the luminous effects to the development of heat due to the passage of a dark body through a gaseous mass, somewhat after the manner in which meteoric stones produce the appearances of shooting stars in passing through our atmosphere. This kind of action was first suggested by Mr. Monck in 1885, but the possibilities of such actions have been recently more fully discussed by Prof. Seeliger. He points out that the photographic investigations of Dr. Max Wolf and others leave but little doubt that space is filled with more or less extensive aggregations of thinly-scattered matter, which may be called cosmical clouds, thereby accepting my view of a "meteoritic plenum."

If a heavenly body in rapid motion becomes involved in one of these cosmical clouds, its surface will become heated, and the vapourised products will be partly detached and assume the velocity of the cloud; the fluctuations of brilliancy of a new star on this hypothesis are produced by the varying density of the cosmic cloud through which the body is passing.

This hypothesis of Prof. Seeliger's has been strongly combated by Dr. Vogel.

Another explanation depending upon the action of gases has been suggested by Dr. Huggins:

"The phenomena of the new star scarcely permit us to suppose even a partial collision; though if the bodies were very diffuse, or the approach close enough, there may have been possibly some mutual interpenetration and mingling of the rarer gases near their boundaries."

The idea that the phenomena might be produced by the close approach of two bodies, and the consequent disturbances due to tidal action, was first started by Klinkerfues; it has been recently strongly advocated by Dr. Huggins, though I fail to see how it fits in with his previous explanation.

The tidal theory differs from Zöllner's only in ascribing the eruptions to the disturbances produced by tidal action when two bodies approach each other. To employ the words used by Dr. Huggins, the tidal action gives rise to "enormous eruptions of the hotter matter from within, immensely greater, but similar in kind, to solar eruptions." This explanation, however, has met with much opposition on physical grounds.

Thus, Prof. Seeliger writes:

"The static theory of the tides, which is used throughout, is quite incapable of giving a correct representation of the deformations which are doubtless produced by the close passage of the two bodies; for with very eccentric orbits (which it is necessary to assume on other grounds), the continually varying action would last for so short a time that one could scarcely expect to derive a trustworthy conclusion in regard to the actual circumstances from a consideration based on the forms which the bodies could assume in equilibrium."

Again, Vogel objects that:

"Sensible tidal action cannot be assumed to last for any considerable time, as on account of the great relative velocity of the bodies, they would separate at the rate of forty-six millions of miles per day."

These, however, are not the only objections which may be raised to the idea that we have to do with phenomena of the nature of solar prominences, whether produced by tidal action in the case of two bodies, or by a bursting of the crust which is forming in the case of a star approaching the end of its career as a luminous body. In the first place, there is no reason to suppose that the prominences in our own sun are produced by tidal action. The fact that many of the lines seen in the spectrum of Nova Aurigæ during its first appearance were coincident with lines seen in the solar chromosphere, appears, at first sight, to support the idea, but, since the spectra of nebulæ also show chromospheric lines, the same argument might also be applied to prove that nebulæ are manifestations of prominences. I do not imagine that very many will be prepared to believe that nebulæ are prominences, for if they are, they must be prominences of an unseen sun!!

Mr. Maunder and others have pointed out that if the phenomena be due to the formation of solar prominences, the bright lines should be displaced to the more refrangible sides of their normal places, for the reason that only those prominences on the side of the star presented to us would be able to produce visible bright lines, and such prominences would necessarily have their chief movement in a direction towards the earth. We have seen, however, that in Nova Aurigæ, the actual displacement of the bright lines was just the reverse.

Again, the fact that Nova Aurigæ ended by becoming a nebula is difficult to reconcile with the idea that in its earliest stages its luminosity was produced by outbursts of the nature of solar prominences. Nothing seems more remote than the possibility of prominences cooling down and becoming nebulæ. To have so-called "solar prominences" there must be a sun to produce them, and that must remain when the outburst of prominences has ceased; in this case the last stage of the spectrum of the new star should have resembled that of the sun. The fact that it did not indicates how worthless is the prominence suggestion in the light of modern knowledge.

Another very important objection to the solar prominence theory is this: If new stars are real stars capable of exhibiting prominence phenomena, then we have real stars ending as nebulæ, and thus clashing with the idea now accepted even by Dr. Huggins, that nebulæ are "early evolutionary forms" of heavenly bodies. Further, if new stars be real stars, we should have to believe that the last expiring atmospheres of stars consist of hydrogen and unknown gases; but if we take the evidence afforded by the stars themselves, we find that instead of their last atmosphere consisting of hydrogen it indicates carbon or carbon compounds.

It is evident, therefore, that at present there is no agreement among authorities as to which of the special theories I have brought to your notice is to hold the field, each special hypothesis having got no further than a damaging criticism from the authors of the others.

The remaining general hypothesis we have to consider is that advanced by myself. We have everywhere in space, as is now being revealed to us, especially by the photographs of Barnard, Max Wolf, and others, meteoritic aggregations, swarms, and streams, the constituents of which are, comparatively speaking, at rest, or are all moving one way, if they are moving at all, and undisturbed, because they are not being intersected by other streams or swarms at any one time. But supposing any of these bodies cross each other, as unfortunately sometimes excursion trains cross each other, then there is a very considerable difference in the phenomena; there are collisions, and the collisions produce increased light, and we think that a new star is being born. Nothing of the kind. No new star is being born; there is simply a disturbance in a certain part of space, and when the disturbance cools down we shall find that that part of space is still absolutely in the same order. In the case of Nova Aurigæ, and in the case of Nova Cygni after the war was over, nebulæ have been found to lie in the precise positions occupied by the new stars, and the only thing that one has to say about it is that the nebulæ were there before, but that in consequence of our incomplete survey of the heavens they had not been observed.

After the new photographic chart of the heavens has been made, in future times, it will be found that all new stars are not really new, but the lighting up of something which existed there already. The argument for this theory, you will understand, is simply this. Suppose I light a match, the smaller the match the sooner will it go out, and similarly the larger a fire the longer will it last. So if you are dealing in space with those illuminations which disappear in hours, days, or weeks, you cannot be dealing with any large mass; therefore the collisions in question cannot be between large masses of matter, but it must be a question of collisions amongst the smallest particles of matter we can conceive.

It is interesting to consider one of the possibilities which may explain why small nebulæ may be overlooked in telescopic observations. In the so-called achromatic telescope, all the rays of light are not brought to quite the same focus, so that when ordinary stellar observations are being made, the focus is adjusted for yellow rays which are most luminous to the eye. Now the greater part of the visual light of a planetary nebula is confined to a single line of the spectrum in the green, so that the focus which is best adapted for observations of stars is not suitable for the observation of a small nebula, the nebula being out of focus, and its feeble light thus reduced by the diffusion of the image. This difference is much more marked in large than small telescopes, and Prof. Campbell has pointed out that a small nebula like Nova Aurigæ will in general appear relatively brighter in a small telescope than a large one.

I will next go into some details touching the phenomena of the Novæ in relation to the hypothesis.

First let us see the crucial phenomena we have to explain. We have (1) the sudden bursting out of light and accompanying spectra; (2) the indication of the existence of two bodies revealed by the spectra; (3) the variations and dimming of the light and accompanying spectral changes; and (4) the final stage giving us the spectrum of a nebula.

Since the new era of spectroscopic work has begun, Nova Aurigæ and Nova Normæ have proved to us that the sudden illumination was, to say the least, associated with two bodies, and that these were in different stages of condensation. On the meteoritic hypothesis it was shown that the main differences between bodies giving bright and dark line spectra is one of condensation only: a sparse swarm gives us bright lines because the number of meteorites in unit volume is small and the interspaces are great; a more condensed swarm gives us dark lines because the number of meteorites in unit volume is greater, and the atmospheres of cooler vapour round each meteorite in collision begins to tell because the interspaces are reduced. I am the more justified in insisting upon the importance of this view that two bodies in different stages of condensation are involved, because years after it was formulated Dr. Huggins apparently arrived at it independently—at all events he makes no reference to my prior announcements when he brings it forward as an explanation of the phenomena.

The following quotations will show how this matter stands:—

"If we assume a brightening of the meteor-swarm due to collisions as the cause of the so-called new stars, we have good grounds for supposing that in these bodies the phenomena should be mixed, for the reason that we should have in one part of the swarm a number of collisions probably of close meteorites, while among the outliers the collisions would be few. We shall, in fact, have in one part the conditions represented in Class III*a*, and in the other such a condition as we get in $\gamma$ Cassiopeiæ."[1]

"The discussion of the observations which have been made of the changes that take place in the spectra of new stars, has already shown that the sequence of phenomena is strikingly similar to that which occurs in cometary spectra after perihelion passage. In general, however, there will be a difference: namely, that in comets there is usually only one swarm to be considered, whereas in new stars, there are two, which may or may not be equally dense. In new stars, we have accordingly the ntegration of two spectra, and the spectrum we see will depend upon the densities and relative velocities of the two swarms."[2]

"The spectrum of Nova Aurigæ would suggest that a dense swarm is moving towards the earth with a great velocity, and passing through a sparser swarm, which is receding."[3]

"The circumstance that the receding body emitted bright lines, while the one approaching us gave a continuous spectrum with broad absorption lines similar to a white star, may, perhaps, be accounted for by the two bodies being in different evolutionary stages, and consequently differing in diffuseness and temperature."[4]

[1] November, 1887. Lockyer. *Proc.* R.S., vol. xliii. p. 147.
[2] November, 1890. Lockyer. *Phil. Trans.*, 182 A, p. 407.
[3] February 11, 1892. Lockyer. *Proc.* R.S., vol. l. p. 435.
[4] May 16, 1892. Dr. Huggins. *Proc.* R.S., vol. li. p. 494.

Now two sheets or streams of meteorites interpenetrating and thus causing collisions will produce luminosities which will indicate the condensation of each, and the spectra of the two Novæ we are considering thus indicate that the colliding swarms were of different degrees of condensation, and the variations of light observed indicate several such encounters between less dense swarms after the most dense one had somewhat cooled down. The final stage was arrived at and the pure nebula spectrum produced when the most condensed swarm had ceased to indicate any disturbance, after all the others had returned to their pristine quiet and invisibility.

It is important to insist upon the fact that the nebulæ are now almost generally conceded to represent "early evolutionary forms." We have then from the first appearance of a Nova to the last a "backwardation" in the phenomena ending in an "early evolutionary form." Increase of temperature is accompanied by spectral changes in a certain order; if the temperature is reduced the changes occur in reverse order, until finally we reach the "early evolutionary form," which cannot be a mass of gas because its temperature is lower than that of the sun, which it is potentially, and it must contain all the substances eventually to appear in the atmosphere of the sun.

On the hypothesis, then, we imagine a nebula in the position occupied by Nova Aurigæ not chronicled for the reason stated. This nebula is approaching us. It was disturbed by a much sparser stream leaving us, the relative velocity being over 500 miles a second. During the time of impact, the disturbances produced in the two swarms gave rise to bright-line spectra in the sparse swarm, and to dark-line spectra in the more condensed one. The spectrum of the sparse swarm disappears, the spectrum of the dense swarm changes gradually from dark to bright lines, and ultimately it puts on the original nebula spectrum. It is still there, and still approaching us.

We have next to consider the objections which have been urged against this hypothesis. They are of a most trivial nature. An objection made by Vogel is that it is improbable that the velocities could have been so great after collisions. The reply is easy. The light was produced by the disturbed members of the two swarms which escaped end-on collision. On the meteoritic hypothesis we can escape from the difficulties produced by the old idea of collisions *en bloc*. Such objectors would urge that the velocity of a comet as a whole would be retarded by passing through the sun's corona, but we have instances to the contrary.

Another objection has been raised by Dr. Vogel because in relation to the Nova I did not restate all I had previously written concerning the origin of the cause of bright and dark line spectra in stars. It has been difficult for him to understand how one (temporary) star should have bright lines in its spectrum, and another (temporary) star should have dark lines. All I can say is that upon such objectors lies the onus of producing a more simple (and yet sufficient) explanation than that I have suggested.[1]

J. NORMAN LOCKYER.

(*To be continued.*)

## THE INTERNATIONAL GEOGRAPHICAL CONGRESS.

THE International Geographical Congress, now a recognised institution, has this year met for the first time on British ground. Originating in a festival organised to celebrate the inauguration of statues of Mercator and Ortelius at Antwerp and Rupelmond, the first Congress was held at Antwerp in August

[1] It has been stated that the meteoritic hypothesis has received a fatal blow from the observations of the Nova (*Astronomy and Astrophysics*, 1892, p. 509). Capable and unprejudiced persons I think will not be of this opinion. I append a quotation from an article by Prof. Campbell, which has appeared since the lectures were delivered.

"As bearing upon any possible theory of Nova Aurigæ, perhaps it will not be out of place to say here what I said last winter in another journal (Pub. A.S P. vi., 52, 133.) The Harvard College Observatory has shown that both Nova Aurigæ and Nova Normæ at discovery possessed substantially identical spectra of bright and dark lines, similarly and equally displaced. Both diminished in brightness, and both assumed the nebular type of spectrum. The new star of 1876 in Cygnus probably had nearly an identical history: passing from a bright star with a spectrum of bright and dark lines, to a faint object with a spectrum consisting of one bright line (undoubtedly the nebular line $\lambda$ 5010, or the two nebular lines $\lambda$ 5010 and $\lambda$ 4960 combined). We may say that only five 'new stars' have been discovered since the application of the spectroscope to astronomical investigations, and that three of these have had substantially identical spectroscopic histories. This is a remarkable fact. We cannot say what the full significance of this fact is. One result, however, is very clear: the *special* theories propounded by various spectroscopists to account for the phenomena observed in Nova Aurigæ must unquestionably give way to the more *general* theories." (*Astrophysical Journal*, Jan. 1895, p. 51.)

1871, under the name of the "Congrès des Sciences géographiques, cosmographiques, et commerciales," and under the influence of the revival of geographical learning subsequent to the Franco-German War, it has met from time to time at different centres, gaining strength and vitality on each occasion. The second Congress assembled at Paris in 1875; the third at Venice in 1881; the fourth at Paris in connection with the Great Exhibition of 1889; and the fifth at Berne in 1891. In each case the representative Geographical Society of the country concerned was responsible for the organisation and arrangement of the meeting, and at Berne it was definitely resolved that in future the Congress should be constituted at intervals of not less than three, nor more than five years, the resolution taking practical shape in the acceptance by the Royal Geographical Society of the responsibilities of a meeting in London in 1895. A proposal, emanating from the Berne Geographical Society, to the effect that the chief officials of each Congress shall retain office until the meeting of the next, is to be submitted this year, and its acceptance would mark a further step towards the establishment of a great permanent organisation for the systematic study and exploration of the globe.

The sixth Congress differs from its predecessors in a characteristically British fashion, inasmuch as it is practically a private enterprise; no State or municipal aid being forthcoming, as on previous occasions. Nevertheless the Royal Geographical Society, aided by grants from a few of the City companies and by private generosity, has been able fully to cope with the demands made on its resources by the immense influx of geographers from all parts of the world. Accommodation has been found in the Imperial Institute, which affords ample room for private and public business meetings, for exhibitions, and for all manner of social functions, as well as opportunity for that private intercourse which goes so far to enhance the value of such meetings. The Congress is under the patronage of the Queen and the Prince of Wales, and the honorary presidency of the King of the Belgians, the Duke of Connaught, the Duke of York, the Crown Prince of Denmark, and the Grand Duke Nicolas Michailovich. The President is, according to the custom of the Congress, the President of the Geographical Society under whose auspices it meets; in this case the President of the Royal Geographical Society, Mr. Clements R. Markham, C.B., F.R.S. A large number of eminent public men and geographers have accepted the position of honorary vice-presidents.

The work of organisation has been carried out by a number of committees, under the chairmanship of Major L. Darwin, R.E.; the general secretaryship is in the hands of Mr. J. Scott Keltie and Dr. H. R. Mill; and the exhibition is under the direction of Mr. E. G. Ravenstein, Mr. John Coles, and Mr. John Thomson.

In devising the general arrangements, it has hitherto been the practice to abstain from formulating any rigorous rules, and to leave the managing Society a pretty free hand. In some cases, notably at Venice, the Congress was somewhat overwhelmed by the exhibition of geographical objects; while in others undue subdivision into sections has tended to defeat one of the most praiseworthy objects of the meeting. Profiting by the experience obtained, the Royal Geographical Society has kept the range of the exhibition within comparatively narrow limits. The Geographical Societies of Paris, Berlin, and St. Petersburg, and various Government departments and private individuals in all parts of the globe have sent representative exhibits of recent work, and the collections have been in many cases arranged entirely by the exhibitors. Another department is devoted to paintings and photographs of geographical interest, including, amongst other things, a series of historical portraits of eminent travellers, cartographers, and geographical writers, many valuable sketches and photographs contributed by explorers, and lantern slides and diagrams adapted to the purposes of geographical education. A third section, due to Mr. E. G. Ravenstein, consists of a loan exhibition, intended to illustrate the development of cartography from the time of Ptolemy to the end of the eighteenth century. Mr. Ravenstein is to be congratulated on the achievement of a remarkable success, for while no important stage of progress is unrepresented, those illustrated by fac-similes only are wonderfully few. The collection includes many priceless examples, such as the Leonardo da Vinci maps belonging to the Queen, the "Henry II." map belonging to the Earl of Crawford and Balcarres, the Mollineux globe from the library of the Middle Temple, the Agas map of London from the Guildhall, the manuscripts of the early Indian surveys by Ritchie and Rennel, Topping, Macluer, and Mackenzie, from the India Office, and extensive contributions from the libraries at Lambeth Palace, the Admiralty, the Ordnance Survey, various Geographical Societies, and the private collections of Mr. S. W. Silver, Mr. H. Yates Thompson, Mr. E. A. Petherick, and many others. It is to be noted that the catalogue of this exhibition, with its appended list of maps, portolani, and atlases in the British Museum, forms an excellent bibliographical outline of the subject.

A similar collection, though on a necessarily smaller scale, has been made by Mr. John Coles, in the department of surveying and meteorological instruments. The exhibits of the Hydrographic Department of the Admiralty and the Ordnance Survey Office are of great historical interest. We could have wished it had been possible to allot a further space to instruments used in deep sea explorations, especially as their modern developments are so well illustrated by Prof. Otto Pettersson and Dr. H. R. Mill.

A final section of the exhibition consists of the most recent equipments for exploration, surveying, mapping, and teaching geography, shown by numerous private firms.

The same leading idea, that of representing general features, has been kept in view in arranging the work of the meetings. While no attempt has been made to present popular programmes, the whole range of geography has been covered, and the chief effort directed towards furthering those larger interests which concern all geographers, rather than to the discussion of more minute technicalities, however important in themselves. Thus general meetings are to be devoted to Polar Exploration, the development of Africa, Exploration, and Cartography; and sectional meetings deal with Geographical Education, Photographic Surveying, Physical Geography, Geodesy, Oceanography, Geographical Orthography and Definitions, and Limnology.

The date of our going to press constrains us to defer a report of most of the work done in all these different departments until next week, except in so far as the earlier meetings are concerned. On Friday evening (July 26) the delegates were presented to H.R.H. the Duke of York by the Ambassador or Chargé d'Affaires of their respective countries. The following were represented, either by Government delegates or by delegates of Geographical Societies:—Austria-Hungary, Belgium, Denmark, France, Germany, Greece, Italy, Netherlands, Norway, Portugal, Roumania, Russia, Spain, Sweden, Switzerland, Turkey, United States, Mexico, Brazil, Japan, Persia, New South Wales, New Zealand, Queensland, South Australia, Tasmania, Victoria, Western Australia, Cape of Good Hope, and the United Kingdom. After the private reception, the Duke of York welcomed the whole Congress in the name of the Queen and the Prince of Wales, and the President made a brief address of welcome on behalf of the Royal Geographical Society, the other British Geographical Societies, and the Geographers of the United Kingdom. The Hon. Chief Justice Daly, of the New York Geographical Society, the oldest President of a Geographical Society living, replied on behalf of the foreign members and delegates, and the meeting adjourned, the remainder of the evening being spent in the gardens of the Institute, where music was discoursed by Strauss' orchestra.

On Saturday (July 27) the Congress assembled at 10 a.m. to hear the President's opening address, which paid a graceful tribute to the geographical work of the nations whose delegates and representatives he cordially welcomed, and gave a forecast of the work about to be undertaken by the Congress. A vote of thanks was proposed by Prince Roland Bonaparte, and seconded by Prof. von den Steinen. At noon two sections were formed. In Section B, which was presided over by Mr. Markham, supported by Chief Justice Daly and Prof. von den Steinen, Prof. Levasseur read a paper on geography in schools and universities, which outlined a system of geographical education extending through primary, secondary, and higher stages. Señor Torres Campos supported the views expressed by Prof. Levasseur, and discussion was continued by M. Ludovic Drapeyron. The importance of a university training for teachers of geography was urged by Dr. R. Lehmann in the second paper, and the needs of geography in secondary education were set forth by Mr. A. J. Herbertson in the third. Thereafter Dr. W. Henkel allowed a paper on geography and history in schools, standing in his name, to be held as read, in order to allow time for discussion. Mr. H. J. Mackinder advocated the establishment of a central school of geography in London, in order to place geographical teaching in this country on a proper footing. Mr.

G. N. Hooper referred to the work done by the London Chamber of Commerce, and the discussion was continued by Messrs. Phillips, Burgess, Batalha Reis, and Yule Oldham. The President proposed that a committee, consisting of Chief Justice Daly (chairman), Prof. Levasseur, Prof. Lehmann, Mr. Mackinder, and Mr. Herbertson, should be appointed to consider a resolution on geographical education, to be submitted to the Congress.

Section C, which met at the same time, concerned itself with photographic surveying. The presidential chair was occupied by Prince Roland Bonaparte and General Walker jointly. In a paper read on his behalf by M. Schrader, Colonel Laussedat considered the application of photography to the rapid determination of points in levelling, and a combined camera and theodolite was exhibited. M. de Déchy, in discussion, insisted that photography must always be merely auxiliary to triangulation, and must not in any way replace it; and Mr. Coles described his work in constructing a map of the Caucasus from photographs alone. Captain E. H. Hills then read a paper on the determination of terrestrial longitudes by means of photography, in which he described improved methods of exposing and measuring plates used in photographing lunar distances, by means of which he had obtained better results than those obtained by Schlichter and Runge. An abstract of a paper by Prof. J. Thoulet, suggesting the extended application of photography to the survey of rapidly shifting sandbanks, was read in his absence. Mr. Coles described and exhibited Colonel Stewart's camera for producing photographs of the whole horizon, and the proceedings closed with an informal communication by M. Janet on the determination of longitudes without instruments of precision.

## HELIUM, A CONSTITUENT OF CERTAIN MINERALS.[1]

### II.

### (II.) *The Properties of Helium.*

FROM what has preceded, it appears that up to now only three minerals are available as sources of helium, unless, indeed, very large quantities of samarskite and yttrotantalite are worked up. These three are clèveite, the uraninite investigated by Hillebrand, and bröggerite. And here we wish to express our indebtedness to Prof. Brögger for his great kindness in placing a large stock of bröggerite at our disposal. It has furnished a large quantity of the helium which we have had in our hands.

Although, so far as we were able to judge by throwing into a two-prism spectroscope of Browning's the spectra of samples of gases obtained from the minerals previously mentioned, all the specimens of helium were identical, still a further proof was desirable. Owing to the small quantities of gas yielded by these minerals, amounting in most cases to a few c.c., it was impossible to ascertain whether these samples were of the same density; but the case was different with the gas from clèveite and from bröggerite. In each case a sufficient quantity was obtained to make it possible to determine the density with fair accuracy. It will be convenient therefore to describe the methods of extracting the gas and the methods determining its density.

In the communication to the Royal Society it was stated that the maximum density of the original gas from cleveite was 3·89. The spectroscope showed the presence of nitrogen in this sample; the bands were very brilliant at high pressure, but on reducing the pressure the yellow line became brilliant, and the nitrogen spectrum disappeared. This always happens when the tube has platinum electrodes and a strong discharge is passed for a considerable time. An attempt was made to remove the nitrogen from this sample of gas by circulating it over red-hot magnesium; but an unfortunate accident caused the admixture of about its own volume of air, carrying with it argon, from which at present there is no known method of separating helium.

It appeared important to decide whether the gas evolved from these minerals is helium, or a compound of hydrogen and helium; for in the preliminary set of experiments the treatment was such that a hydride would have been decomposed either by sparking with oxygen or by passage over copper oxide at a red heat.

[1] A paper by Prof. William Ramsay, F.R.S., Dr. J. Norman Collie, and Mr. Morris Travers, read before the Chemical Society on June 20. (Continued from p. 308.)

The result of experiments directed to this end is to show that no combined hydrogen is present. Gas was extracted from nineteen grams of bröggerite by heating it in a combustion-tube to dull redness; the combustion-tube was connected with a Töppler's pump by means of thick-walled india-rubber tubing, wired carefully. Special experiments showed that the leakage through the india-rubber amounted between Saturday and Monday to less than one small bubble. The bröggerite yielded about 75 c.c. of gas, a large portion of which was absorbed by caustic soda, leaving about 35 c.c. A second charge of 18·3 grams gave 58·5 c.c., and a third, of 22·1 grams, gave 66·0 c.c. The amount of gas evolved depends largely on the temperature. The evolution is rapid at first, but becomes very slow after three hours, and the heating was always stopped before all the gas which might have been extracted had come off. The last portions, as will be seen later, were extracted by fusion with hydrogen potassium sulphate.

This crude product from bröggerite blackened mercury, doubtless owing to the presence of hydrogen sulphide.

The density of this sample was determined; the data are these.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperarure ... ... ... ... | 22·9 |
| Pressure (corr.) ... ... ... | 766·7 mm. |
| Weight... ... ... ... ... | 0·0327 gram |
| Density (O = 16) ... ... ... | 11·90 |

The exceedingly small capacity of the bulb calls for some remark, but for no apology. The object here is, not to determine the density with the utmost accuracy, but to secure a guide, sufficient for our purpose, which will indicate the probable molecular weight. Now the hydrogen contained in such a bulb at 0° and 760 mm. weighs approximately 0·0030 gram. A sensitive balance by Oertling, adjusted for the special purpose, could easily be read to 0·00005 gram, without resorting to the the reading of oscillations of the pointer; and this gives an accuracy of 5 parts in 300, or 1·7 per cent. Hence the density of hydrogen, thus determined, might vary between 0·983 and 1·017. It is evident that such an approximation is quite sufficient for our present purpose. The total volume of this gas was 124·5 c.c. A solution of soda was introduced by means of a pipette, and after all absorption had ceased, the residue measured 78·0 c.c. The density was again determined.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperature ... ... ... ... | 21·6° |
| Pressure (corr.) ... ... ... | 765·4 mm. |
| Weight ... ... ... ... | 0·0058 gram |
| Density (O = 16) ... ... ... | 2·105 |

This gas was now left in contact with palladium sponge for a night. The sponge was made by reducing the chloride in a current of hydrogen, at a dull red heat. As it was somewhat porous, it was hammered on a steel anvil before introducing it into the gas, which, of course, was confined over mercury. The contraction amounted to abont 1/30th. The density was again taken.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperature ... ... ... ... | 19·2° |
| Pressure (corr.) ... ... ... | 760·2 mm. |
| Weight ... ... ... ... | 0·00630 gram |
| Density (O = 16) ... ... ... | 2·284 |

This gas had undergone no treatment which was of a kind to remove combined hydrogen, unless, indeed—a very improbable assumption—it be supposed that the compound should be decomposed by contact with metallic palladium. The gas was therefore placed in contact with copper oxide, which had previously been heated to redness in a vacuum, and a tube filled with phosphoric anhydride was so interposed as to absorb any water produced. The gain in weight of this tube was 0·0016 gram, indicating the oxidation of about 2 c.c. of hydrogen. In all probability this hydrogen had remained over after treatment with palladium; for it bears no proportion to the total quantity of gas—78 c.c.

The density was again determined.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperature ... ... ... ... | 16·67° |
| Pressure (corr.) ... ... ... | 754·9 mm. |
| Weight ... ... ... ... | 0·00720 gram |
| Density (O = 16) ... ... ... | 2·606 |

We give thus minutely all the determination of density of such samples, because, although they refer to an imperfectly purified sample, yet they show that the density is very low, and they trace, moreover, the gradual change as one ingredient after another is removed.

The bröggerite which had been heated in a vacuum was next fused in successive portions with hydrogen potassium sulphate. A large quantity of gas was evolved, consisting of sulphur dioxide, carbon dioxide, nitrogen, and helium. The sulphur dioxide was removed with chromic mixture, and the carbon dioxide with caustic soda; the yield was 45 c.c. The density was then determined.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperature ... ... ... ... | 16·18° |
| Pressure (corr.) ... ... ... | 753·3 mm. |
| Weight ... ... ... ... | 0·01035 gram |
| Density (O = 16) ... ... ... | 3·748 |

No alteration in volume occurred on passing the gas for several hours over red-hot copper oxide. Hence no hydrogen was present in the free state; and if combined, passage over copper oxide does not decompose the hydride, as was seen before, when the water produced was weighed. It may be remarked that every known hydride would yield its hydrogen on such treatment.

This sample of gas was next circulated over red-hot magnesium for several hours. It is hardly necessary to state that the magnesium was first heated to redness in a vacuum so as to remove hydrogen. In case any should escape removal, however, a red-hot tube of copper oxide formed part of the circuit, as well as a tube filled with phosphoric anhydride. Some caustic soda solution was present in the reservoir above the mercury, which would have absorbed the products of combustion of any hydrocarbon present. The density of this gas was calculated from the data appended.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperature ... ... ... | 14·88° |
| Pressure (corr.) ... ... ... | 756·0 mm. |
| Weight ... ... ... ... | 0·00845 gram |
| Density (O = 16) ... ... ... | 3·037 |

On examining the magnesium tube, after it had cooled, it was found that on moistening it ammonia was evolved. The gas was, therefore, again circulated over magnesium, at a somewhat higher temperature, so high, indeed, that the gas must have passed repeatedly through magnesium vapour. On pumping out the tubes, an accident led to the loss of a few c.c. of gas; hence the weighing bulb had to be filled at a somewhat reduced pressure. The density is given below.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperature ... ... ... | 18·33° |
| Pressure (corr.) ... ... ... | 615·8 mm. |
| Weight ... ... ... ... | 0·0049 gram |
| Density (O = 16) ... ... ... | 2·187 |

Again, on moistening the broken magnesium tube, ammonia was evolved; it was recognised by its odour and by its turning red litmus paper blue.

A further experiment was made with bröggerite. 30·8 grams were heated in a vacuum and the gas was collected over mercury, on to the surface of which a few c.c. of caustic soda solution were introduced. The yield of gas was 65 c.c. It was circulated over copper oxide to remove hydrogen, and its density was then determined.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperature ... ... ... | 19·70° |
| Pressure (corr.) ... ... ... | 756·7 mm. |
| Weight ... ... ... ... | 0·0068 gram |
| Density .. ... ... ... | 2·481 |

The density of this sample is almost coincident with that of a previous sample, 2·606, obtained in the same way, after it had been purified from hydrogen. This gas was next circulated over very hot magnesium, so as to remove nitrogen. Again, it is certain that for many hours the gas must have been mixed with magnesium vapour, for the magnesium had been completely volatilised out of the hot part of the combustion-tube, and had condensed at the cool end. Again, the product, when moistened showed the reactions of ammonia, proving that nitrogen had been removed. The density of this sample was next taken.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 cc. |
| Temperature ... ... ... | 19·17° |
| Pressure (corr.) ... ... ... | 756·7 mm. |
| Weight ... ... ... ... | 0·0056 gram |
| Density ... ... ... ... | 2·044 |

The copper oxide tube was omitted during this circulation; hence the density was low, 2·044. The spectrum of this gas showed hydrogen lines and feeble nitrogen bands. A second determination of density, in which the bulb was freshly filled, gave, at the same pressure and at a temperature differing by only 1° from the previous one, an identical weight. Further circulation for a whole day over red-hot magnesium, raised to the highest temperature which the tube could stand, gave a specimen from which hydrogen and nitrogen were absent; at least, the barest trace was visible in a vacuum-tube filled at a fairly high pressure; and care was taken to interpose a red-hot copper oxide tube, and, as usual, a tube containing phosphorus pentoxide. The effect of this circulation was to raise the density.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperature ... ... ... | 17·1° |
| Pressure (corr.) ... ... ... | 763·2 mm. |
| Weight ... ... ... ... | 0·0060 gram |
| Density (O = 16) ... ... ... | 2·152 |

It is of interest to note that this sample, procured by heating bröggerite in a vacuum, has a density practically identical with that of gas obtained by fusing bröggerite with hydrogen potassium sulphate; that sample had density 2·187.

We next proceeded to extract the gas from 6·96 grams of Swedish clèveite. When heated in a vacuum, the gas was rapidly evolved at first, more quickly than from bröggerite. About 60 c.c. were obtained, and, after treatment with soda, the residue occupied 26·3 c.c. As this was not sufficient for our purpose, and as we had already by density and spectrum proved the identity of gas evolved from bröggerite on heating, and on fusion with acid sulphate, the remaining clèveite was mixed with about five times its weight of fused and dried hydrogen potassium sulphate, placed in a tube, and heated in a vacuum. A further quantity of gas was evolved, which was at once treated with caustic soda solution. Both quantities of gas were mixed. This sample was then circulated over copper oxide for several hours, and the density was then determined with the following result.

| | |
|---|---|
| Volume of bulb ... ... ... | 33·023 c.c. |
| Temperature... . . ... ... | 19·43° |
| Pressure (corr.) ... ... ... | 763·2 mm. |
| Weight ... ... ... ... | 0·0061 gram |
| Density ... ... ... ... | 2·205 |

The spectrum of this gas showed the merest trace of nitrogen, but no hydrogen. The density, it will be seen, is practically coincident with that of the gas from bröggerite. It is noteworthy that the gas from clèveite contains no nitrogen. We are absolutely certain that the presence of nitrogen in the gas from bröggerite is not to be explained by leakage of air, for the tightness of the apparatus was frequently tested during each operation.

We have therefore three determinations of density, and the mean may be taken as approximately correct to within 0·05. They are:

| | |
|---|---|
| Gas from bröggerite by heating ... ... | 2·152 |
| Gas from bröggerite with $HKSO_4$ ... | 2·187 |
| Gas from clèveite ... ... ... ... | 2·205 |
| Mean ... ... | 2·181 |

All these samples of gas were now mixed and passed through the usual absorbents for nitrogen and for hydrogen, namely magnesium, copper oxide, soda-lime, and phosphoric anhydride. The density of this sample was then determined with the larger bulb. The error due to error in weighing cannot in this case amount to more than 0·3 per cent., and is probably less. Of course, the purity of the gas would affect the result. The data are as follows.

| | |
|---|---|
| Volume of bulb ... ... ... | 162·843 c.c. |
| Temperature... ... ... ... | 17·07° |
| Pressure (corr.) ... ... ... | 764·9 mm. |
| Weight ... ... ... ... | 0·03057 gram |
| Density (O = 16) ... ... ... | 2·218 |

*The wave-length of sound* was determined with this sample of gas in a tube 1 metre in length and 9 mm. internal diameter; the vibrating rod was 580 mm. long. We found it exceedingly difficult to procure a tube in which really good sound-waves could be shown with helium; indeed, we were on several occasions nearly despairing of gaining our object. But at last perfect waves, easily read and easily counted, were produced, and measurements were taken with the following results.

| Series ...... | I. | II. | III. | IV. | V. | VI. | VII. |
|---|---|---|---|---|---|---|---|
| Half wave-length | 98·6 | 98·6 | 97·6 | 98·3 | 100·0 | 98·6 | 97·9 mm. |

Mean of all, 98·8 mm. at 18·9°.

In air, a similar series gave the numbers

| Series ... | I. | II. | III. | IV. | V. |
|---|---|---|---|---|---|
| Half wave-length ... | 36·00 | 36·03 | 36·11 | 35·89 | 36·16 |

Mean, 36·04 mm. at 20·1°

The ratio of the specific heat at constant volume to that at constant pressure for air is 1·408; that for helium is—

$$\frac{(36{\cdot}04)^2 \times (273 + 18{\cdot}9) \times 14{\cdot}479}{273 + 20{\cdot}1} : (98{\cdot}8)^2 \times 2{\cdot}218 :: 1{\cdot}408 : 1{\cdot}632$$

This sample of gas was again circulated over very hot magnesium and copper oxide for seven hours; the magnesium had no smell of ammonia when breathed on, nor did it turn red litmus paper blue until after long standing. The magnesium was mostly volatilised out of the hot part of the tube.

The *density* of this sample of gas was determined.

| | |
|---|---|
| Volume of bulb ... ... ... | 162·843 c.c. |
| Temperature.. ... ... ... | 19·8° |
| Pressure (corr.) ... ... ... | 730·0 mm. |
| Weight ... ... ... ... | 0·0278 gram |
| Density ... ... ... ... | 2·133 |

The wave-length of sound was re-determined in the same tube as before. The figures are

| Series... | I. | II. | III. | IV. | V. | VI. | VII. | VIII. |
|---|---|---|---|---|---|---|---|---|
| Half wave-length. | 102·7 | 100·7 | 101·6 | 100·7 | 102·6 | 101·6 | 100·9 | 101·1 mm. |

Mean of all, 101·5 mm.

The ratio of the specific heats of helium, calculated from these numbers as before, is 1·652, a sufficiently close approximation to the theoretical number 1·66. In the case of argon, the purest specimen obtained gave for the ratio 1·659; and as remarked (in the *Philosophical Transactions*, 1895, 52), not much dependence can be placed on the accuracy of the last figure.

The result of these experiments goes to prove that the density of the gas named helium is not less than 2·13, and that it has the same claim to be considered a monatomic gas as mercury gas; or if it is a mixture, it must be a mixture of monatomic gases.

As hydrogen was often evolved along with helium from minerals, it occurred to us that if a definite ratio could be found between the helium and the hydrogen evolved by the action of acid, some idea might be gained as to the valency of helium. It would be as if, for example, hydrogen and chlorine were evolved separately from salt by sulphuric acid, instead of in combination; by measuring each, the deduction could be drawn that chlorine was univalent. Experiments made to this end showed, however, that from some minerals no hydrogen is evolved. Gas, from a sample of uraninite sent by Dr. Hillebrand, contained no trace of hydrogen. It is, of course, possible, and, indeed, not unlikely, that all hydrogen is absorbed in reducing the uranic oxide to uranous oxide. The problem then becomes a complicated one; but we hope to solve it by future experiments.

As yet but few experiments have been made with the object of inducing helium to enter into combination. Like argon, it is not attacked by oxygen in presence of caustic soda under the action of the electric discharge; indeed, this forms a good method of removing all impurities other than argon. Again, like argon, it is not affected by red-hot magnesium, and it is not oxidised by copper oxide at a red heat.

As helium is evolved from clèveite and similar minerals at a red heat, an attempt was made to reabsorb it by heating the powdered mineral to redness in contact with the gas, but not to so high a temperature as that which had served to cause it to be evolved. But the attempt was fruitless; no gas was absorbed. When all the gas in the tubes had been pumped out, after they were cold, heating failed to cause the evolution of more gas.

A further experiment was made, in which metallic uranium was heated to bright redness with a blow-pipe in contact with a mixture of helium and oxygen, the latter gas being greatly in excess. But, curiously, the oxidation of the uranium was very slow, and all the helium was recovered, none having been absorbed. The conditions have yet to be discovered under which helium can be made to combine with oxides of uranium, so as to reproduce the natural product.

*The Solubility of Helium.*

Helium is very sparingly soluble in water. A determination made by the method previously described for argon (*Phil. Trans.* A, 1895, 37) gave 0·0073 as its coefficient at 18·2°. The tube contained 162·3 arbitrary divisions, of which 26·0 were occupied by helium and 136·3 by water. After shaking, the volume of the helium was reduced to 25·0 divisions, and that of the water was increased to 137·3. As 137·3 absorb 1·0, 1 volume of water absorbs 0·0073 volume. The whole apparatus was jacketed with running water during this experiment.

This is the lowest solubility hitherto recorded. Generally speaking, the solubility of a gas is related to the temperature at which it condenses to a liquid, and the sparing solubility of helium points to its having a very low boiling point. Prof. Olszewski has kindly undertaken to make experiments on the temperature of liquefaction of helium, and it will be interesting to find whether its boiling point does not lie below, or, at least, as low as that of hydrogen; for their molecular weights are not very different, and helium is a monatomic gas, a condition which appears to lower the boiling point.

Helium is totally insoluble in absolute alcohol and in benzene.

*The Spectrum of Helium.*

Mr. Crookes is making an exhaustive study of the spectrum of helium, and will shortly publish an account of his work. But, as some of the deductions to be drawn later depend on the lines observed, it is necessary here to add a few words. In general terms, the spectrum has already been described. The particular point to which attention is necessary here is that at least two of the lines in the spectrum of helium, seen with a wide dispersion prism, are coincident with two of the argon lines. These occur in the red, and comprise one of each of the two pairs of characteristic argon lines. This observation has been frequently repeated, using for the purpose spectroscopes of different dispersive power, and throwing into the field both spectra at the same time, with an exceedingly narrow slit; and we may say that if not absolutely identical, the lines are so near that it is not possible with the means at our disposal to recognise any difference in position. But the relative brilliancy is by no means the same. One of the argon lines, rather faint, is coincident with the prominent red of the helium spectrum, and one of the strong red argon lines is coincident with a faint red line in the helium spectrum.

Besides these two, there is a line in the orange-red, which though perhaps not identical, yet is very close. This line is faint in helium, but moderately strong in argon. It is much more easily visible with helium in the "negative glow" than in the capillary tube.

It may also be of interest to state that, according to Runge's observation, the brilliant yellow line of helium is undoubtedly a doublet. This was frequently observed by us with a grating of 14,000 lines to the inch in the spectrum of the third order. But it must also be noted that one of the lines is very faint; the other, more refrangible, is immensely brighter. The distance, judged by eye, appears to be about 1/50th part of that between the lines $D_1$ and $D_2$ of sodium. Accurate information on this last point may be looked for from Mr. Crookes, Mr. Lockyer, and from many others who are interested in the probable occurrence of this element in the sun.[1]

III. *General Conclusions.*

It cannot be doubted that a close analogy exists between argon and helium. Both resist sparking with oxygen in presence of caustic soda; both are unattacked by red-hot magnesium; and if we draw the usual inference from the ratio

[1] Prof. Hale and Dr. Huggins have recently observed that the solar line $D_3$ is also a doublet. (W. R., July 20).

between their specific heats at constant volume and at constant pressure, both are monatomic gases. These properties undoubtedly place them in the same chemical class, and differentiate them from all known elements.

Although opinion is divided on the precise significance of the ratio of specific heats, 1·66, it appears to be most probable that in all cases, as in that of mercury, this ratio points to the monatomicity of the molecule. If we assume this provisionally, it follows that the atomic weight of helium is identical with its molecular weight. The molecular weight is twice the density, for the molecular weights of gases are compared with the atomic weight of hydrogen, taken as unity; hence the atomic weight of helium on this assumption is $2{\cdot}13 \times 2 = 4{\cdot}26$. But again we assume, in making this calculation, that helium is a single element, and not a mixture of elements. Before discussing this question, it appears advisable to inquire whether there is any evidence which would corroborate the deduction that it is a monatomic element. This evidence must be sought for in the properties of argon, for those of helium have not as yet been sufficiently investigated.

We know from countless examples among compounds of hydrogen and carbon that increase in molecular weight is accompanied by rise of boiling point; and it may be stated as a proved fact that a polymeride has always a higher boiling point than the simpler molecule of which the polymeride is formed. Among the substances germain to this inquiry, ozone and oxygen may be cited; the complex molecule of ozone is shown by the higher temperature at which it boils. It might be concluded with certainty, therefore, that $A_2$, could it exist, should have a higher boiling point than $A_1$.

Next, it is generally the case that the boiling point of an element, provided it has not a complex molecule like that of sulphur and phosphorus, is lower, the lower its molecular weight. There are the well-known instances of chlorine, bromine, and iodine; but if it be objected that these all belong to the same group, we may cite the cases of hydrogen, $-243.5°$; nitrogen, $-194.4°$; and oxygen, $-182{\cdot}7°$; and we may add chlorine, $-102°$. If argon possessed the atomic weight 20 and the molecular weight 40, it is probable that its boiling point would lie above that of chlorine, instead of, as is actually the fact, at $-187°$—below that of oxygen. But, it may be objected, the boiling point is determined, not by the molecular weight, but by the density. It may be urged that the density of argon is 20, and that its molecules, like those of oxygen and nitrogen, are diatomic, in spite of the argument to the contrary from the ratio of specific heats. The answer to this objection is obvious; if this were so, its boiling point should lie above, and not below that of oxygen.

These considerations cannot, of course, be accepted as evidence, but merely as corroborative of the conclusion as regards the monatomicity of argon. If they apply to argon, they apply with equal force to helium; and if they are accepted, it follows that the atomic weight of helium is 4·26.

It is again necessary to consider the character of argon in attempting to answer the next question: Are argon and helium single elements or mixtures of elements? But before discussing it, let us consider another question: How does argon happen to occur in the air and helium only in minerals? Why is helium not present in air? A satisfactory answer to this question is, we think, contained in a paper by Dr. Johnstone Stoney (*Chem. News*, 1895, lxxi. 67). He there shows that were hydrogen to be present in air (and it might be present, in spite of the oxygen with which it could be mixed, for a small quantity would surely escape combination), it would, in virtue of the velocity of its own proper molecular motion, remove itself from our planet, and emigrate to a celestial body possessing sufficient gravitational attraction to hold it fast. Dr. Stoney suggests this explanation to account for the absence of an atmosphere and of water vapour on the moon, and for the presence of an atmosphere of hydrogen on the sun. It would also account for the absence of helium in our atmosphere, and for the presence of the chromospheric line $D_3$. Of course if an element can form compounds, or if it is absorbed by solids, as helium appears to be, it will, like hydrogen and helium, be found on the earth.

The inertness of these gases would favour their existence in the free state. And argon exists in the atmosphere, precisely because it forms no compounds. Similarly nitrogen is a constituent of air, because in the first place those elements with which it combines directly are comparatively rare, and also because such compounds are mostly decomposed by water; and the excess of nitrogen therefore occurs in the free state. Similarly, the occurrence of free oxygen is due to the fact that some remains over, after all or almost all the readily oxidised substances have already united with oxygen. If there exist gases similar to argon in inertness, they too may be looked for in air.

Now if argon possess the atomic weight 40, there is no place for it in the periodic table of the elements. And up to now there is no exception to this orderly arrangement, if the doubtful case of tellurium be excluded. Rayleigh and Ramsay have shown that the high density of argon can hardly be accounted for by supposing that molecules of $A_2$ are mixed with molecules of $A_1$; and excluding as untenable the supposition that argon is a compound, the only remaining suggestion is that it is a mixture. No attempts have as yet been made to test the correctness of this idea; but experiments have already been started which, it is hoped, will throw light on this question.

The density of argon is too high; to fill its place in the periodic table, between chlorine and potassium, its density should be about 19 and its atomic weight 38. We might expect the presence of another element with a density of 41 and an atomic weight of 82, to follow bromine, as argon follows chlorine; and this element would probably also be a gas, since its density would be only a little higher than that of chlorine.

But here we meet with a difficulty. There are certain lines in the spectrum of helium coincident with lines in the argon spectrum. There can be only one explanation, excluding the extremely improbable hypothesis, which is not verified in any instance, that two elements may give spectra containing identical lines. That explanation is, of course, that each contains some common ingredient; and there appears to be a place for one with density 10 and atomic weight 20, to follow fluorine in the periodic table. The density of helium is, however, so low, that there does not appear room for any large quantity of a heavier gas; and to fit the periodic table, the density of argon should be diminished by removal of a heavier admixture, rather than increased by removal of a lighter one.

Such are the problems which now confront us. Until more experiments have thrown further light on the subject, we regard it as labour lost to discuss the relations of these curious elements to others which find their proper place in the periodic table.

## UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Sir Julian Goldsmid has been elected Vice-Chancellor of the University of London, in succession to Sir James Paget, who has resigned.

The new *Directory* of the Department of Science and Art, which has just come to hand, contains the regulations for Organised Science Schools, previously referred to in these columns. Among other matter new to the *Directory*, and announcements of changes, we notice that a new method of according the National Scholarships is in contemplation. The change will not take effect until the Session 1896–97, and due intimation of its nature will be given. The syllabus of Practical Plane and Solid Geometry has been recast in the elementary stage, in the direction already noted, and new syllabuses are given for Inorganic Chemistry, theoretical and practical, Geology, and Physiography. It is not clear, however, whether the questions to be set for the examinations next May will be based upon the new or the old syllabuses.

At the ordinary quarterly meeting of the Royal College of Physicians of London, held on Thursday last, Sir Russell Reynolds, F.R.S., in the chair, the following gentlemen were elected officers of the College:—Censors, Sir William H. Broadbent, Dr. P. H. Pye-Smith, Dr. T. Tillyer Whipham, Dr. William Cayley; treasurer, Sir Dyce Duckworth; emeritus registrar, Sir Henry Pitman; registrar, Dr. Edward Liveing; librarian, Dr. William Munk; examiners—chemistry and chemical physics, Mr. Charles E. Groves, F.R.S., Mr. W. R. Dunstan, Mr. J. Millar Thomson, Dr. Samuel Ricleal, Dr. R. Taylor Plimpton; materia medica and pharmacy, Dr. T. Lauder Brunton, F.R.S., Dr. Daniel J. Leech, Dr. Sidney P. Phillips, Dr. Frederick Willcocks, Dr. Francis G. Penrose;

elementary biology, Mr. F. Gymer Parsons, Mr. P. Chalmers Mitchell; elementary physiology, Dr. H. Lewis Jones; physiology, Dr. Vincent D. Harris, Dr. Thomas Oliver, Dr. Frederick W. Mott; anatomy, Mr. Charles Stonham, Prof. G. Dancer Thane; medical anatomy and principles and practice of medicine, Dr. Philip J. Hensley, Dr. J. Burney Yeo, Dr. G. Vivian Poore, Dr. J. Mitchell Bruce, Dr. Frederick Taylor, Dr. Stephen Mackenzie, Dr. William Ewart, Dr. Seymour J. Sharkey, Dr. J. Kingston Fowler, Dr. Robert Saundby; midwifery, Dr. J. Baptiste Potter, Dr. J. Watt Black, Dr. Peter Horrocks, Dr. Walter S. A. Griffith; surgical anatomy and principles and practice of surgery, Mr. John Langton, Mr. J. N. C. Davies-Colley; public health, Dr. Charles H. Ralfe, Dr. William Pasteur; Murchison Scholarship, Dr. F. Charlwood Turner, Dr. Samuel H. West.

WE gave last week the names of the Research Scholars appointed for 1895, by Her Majesty's Commissioners for the Exhibition of 1851. We are now informed that the following scholars, appointed in 1894, have forwarded satisfactory reports of their work during the first year of their scholarships, which have accordingly been renewed for a second year.

| Name of Scholar. | Nominating Institution. | Place of Study. |
|---|---|---|
| J. C. Beattie ... ... | University of Edinburgh | University of Vienna. |
| J. R. E. Murray ... | University of Glasgow ... | University of Glasgow. |
| W. B. Davidson ... | University of Aberdeen... | University of Würzburg. |
| R. C. Clinker... ... | University College, Bristol ... ... ... | University College, Bristol. |
| F. Dent ... .. | Yorkshire College, Leeds | University of Munich. |
| A. J. Ewart ... .. | University College, Liverpool ... ... | University of Leipzig. |
| D. K. Morris... ... | University College, London ... ... ... | University College, London. |
| J. Frith ... ... | Owens College, Manchester ... ... .. | Owens College. |
| R. Beattie ... .. | Durham College of Science ... ... ... | Durham College of Science. |
| W. B. Burnie... ... | University College, Nottingham ... ... | Central Technical College. |
| J. A. McClelland ... | Queen's College, Galway | Owens College. |
| F. B. Kenrick ... | University of Toronto ... | University of Leipzig. |
| F. J. A. McKittrick | Dalhousie University, Halifax, Nova Scotia... | Cornell University. |

*Note*.—Such of the above Scholars as remained at the nominating Institution for the first year will now proceed to another Institution in England or abroad.

The following scholars, appointed in 1893, have been selected for exceptional renewal for a third year:—

| Name of Scholar. | Nominating Institution. | Place of Study. |
|---|---|---|
| H. W. Bolam... ... | University of Edinburgh | University of Leipzig. |
| J. W. Walker... ... | University of St. Andrews ... ... | Universities of Leipzig and St. Andrews. |
| J. E. Myers ... ... | Yorkshire College, Leeds | University of Strassburg |
| E. C. C. Baly... ... | University College, London ... ... ... | University College, London. |

## SCIENTIFIC SERIALS.

*American Meteorological Journal*, July.—The geographical distribution of the maximum and minimum hourly wind velocities . . . for January and July, for the United States, by Dr. F. Waldo. This discussion is based on the Signal Service and Weather Bureau observations, and the subject is treated in various ways, and illustrated by wind charts. We select from these (1) the hour of maximum wind and (2) the maximum hourly wind, in miles per hour. There is no great regularity in the time of occurrence of the strongest wind; in January it occurs on the Atlantic coast from 2h. to 4h. a.m., and on the North Pacific coast it is retarded to 6h. a.m. On the Gulf of Mexico it takes place about noon, while at inland stations it occurs generally about 2h. p.m. In July, on the Atlantic coast, there is a maximum wind about 2h. p.m. in latitude 45°, but with southward progress it is retarded, until in latitude 30° the hour is changed to 6h. p.m. In the southern part of the Pacific coast, the time of maximum is 1h. p.m., which is much earlier than for the adjacent inland or the northern part of the coast. In general, for the inland north-east the hour is 2h. p.m., and there is a retardation with both western and southern progress. In January the maximum hourly wind reaches a velocity of seventeen miles on the northern parts of the Atlantic and Pacific coasts, decreasing with southward progress, while the inland distribution shows a maximum of ten to thirteen miles per hour over the Great Plains. In July, the maximum hourly wind is eleven to thirteen miles on the Atlantic coast, while on the North Pacific coast there is a very small maximum (eight miles), but this is counterbalanced by the very high velocity of eighteen miles per hour on the central Californian coast. A reference to the wind charts shows the prevailing conditions much better than any verbal description can do.

*Bulletin of the American Mathematical Society*, No. 9. (June 1895, New York).—Mr. J. de Perott gives a very interesting sketch of Euclidian arithmetic in connection with a notice of the late M. Stieltjes' contribution to the *Annales de la Faculté des Sciences de Toulouse*, vol. iv., entitled "Sur la théorie des nombres." M. Stieltjes had it in contemplation to write an extensive treatise on the theory of numbers, but unhappily his weak health and final untimely death prevented his getting beyond the paper noticed by Mr. de Perott. This paper is devoted to a greatly generalised form of Euclid's work. "It does not insist on the definition of number, nor on the laws which are at the base of the operations we perform on numbers, but passes immediately to the exposition of the chief properties of the least common multiple and the greatest common divisor of numbers. . . . Poinsot was the first, I think, to whom it occurred that the course could be reversed." The results are expressed in a very symmetrical form by the author of the note. —Mr. G. L. Brown writes a short note on Hölder's theorem concerning the constancy of factor-groups, and Prof. F. Morley a like note on the theory of three similar figures. The theory has been recently given in the sixth edition of Casey's "Sequel to Euclid," and also in the second edition of his "Conics." Prof. Morley believes that something is to be said in favour of an appropriate analytic handling of the theory, and gives here some preliminary equations in a convenient form.

*Bollettino della Societa Sismologicá Italiana*, I., 1895, No. 3.—Microseismograph for continuous registration, by Prof. G. Vicentini (see p. 178.)—New type of seismic photochronograph and its applications, by A. Cancani. A description of an instrument by which the face of a chronometer is photographed at the moment of the shock or of the arrival of long-period pulsations from a distant earthquake.—Review of the principal eruptive phenomena in Sicily and the adjacent islands during the four months January-April, 1895, by S. Archidiacono.—The Viggianello (Basilicata) earthquake of May 28, 1894, by M. Baratta. An account of an interesting tectonic earthquake. The meizoseismal area, which is elliptical and only about 17 km. long, is restricted to the northern slopes of M. Pollino. This group of mountains represents the northern half of a vast ellipsoid of dolomites and limestones, traversed by great fractures, which, if produced, pass through Rotonda and Viggianello, the towns most damaged by the shock.—Notices of Italian earthquakes (February-April, 1895).

## SOCIETIES AND ACADEMIES.

### PARIS.

**Academy of Sciences**, July 22.—M. Marey in the chair.—Researches on the composition of grapes from the principal French vines, by MM. Aimé Girard and L. Lindet.—On the osmotic phenomena produced between ether and methyl alcohol across different diaphragms, by M. F. M. Raoult. It is found that with ether and methyl alcohol on the respective sides of a diaphragm of pig's bladder, the methyl alcohol passes by osmosis to the ether side. The bladder membrane appears to be impermeable to ether; even with mixtures the transference is always of methyl alcohol towards the side where it is of less concentration. Exactly the reverse occurs with a vulcanised caoutchouc membrane, which is impermeable to methyl alcohol, but permeable to ether. The experiments show: (1) that osmosis between two determined liquids may not only vary much in energy, but even change its sense with the nature of the diaphragm; (2) that the osmotic movement of substances across the diaphragm may be absolutely independent of their molecular weights and of their condition as dissolved substance or solvent.—Action of phenyl isocyanate on some acids and ethereal salts, by M. A. Hallèr.—M. Retzius was elected Correspondant of the Anatomy and Zoology Section, in succession to M. Carl Vogt.—Abnormal refractions at the surface of water, by M. Ch. Dufour. Attention is directed to a source of error, due to irregular refraction caused by differences in temperature between water and air immediately above its surface, which may arise in taking the latitude or determining time at sea.—On static or dynamic explosive potentials, by M. R. Swyngedauw. According to the experiments described, the explosive potential between two poles shielded from ultra-violet radiations is not appreciably diminished by very small and very rapid variations of potential.—On a phosphorescence phenomenon obtained in tubes containing rarefied nitrogen after the passage of the electric discharge, by M. Gaston Séguy. In presence of vapours of stannic chloride, the author finds the light emitted from a nitrogen tube to be rose-coloured during the discharge, and milky white for some 10 to 80 seconds after interruption of the current.—On the electromotive force of the Latimer Clark, Gouy, and Daniell standards, by M. C. Limb. The values found by the author's method for the elements at 0° C. are: Latimer Clark 1·4535 volts (absolute), Gouy 1·3928 volts (abs.), Daniell (Fleming type) 1·0943 volts (abs.).—On Natterer's tubes, by M. Gouy.—On anhydrous crystallised manganese sulphide, by M. A. Mourlot. Crystallised sulphide, identical with *alabandine*, has been obtained by means of the electric furnace. Small cubes or transparent derived octahedra of a greenish shade are obtained. They have the density 3·92 and hardness 3·5 to 4.—On some properties of combinations of ferrous chloride and nitric oxide, by M. V. Thomas. The experiments detailed show that the three compounds obtained by the author in the dry way possess no appreciable tension of dissociation at the ordinary temperature, and hence differ from the compounds obtained in solution by M. Gay.—On some alkaline phosphides, by M. C. Hugot.—Specific heats of superfused formic and acetic acids. Modifications applied to Regnault's thermocalorimeter to enable the determination of the specific heats of a large number of superfused liquids, by MM. Massol and Guillot. The specific heats of formic and acetic acids in the solid state are much greater than their specific heats in the liquid state. The specific heat in the liquid state diminishes with the temperature. When superfused, the specific heat is slightly augmented, but remains of the same order as the specific heat in the liquid state.—Synthetic formation of nitro-alcohols, by M. Louis Henry.—Oxidation of inactive campholenic acid, by M. A. Béhal.—On the constitution of vegetable albumenoid substances, by M. E. Fleurent.—Influence of respiration on the volumetric trace of the limbs, by MM. A. Binet and J. Courtier.—Modifications of the heat radiated produced by faradisation, by M. L. Lecercle. An account of the local rise in temperature produced in animals by electric excitation, and its effect on the general temperature.—Aggravation of the effects of certain microbe toxines by their passage through the liver, by MM. J. Teissier and L. Guinard.—A contribution to the histology of unicellular glands, by MM. J. Kunstler and A. Gruvel.—On the evolution of the magmas of certain amphibole granites, by M. A. Michel Levy.—On the first alcohol thermometer used in Paris, by M. l'Abbé Maze.

BERLIN.

**Physiological Society,** June 7.—Prof. Munk, President, in the chair.—Prof. Baginski reported on experiments made, in conjunction with Dr. Sommerfeld, on bile from 115 children. Analysis showed that, in comparison with the bile of adults, it contained more water and mucin and less bile-salts. It contained no urea or ethereal sulphates, and in the case of children who had died of diphtheria it was free from bile-salts. Examination of the urine of children suffering from various forms of nephritis showed that it contained an abnormally large amount of xanthin compounds, which could not be accounted for by any breaking down of epithelial cells or blood corpuscles. Dr. Benda described longitudinal bands in the mucous membrane of the true vocal cords of man, which cause corresponding furrows in the inner side of the epithelial layer. They can be readily brought into view by the removal of the epithelium by macerating in dilute acetic acid. The bands are shorter than the vocal cords, and are pointed at each end. Karyokinetic cell-division can often be seen taking place in the epithelial layer.

June 21.—Prof. du Bois Reymond, President, in the chair.—Dr. Schulz spoke on the anatomy of unstriated muscles in vertebrates. He finds that they consist of elongated cells, pointed at each end, whose length is very variable in different animals. Each cell consists of fibrils imbedded in a highly refractive interfibrillar substance, and of granules and a nucleus in the middle of the cell with two nuclear bodies. Two nuclei in one cell were only seen once among thousands of preparations. The fibrils interlace with each other. The separate cells are not held together by any cement-substance, but by protoplasmic threads and branches. The transverse striation described by many observers appears to be due to a wrinkling of the cell resulting from incomplete extension after having been contracted. Nerve fibres are very plentiful. With methylene-blue, gold chloride, or by Golgi's method numerous ganglion-cells can be brought into view, from which short branches are distributed to the muscle cells. In addition to these numerous nerve-fibrils can be seen ending in minute bulbous swellings which are applied to the muscle. The nerves are sensory as well as motor.—Dr. Cohnstein reported experiments on injecting solutions of sugar into the blood-vessels, in support of his views on the formation of lymph in opposition to Heidenhain. The results were the same as on the injection of salt solutions. The amount of sugar in the blood rose and fell very rapidly, whereas it rose and fell very slowly in the lymph. The maximum of sugar observed in the lymph was equal to the maximum met with at an earlier stage of the experiment in the blood. The solid constituents of the blood became less after the injection, and then increased slowly to the normal; in the lymph, on the other hand, they increased at first and then became less. After the injection of sugar the blood capillaries of a frog's web were considerably dilated and the circulation quickened. Dr. Cohnstein interpreted these results as indicating an initial passage of water from the intercellular spaces into the blood-vessels, followed at a later stage by a return filtration into the lymph. He had also observed a diminution in the secretion of bile after the injection of sugar, and attributed this to compression of the bile capillaries resulting from dilatation of the blood capillaries.

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