THURSDAY, AUGUST 23, 1888.

BRITISH PETROGRAPHY.

British Petrography: with Special Reference to the Igneous Rocks. By J. J. Harris Teall, M.A., F.G.S. With Forty-seven Plates. (London: Dulau and Co., 1888.)

THIS handsome volume, with its beautifully chromolithographed plates, supplies a want that has long been felt in English scientific literature. It was scarcely fitting that in this country, where the application of the microscope to the study of thin sections of rock was first suggested and practically carried out, there should exist no comprehensive work dealing with the chief varieties of our native rocks, as illustrated by their microscopic characters.

In its general appearance, plan, and scope, this volume reminds one so closely of the "Minéralogie Micrographique: Introduction à l'Étude des Roches Éruptives Françaises," of MM. Fouqué and Michel Lévy, that it is scarcely possible to avoid a comparison between the two works. Artistically, the forty-seven plates of the English treatise may perhaps even claim superiority over the fifty-five plates in the French work; though in the exact presentation of minute but characteristic details, and in the accuracy of tints employed, the palm must in some cases be awarded to the latter. There are some plates in the volume before us, however, in which truthful delineation of details has been so admirably combined with a general beauty of effect as practically to leave nothing to be desired in work of this class.

Like his French predecessors, the author of this volume has found it desirable to go outside of the country illustrated for a few of his types of igneous rock. A striking testimony, however, to the variety as well as the beauty of our native rocks is found in the circumstance that it has been possible to present so complete a selection of the chief types of igneous materials without going beyond the limits of the British Isles except in two instances,—those, namely, of the Lherzolite of the Ariège, and of the Lencitic rock of the Eifel. Nor are the varieties of British igneous rocks by any means exhausted in the illustrations of the work before us. The rhyolites, which perhaps are less adequately represented than some other groups, might have had their more crystalline varieties (Nevadites) well illustrated by the beautiful rocks of Tardree, Co. Antrim, while examples of trachyte of graphic-granite, and of various types of granulites and "trap-granulites," might have been easily obtained from Scotland. On the whole, however, we think the author has shown excellent judgment in his selection of types, and he is to be heartily congratulated upon his success in, securing accurate drawings, and exact reproductions of those drawings by the process of chromolithography—results which we are assured could not have been attained without much labour and extreme care.

Although the book is one which is especially noteworthy for the beauty of its illustrations, it would be a mistake to suppose that it belongs to that class of works in which everything else is sacrificed to showy plates, and scientific accuracy is regarded as merely a secondary object. On the contrary, the author has clearly devoted great pains to the perfecting of his text, which constitutes in itself an excellent introduction to the study of petrography. Some of the rocks chosen for illustration have already been described by other authors, and in these cases Mr. Teall, while doing full justice to the labours of his predecessors and contemporaries, has not unfrequently been able to extend, supplement, or correct their results by the light of more recent researches; in the case of rocks which have not been previously described, the author has himself investigated their chemical and microscopical characters, in some instances in a very complete and exhaustive manner. In all cases he has earned the gratitude of students by the copiousness of his references to the ever-growing mass of literature which deals with the question of the minute structure of minerals and rocks.

While MM. Fouqué and Lévy have devoted the text of their work to a systematic description of the various species of rock-forming minerals, and especially of those characters which enable us to recognize them when seen in thin sections under the microscope, the author has aimed rather at describing the rocks themselves, incidentally discussing the characters of each species of mineral as it presents itself in the different groups of rocks. This plan, while attended with certain advantages, may perhaps be objected to on the ground that it is only possible to gather the whole of the conclusions of the author upon any particular mineral after consulting different and widely-separated portions of the book. This is rendered more easy, however, by the very full index which is supplied.

The work, we are informed in the preface, was commenced as a serial publication, and to this cause probably must be ascribed its most serious defect as a means of instruction: this is the absence of references and cross-references between the text and the atlas of plates; these, indeed, constituting two practically independent works. Had all the plates been before the author during the time that he was preparing the text, he would frequently have been able to illustrate his remarks upon the minerals and structures in the rocks he is describing by references to his own admirable drawings. To the same cause, too, we must ascribe the only other serious blemish we have detected in the book—a rather large proportion of misprints, which, though usually obvious enough to the initiated, may occasion considerable embarrassment to the student.

However much the beginner, taking up this attractive volume, may be delighted with the mode of study of which it aims at giving an exposition, he will scarcely be led into the fatal error of supposing that everything necessary to a person seeking to employ the method is a microscope and some rock-sections. The author makes it perfectly clear that unless the student is prepared to go through a certain amount of preliminary training, the microscopic examination of a rock is more likely to lead to error rather than to truth. So much knowledge of crystallography as will enable the observer to appreciate the position of any section with respect to the axes of the crystal, and such an acquaintance with the principles of physical optics as will suffice to guide him in interpreting the chief phenomena revealed, when either plane or convergent polarized light are employed, are absolutely indispensable. But in addition to these there is a vast mass of knowledge, which has been gradually acquired and is ever increasing, concerning the internal peculiarities of minerals, especially such as appear in the varieties that constitute rocks, and with respect to the wonderful series of changes which they undergo when exposed to different conditions; and the more of this kind of knowledge the student can bring to the investigation of a rock the less liable will he be to fall into error. In this branch of science, as in every other, the experience which can only be obtained by long-continued study of the subject must always supplement, and may sometimes even supersede, the results obtained by the application of rigid rules of of procedure.

As a suggestion has recently been made in the pages of NATURE that all which is required to secure a uniform and uniformly-acceptable classification and nomenclature of rocks is that some master of the modern methods of research should bring in a sweeping "reform bill" on the subject, it may be well to quote the author's views upon petrographic notation and classification. Writing after the two years of careful labour devoted to the preparation of this work, he remarks:—

> "As regards the classification of rocks, I am sorry to say that increasing knowledge has not tended to bring about any clearness of view. The more rocks are studied the less they seem to me to adapt themselves to any classification at all comparable in definiteness with the classifications of organic bodies and mineral substances. Rock-masses often vary so much in composition and structure that any scheme of classification based on work done in the laboratory is unsuitable for the expression of broad geological facts. It is absolutely impossible to map the different varieties recognized by modern petrographers. The conclusion at which I have arrived is that the necessity for giving names to rocks arises rather from work done in the field than from work done in the laboratory. Rock specimens are mineral-aggregates, and may be described as such. Rock-masses are integral portions of the earth's crust, and possess a certain amount of individuality in virtue of their mode of occurrence."

With these remarks we very cordially agree. Systematic mineralogy is a branch of natural-history science; for, in their crystalline forms and chemical constitution, minerals supply safe criteria which enable us to define species and varieties, and also permit us to group these into larger divisions. But most petrographical classifications seem to be of value only so long as we confine our attention to the selected fragments that fill the cases in a petrographical museum. In the field one type is often found passing into another which the mere petrographer may have placed in a totally different class.

There is perhaps just now a danger of our exaggerating the importance of the microscopic method as applied to the study of rocks. That the method has already done much in enabling us to follow out and trace the effects of the slow processes of change within the earth's crust, and that it will do still more in the future, no one can doubt. But when it is sought to make the microscope a "court of final appeal" in geological questions, and in doing so to disregard the importance of field-observation, we perceive the same source of danger as is now perhaps being experienced in connection with almost every branch of natural-history research. It must be remembered that, while the microscope enables us to see a little more than the naked eye or the pocket lens, yet nevertheless, between what is actually seen by the very highest powers of our microscopes and the molecular groupings and reactions which give rise to the varied phenomena of the mineral kingdom, there is room for almost infinite possibilities. We accept the teaching of the microscope with all thankfulness, but we recognize the fact at the same time that it has enabled us to get only a very little nearer to the heart of those great physical problems which we aim at solving.

In congratulating the author upon the completion and publication of a book which, as we learn from his preface, has occasioned him no little anxiety as well as so much labour, we may express the hope that his project of treating the aqueous and metamorphic rocks in the same attractive and thorough fashion may be realized. We cannot conclude this notice without a word of commendation for the excellent glossary of terms used in describing rocks, which has been supplied by Dr. F. H. Hatch, and will, we are assured, prove of the greatest service to students. JOHN W. JUDD.

SILKWORMS.

Silkworms. ("Young Collector Series.") By E. A. Butler, B.A., B.Sc., Author of "Pond Life: Insects," &c. (London: Swan Sonnenschein, Lowrey, and Co., 1888.)

THE silkworm is so familiar an insect to everyone, and is interesting from so many points of view, that we gladly welcome this small volume from the pen of a well-known writer on popular natural history. The space which can be allotted to this subject in works on general zoology, or even on general entomology, is necessarily small; and when we consider that a whole library could be written on the history and structure of any single insect, a book dealing almost exclusively with *Bombyx mori* should be a useful addition to our entomological literature. The present work is fairly comprehensive in its scope, and is written in such a manner as to be intelligible to everyone, however ignorant of natural history. Numerous woodcuts are added, wherever they seem to be required to elucidate the text.

Mr. Butler appears to be adequately acquainted with his subject, and we have glanced through his book without noticing any very serious errors, or meeting with many statements which we felt disposed to question. But we can hardly accept the inconceivable narrative which Mr. Butler has copied from the *Entomologist* on pp. 78 and 79, about a male and female moth being developed upside down in a single pupa formed by a single larva. Until more instances of a similar nature are recorded, we fancy that most charitably-disposed people will be inclined to imagine that some extraordinary error must have occurred. In this case, and in a few others, Mr. Butler quotes his authorities. Although it would be unfair to expect the author of a work like the present to quote authorities throughout, we think that it would have been more satisfactory to Mr. Butler's readers, especially to those who may wish to go further into the subject, if he had indicated in a brief preface the chief sources from whence he had derived his information, and how far portions of it were based upon his own observations.

We must take exception to one statement (on p. 79) as rather too sweeping. "Silk-producing *Lepidoptera* belong exclusively to two families, the *Bombyridæ* and the *Saturniidæ*." All, or very nearly all, *Lepidoptera* produce more or less silk; but even if we understand Mr. Butler to mean "all *Lepidoptera* which produce silk of economic value," he would still have spoken too positively, for we believe that various species belonging to the *Lasiocampidæ*, and perhaps to other families of Bombyces, have been used as silk-producers in various countries; as, for example, *Libethra cajani* in Madagascar.

Mr. Butler has divided his work into six chapters. The first treats of "The History of Silk Culture," and contains a sketch of the gradual progress of silk-culture and manufacture, and of the introduction of these industries into one country after another, from their commencement in China, according to tradition, about 2600 B.C., to the present time. One point seems to have been overlooked, viz. the modern origin of the name Morea for the Peloponnesus, and its derivation from the mulberry-tree.

The second chapter, "The Silkworm: its Form and Life-History," deals with the metamorphoses, and the external structure and changes of the insect in its various stages. The mode of denuding the wings to examine the neuration; parthenogenesis, and other incidental matters, are likewise noticed. Mr. Butler objects to the term "nervures" as applied to the branching tubes which traverse the wings of butterflies and moths; but we may be permitted to point out that such terms, when used in a purely technical and conventional manner, though frequently incorrect in themselves, rarely mislead anyone.

Chapter III., "The Silkworm: its Internal Structure," treats, of course, of internal anatomy. Detailed directions are given for dissecting silkworms. The chapter closes with remarks on Lyonnet's great work on the anatomy of the larva of the goat-moth, and with a detailed explanation of the position of *Bombyx mori* in the system of Nature.

Chapter IV., "The Silkworm: its Rearing and Management," notices some of the principal races of silkworms, the manner of rearing them, and the mode of preparing the silk. The last paragraph briefly alludes to some allied species of true *Bombyx*.

Chapter V. deals with "The Silkworm: its Diseases and Imperfections." The three most serious diseases, flaquerie, muscardine and pebrine, are discussed rather fully, as well as M. Pasteur's method of combating pebrine by microscopic examination of brood females.

In the concluding chapter (VI.), the author discusses "Wild Silkworms," many of which he figures. His treatment of this part of the subject is necessarily somewhat brief, but this is the less to be regretted, as those who wish for further information will probably find much of what they require in Mr. Wardle's "Hand-book of the Collection illustrative of the Wild Silks of India, in the Indian Section of the South Kensington Museum." This book was published by the Science and Art Department of the Committee of Council on Education in 1881; and though earlier in date, it will be found a most useful appendix to Mr. Butler's work.

Mr. Butler himself may fairly be congratulated on his success in compressing so large an amount of useful matter as his book contains into the moderate compass of just 100 pages. W. F. KIRBY.

OUR BOOK SHELF.

Allgemeine Geologie. Von Dr. Karl von Fritsch, Professor an der Universität in Halle. (Stuttgart: J. Engelhorn, 1888.)

THIS is one of a very useful series of volumes which is appearing under the editorship of Dr. Friedrich Ratzel, with the title of "Library of Geographical Handbooks." As the subjects of glaciers and of volcanoes and earthquakes have had special volumes of the series devoted to their discussion, while many other problems of geological interest are treated of in separate monographs, such as those which deal with the geography of the ocean, and the morphology of the earth's surface, Dr. von Fritsch has been able to limit the scope of the work now before us to certain definite lines of inquiry. The first division of the book is devoted to "Geophysiography," or a discussion of the features of the earth as a member of the solar system, and of the relations of the atmosphere and ocean to the lithosphere or solid crust of the globe. The second division, "Geotektonik," deals with the forms and relations of the rock-masses that build up the solid crust, and is treated with considerable fullness, the illustrations being for the most part new, and not of the kind which find a place in the ordinary text-books of geology. In the third part, "Geochemistry," or chemical geology, we have a short sketch of the present state of petrography, or the description of rocks, followed by remarks on petrogeny, or the theory of their origin. It would be unfair to expect, in the 175 pages at the author's disposal, anything like a complete treatment of the numerous and difficult problems presented by petrological science at the present day, but it is certainly possible to conceive of a bolder and more masterly treatment of the whole question than is found in the present work. "Geomechanik," or physical geology, treats of the questions usually grouped by English writers under the head of dynamical geology; and the fifth and concluding portion of the work is devoted to "Geogenie," or a general sketch of historical geology. The work is of interest to English students and teachers of geological science, as illustrating the general methods of treatment of the subject which prevail in Germany. Without aiming at the comprehensive character which belongs to the well-known treatises of Credner and Gümbel, this book forms an admirable sketch of the chief facts and theories of geological science, which are presented always in an attractive and sometimes in a somewhat novel manner.

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.*]

Functionless Organs.

IN an interesting letter which appeared in NATURE (p. 341), under the above title, the Duke of Argyll brings forward a "doctrine of prophetic germs" as explanatory of certain rudimentary structures. He refers particularly to the electrical organ of the skate, which he regards as an example of such a germ. The doctrine is that these functionless organs are not structures which have been useful, but are endowed with "utilities yet to be."

In the lecture which I gave at the Royal Institution, on "Electrical Fishes," in May 1887, I pointed out, in discussing the particular instance referred to, that the difficulty suggested by your distinguished correspondent was familiar to Mr. Darwin, and that it was dealt with by him in the sixth chapter of the "Origin," in what seemed to me to be the only way which was then, or is now, possible. We should learn to understand it, he said, by observing "by what graduated steps" [electrical organs] "have been developed in each separate group of fishes." By this I understand him to have meant that what we require to know is, under what conditions the development of electrical organs has actually taken place.

On morphological grounds, we know that a striped muscular fibre taken together with its nerve, and the electrical disk of the organ of the skate taken together with its nerve, are homologous structures—that is, that they are made up of corresponding parts, and have corresponding places in the normal order of development; so that they are in collateral, not in sequential, relation to each other. In other words, both spring from the same origin, not one from the other; and the development of one is quite as *normal* as of the other. An electrical organ is no more an abnormal muscle, than a muscle a misdeveloped electrical organ.

In accordance with Mr. Darwin's teaching, external conditions, whether antecedent or collateral, influence development only in accordance with morphological laws—that is, with the normal order of development. In the present instance we have some knowledge of the order, but the conditions are unknown; and what we have to do is to ascertain what conditions of existence have given predominance to one order rather than to the other, so as, in certain cases, to determine the development of apparatus for producing electrical discharges in place of apparatus for doing mechanical work.

This is the problem, and it will take a long time to investigate it. We know a great deal more now than Mr. Darwin did twenty-five years ago about the structure, development, and mode of working of the electrical organ, but scarcely more than he did about the "why" of its existence in such animals as the skate. Nor shall we be able to give any better account of it until time and opportunity have been afforded for the examination and comparison of a much larger number of instances than are at present accessible to us.

I need only add a word as to his Grace's suggestion that the electrical organ of the skate may be regarded as a "prophetic germ." I would observe that, although in some species of skate the organ is imperfect, it shows no sign of incompleteness in others, and therefore cannot be properly designated a germ. As to the organ being prophetic, I am not sure that I understand what the word means. If the prophecy is such as might encourage the present race of skates to hope to be provided at some future period with more efficient apparatus, I am afraid that any such expectation on their part would be illusory.

Oxford August 15. J. BURDON-SANDERSON.

ON the part of, I believe, a very large class of unprofessional students of science and theology, I should like to express the profound dissatisfaction, not unmingled with irritation, with which we have read the Duke of Argyll's recent contributions to the subject of evolution. The complete collapse of the grave charges made against the advocates of evolution in the article entitled "A Great Lesson" in the September (1887) number of the *Nineteenth Century*, is too well known to need comment.

The letter on "Functionless Organs" affords another instance of the illogical and dogmatic style with which we are too familiar. Passing over any notice of the absolute inconceivability of any *cause* for the development of "prophetic structures," the Duke of Argyll once more repeats the exploded notion that "the element of fortuity is inseparable from the idea of natural selection," whereas, as has been proved over and over again, the ideas of fortuity and of evolution, of which process natural selection is so integral a part, are absolutely incompatible. But perhaps the climax is reached in the following quotation: "Hitherto I have never yet met with a case in which an expert interprets functionless organs as structures on the way to use." Having at last found a solitary case which, it is thought, by one expert, may be interpreted against the Darwinian conception of evolution, he immediately jumps to the conclusion that "everywhere, in reasoning and observation, it is breaking down."

Apropos of Mr. J. G. Hurst's pertinent queries on p. 364 of your last issue, it may be well to recall the Duke of Argyll's dictum given in the "Reign of Law," *i.e.* that in man's structure "there is no aborted member. Every part is put to its highest use." SAMUEL F. WILSON.

Warsop, August 18.

Lamarckism *versus* Darwinism.

IT is to be regretted that Dr. Romanes has not written anything which can be considered as a reply to my letter. Although Prof. Weismann's essays, to which I referred, are certainly "two of the most notorious essays in the recent literature of Darwinism," it is nevertheless equally certain that a large and important part of their contents is devoted to the consideration of the causes of variation. This being the case, I may safely leave the evidence in support of the statement in my first letter to anyone who will take the trouble to read p. 841 of the June number of the *Contemporary Review*. As it is probable that many people have already read the article in question, and that others may be induced to do so as a result of this correspondence, I think that on this account it may be worth while for Dr. Romanes to notice the criticism, and if possible to show that his remark about Prof. Weismann is intended to bear some other than its obvious meaning.

I need hardly make any further reference to the second and third paragraphs of Dr. Romanes's letter, for I have already explained my position in my first letter. I need only reassert that I was in no way influenced by Dr. Romanes's remarks or opinions about myself; nor am I concerned to allude to the personal references contained in his letter, except to express regret if anything in the form as apart from the substance of my first letter should have caused the annoyance which Dr. Romanes takes no pains to conceal.

In conclusion, it may be worth while to draw attention to the curious coincidence which brings into the same number of NATURE a letter from Prof. E. Ray Lankester, containing an expression of opinion diametrically opposed to that of Dr. Romanes upon the interesting question of Lamarck *versus* Darwin. EDWARD B. POULTON.

Oxford, August 17.

WITH reference to the recent revival of what may be considered as "pure" Lamarckism, it appears to me of importance that those who have followed the course of biological work and thought in this direction should at the present juncture declare their views with respect to the interpretation of such results as those obtained by Mr. Poulton, and referred to by Dr. Romanes in his letter of August 9 (p. 364). I am glad of the present opportunity of discussing this matter, because Mr. Poulton's work is to a large extent an expansion and experimental confirmation of views to which I gave expression in a paper published in 1873 (Proc. Zool. Soc., p. 159). I have no desire to enter into the personal question as to whether Dr. Romanes has or has not made himself acquainted with Weismann's essays, but I must express my disappointment that he has not given us a more explicit statement concerning the precise manner in which he interprets the experiments in the Lamarckian sense. For my own part I may add that I have had opportunities of witnessing Mr. Poulton's experiments at intervals during their progress, and of discussing their bearings with him, and I must confess that I am at present completely at a loss to see how they can by any means be interpreted in the manner Dr. Romanes suggests.

The conclusions at which I arrived in the paper referred to may be very briefly summarized. We find in many species of insects, &c., a variability in colour which is distinctly of an *adaptive* character, enabling the insect to become adapted to a variable environment, and thus being obviously advantageous to the possessors of such a faculty. From this it seemed but a natural conclusion that such a power of adaptability should have been conferred by the usual operation of the law of the survival of the fittest. This conclusion I ventured to draw in 1873, after carefully considering all the cases which I could collect. But in thus grouping what I called at the time "variable protective colouring" among the biological phenomena capable of being regarded as the result of the action of natural selection, I was careful to point out that the precise mechanism of the process by which this adaptability was brought about remained to be investigated for each case. This is the work which has been so admirably carried out by Mr. Poulton for certain Lepidopterous larvæ, pupæ, and cocoons, and the results which he has obtained go far to show that this adaptability in colour is possessed by a much larger number of species than was formerly suspected, and that the modification is invariably in the direction of protection. The experiments prove also that the stimulus prompting the colour change is given by the colour of the surroundings, but the precise means by which the stimulus is conveyed to the pigment-secreting cells has not yet been made out. This part of the work is no doubt the most difficult to deal with from the experimental side, but any objection to the Darwinian explanation which may be urged from the point of view of our ignorance of the nature of this correlation between an external stimulus and the power of secreting a particular colour applies with equal or greater force to the theory of "direct action" upon which so much stress is laid by the new Lamarckian school. The difficulty in the way of completing the explanation of this kind of action is of precisely the same nature as that which meets us when we attempt to explain the power of colour adaptability in a frog or fish as depending upon a colour stimulus, which in these cases is known to be conveyed through the eye. All that is contended for is that the power of adaptation has been conferred by natural selection, an agency capable of dealing with complex physiological relationships in precisely the same way that it deals with all other kinds of variations. In these cases of variable protective colouring we are concerned with the *origin* of the initial variations only in the same manner that we are concerned with their origin in ordinary cases of protective resemblance. Why the colour variability should always be restricted to the limits of protective shades is perfectly intelligible from the purely Darwinian stand-point, but is, as it appears to me, absolutely devoid of meaning if we accept the theory of "direct action."

R. Meldola.

August 18.

MODERN VIEWS OF ELECTRICITY.[1]

Part IV.—Radiation.

IX.

So far as we have been able to understand and explain electrical phenomena, it has been by assuming the existence of a medium endowed with certain mechanical or *quasi*-mechanical properties, such as mobility, incompressibility or infinite elasticity of volume, combined with a certain amount of plasticity or finite elasticity of shape. We also imagined the medium as composed of two opposite constituents, which we called positive and negative electricity respectively, and which were connected in such a way that whatever one did the other tended to do the precise opposite. Further, we were led to endow each of these constituents with a certain amount of inertia, and we recognized something of the nature of friction between each constituent and ordinary matter.

Broadly speaking we may say—

(1) That *friction* makes itself conspicuous in the discussion of current-electricity or the properties of conductors, and that the laws of it are summarized in the statement known by the name of Ohm, viz. that the current through a given conductor is proportional to the force that drives it, or that the opposition force exerted by a conductor upon a current is simply proportional to the strength of that current.

(2) That *elasticity* is recognized as necessary when studying the facts of electrostatics or the properties of insulators—electric displacement and recoil, or charge and discharge: the laws having been studied by Faraday, and the relative pliability (or shearability if there were such a word) of the medium in different substances being measured and stated in terms of that of air as their specific inductive capacity, K.

(3) That *inertia* is brought into prominence by the facts of magnetism, studied chiefly perhaps by Thomson, who has called the relative density of the medium in different substances their magnetic permeability or magnetic inductive capacity; the ratio of its value for any substance to its value for common air being called μ.

(4) That the *doubleness of constitution* of the medium—its being composed of two precisely opposite entities—is suggested by the facts of electrolysis, by the absence of mechanical momentum in currents and magnets, and by the difficulty of otherwise conceiving a medium endowed with rigidity which yet is perfectly fluid to masses of matter moving through it.

With the hypothesis of doubleness of constitution this difficulty disappears. The ether as a whole may be perfectly fluid and allow bodies to pass through it without resistance, while its two components may be elastically attached together and may resist any forces tending to separate them with any required rigidity. It is like the difference between passing one's hand through water, and chemically decomposing it; it is like the difference between waving a piece of canvas about, and tearing it into its constituent threads.

To put the matter boldly and baldly: we are familiar with the conceptions of matter and of ether, and it is known that the two things react on each other in some way, so that although matter appears to move freely through a free portion of the ether, yet another portion appears to move with matter as if bound to it. This mode of regarding the facts is as old as Fresnel. We now proceed a step further, and analyze the ether into two constituents—two equal opposite constituents—each endowed with inertia, and each connected to the other by elastic ties: ties which the presence of gross matter in general weakens and in some cases dissolves. The two constituents are called positive and negative electricity respectively, and of these two electricities we imagine the ether to be composed. The tie between them is dissolved in metals, it is relaxed or made less rigid in ordinary insulators. The specific inductive capacity of a substance means the reciprocal of the rigidity of its doubly constituted ether. Let us call this rigidity k, so that $k = \frac{1}{K}$.

The neighbourhood of gross matter seems also to render ether more *dense*. It is difficult to suppose that it can really condense an incompressible fluid, but it may load it or otherwise modify it so as to produce the effect of increased density. In iron this density reaches its highest known value, and in all substances the density or inertia per unit volume of their ether may be denoted by μ, and called their magnetic permeability.

Let it be understood what we are doing. In Part I. we discussed effects very analogous to those which would be produced by an elastic incompressible medium (roughly like india-rubber or jelly). In Parts II. and III. we discussed effects suggesting, and more or less necessitating, the idea of a property of the medium very analogous to inertia; and we were also led to postulate a doubleness of constitution for the medium, so that shearing strains may go on in it and yet it be perfectly fluid as a whole. We are now pushing these analogies and ideas into greater definiteness and baldness of statement. We already know of a continuous incompressible fluid filling all space, and we call it the ether. Let us suppose that it is composed of, and by electromotive force analyzable into, two constituents; let these constituents cling together with a certain tenacity, so that the medium shall have an electromotive elasticity, though mechanically quite fluid; and let each constituent possess inertia, or something so like inertia as to produce similar effects. Making this hypothesis, electrical effects are to a certain extent explained. Not ultimately indeed—few things can be explained ultimately—not even as ultimately as could be wished; for the nature of the connection between the two constituents of the ether and between the ether and gross matter—the nature of the force, that is, and the nature of the inertia—remains untouched. This is a limitation to be clearly admitted; but if that were the only one—if all else in the hypothesis were true—we should do well, and a distinct step would have been gained. It is hardly to be hoped that this is so—hardly to be expected that the bald statement

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[1] Continued from vol. xxxvii. p. 368.

above is more than a kind of parody of the truth; nevertheless, supposing it only a parody, supposing what we call electromotive elasticity and inertia are things capable of clearer conception and more adequate statement, yet, inasmuch as they correspond to and represent a real analogy, and inasmuch as we find that a medium so constructed would behave in a very electrical manner, and might in conjunction with matter be capable of giving rise to all known electrical phenomena, we are bound to follow out the conception into other regions, and see whether any other abstruse phenomena, not commonly recognized as electrical, will not also fall into the dominion of this hypothetical substance and be equally explained by it. This is what we shall now proceed to do.

Before beginning, however, let me just say what I mean by "electromotive elasticity." It might be called chemical elasticity, or molecular elasticity. There is a well-known distinction between electromotive force and ordinary matter-moving force. The one acts upon electricity, straining or moving or, in general, "displacing" it; the other acts upon matter, displacing it. The nature of neither force can be considered known, but crudely we may say that as electricity is to matter so is electromotive force to common mechanical force: so also is electromotive elasticity to the common shape-elasticity or rigidity of ordinary matter: so perhaps, once more, may electrical inertia be to ordinary inertia.

Inertia is defined as the ratio of force to acceleration; similarly electric inertia is the ratio of electromotive force to the acceleration of electric displacement. It is quite possible that electric inertia and ordinary inertia are the same thing, just as electric energy is the same with mechanical energy. If this were known to be so, it would be a step upward towards a mechanical explanation; but it is by no means necessarily or certainly so; and, whether it be so or not, the analogy undoubtedly holds, and may be fruitfully pursued.

And as to "electromotive elasticity," one may say that pure water or gas is electromotively elastic, though mechanically limpid; each resists electric forces up to a certain limit of tenacity, beyond which it is broken; and it recoils when they are withdrawn. Glass acts in the same way, but that happens to be mechanically elastic too. Its mechanical elasticity and tenacity have, however, nothing to do with its electric elasticity and tenacity.

One perceives in a general way why fluids can be electrically, or chemically, or molecularly elastic: it is because their molecules are doubly or multiply composed, and the constituent atoms cling together, while the several molecules are free of one another. Mechanical forces deal with the molecule as a whole, and to them the substance is fluid; electrical or chemical forces deal with the constituents of the molecule, setting up between them a shearing strain and endeavouring to tear them asunder. To such forces, therefore, the fluid is elastic and tenacious up to a certain limit. Extend this view of things to the constitution of the ether, and one has at least a definite position whence to further proceed.

It may be convenient and not impertinent here to say that a student might find it a help to re-read Parts I. and II. in the light of what has just been said: remembering that, for the sake of simplicity, only the simple fact of an elastic medium was at first contemplated and insisted on; no attempt being made to devise a mechanism for its elasticity by considering it as composed of two constituents. Hence the manifest artificiality of such figures as Fig. 6 (NATURE, vol. xxxvi. p. 559), where fixed beams are introduced to serve as the support of the elastic connections. But it is pretty obvious now, and it has been said in Part III., that a closer analogy will be obtained by considering two sets of beads arranged in alternate parallel rows connected by elastic threads, and displaced simultaneously in opposite directions.

Recovery of the Medium from Strain.

We have now to consider the behaviour of a medium endowed with an elastic rigidity, k, and a density, μ, subject to displacements or strains. One obvious fact is that when the distorting force is removed the medium will spring back to its old position, overshoot it on the other side, spring back again, and thus continue oscillating till the original energy is rubbed away by viscosity or internal friction. If the viscosity is very considerable, it will not be able so to oscillate; it will then merely slide back in a dead-beat manner towards its unstrained state, taking a theoretically infinite time to get completely back, but practically restoring itself to something very near its original state in what may be quite a short time. The recovery may in fact be either a brisk recoil or a leak of any degree of slowness, according to the amount of viscosity as compared with the inertia and elasticity.

The matter is one of simple mechanics. It is a case of simple harmonic motion modified by a friction proportional to the speed. The electrical case is simpler than any mechanical one, for two reasons: first, because so long as capacity is constant (and no variation has yet been discovered) Hooke's law will be accurately obeyed—restoring force will be accurately proportional to displacement; secondly, because for all conductors which obey Ohm's law (and no true conductor is known to disobey it) the friction force is accurately proportional to the first power of velocity.

There are two, or perhaps one may say three, main cases. First, where the friction is great. In that case the recovery is of the nature of a slow leak, according to a decreasing geometrical progression or a logarithmic curve; the logarithmic decrement being independent of the inertia, and being equal to the quotient of the elasticity and the resistance coefficients.

As the resistance is made less, the recovery becomes quicker and quicker until inertia begins to prominently assert its effect and to once more lengthen out the time of final recovery by carrying the recoiling matter beyond its natural position, and so prolonging the disturbance by oscillations. The quickest recovery possible is obtained just before these oscillations begin; and it can be shown that this is when the resistance coefficient is equal to twice the geometric mean of the elasticity and the inertia. One may consider this to be the second main case.

The third principal case is when the resistance is quite small, and when the recovery is therefore distinctly oscillatory. If the viscosity were really zero, the motion would be simply harmonic for ever, unless some other mode of dissipating energy were provided; but if some such mode were provided, or if the viscosity had a finite value, then the vibrations would be simply harmonic with a dying out amplitude, the extremities of all the swings lying on a logarithmic curve. In such a case as this, the rate of swing is practically independent of friction; it depends only on elasticity and inertia; and, as is well known for simple harmonic motion, the time of a complete swing is 2π times the square root of the ratio of inertia and elasticity coefficients.

Making the statement more electrically concrete, we may consider a circuit with a certain amount of stored-up potential energy or electrical strain in it: for instance, a charged Leyden jar provided with a nearly complete discharge circuit. The main elastic coefficient here is the reciprocal of the capacity of the jar: the more capacious the jar the more "pliable" it is—the less force of recoil for a given displacement,—so that capacity is the inverse of rigidity. The main inertia coefficient is that which is known electrically as the "self-induction" of the circuit: it involves the inertia of all the displaced matter and ether, of everything which will be moved or disturbed when the jar is discharged. It is not a very simple thing to calculate its value in any given case; still it can be done, and the general idea is plain enough without understanding the exact function and importance of every portion of the surrounding space.

Corresponding, then, to the well-known simple harmonic $T = 2\pi\sqrt{\frac{m}{k}}$, we have, writing L for the self-induction or inertia of the circuit, and S for its capacity or inverse rigidity constant,

$$T = 2\pi\sqrt{LS},$$

This, therefore, is the time of a complete swing. Directly the jar is discharged, these oscillations begin, and they continue like the vibration of a tuning-fork until they are damped out of existence by viscosity and other modes of dissipation of energy.

But now just consider a tuning-fork. Suppose its substance were absolutely unviscous, would it go on vibrating for ever? In a vacuum it might: in air it certainly would not. And why not? Because it is surrounded by a medium capable of taking up vibrations and of propagating them outwards without limit. The existence of a vibrating body in a suitable medium means the carving of that medium into a succession of waves and the transmission of these waves away into space or into absorbing obstacles. It means, therefore, the conveyance away of the energy of the vibrating body, and its subsequent appearance in some other form wherever the radiating waves are quenched.

The laws of this kind of wave-propagation are well known; the rate at which waves travel through the medium depends not at all on any properties of the original vibrating body, the source of the disturbance; it depends solely on the properties of the medium. They travel at a rate precisely equal to the square root of the ratio of its elasticity to its density.

Although the speed of travel is thus fixed independently of the source, the length of the individual waves is not so independent. The length of the waves depends both on the rate at which they travel and on the rate at which the source vibrates. It is well known and immediately obvious that the length of each wave is simply equal to the product of the speed of travel into the time of one vibration.

But not every medium is able to convey every kind of vibration. It may be that the mode of vibration of a body is entirely other than that which the medium surrounding it can convey: in that case no dissipation of energy by wave-propagation can result, no radiation will be excited. The only kind of radiation which common fluids are mechanically able to transmit is well known: it is that which appeals to our ears as sound. The elasticity concerned in such disturbance as this is mere volume elasticity or incompressibility. But electrical experiments (the Cavendish experiment,[1] and Faraday's ice-pail experiment) prove the ether to be enormously —perhaps absolutely—incompressible; and if so, such vibrations as these would travel with infinite speed and not carve proper waves at all.

Conceivably (I should like to say probably) *gravitation* is transmitted by such longitudinal impulses or thrusts, and in that case it is nearly or quite instantaneous; and the rate at which it travels, if finite, can be determined by a still more accurate repetition of the Cavendish experiment than has yet been made; but true radiation transmitted by the ether cannot be of this longitudinal character. The elasticity possessed by the ether is of the nature of rigidity: it has to do with shears and distortions; not mechanical stresses, indeed—to them it is quite limpid and resistless—but electromotive stresses: it has an electrical rigidity, and it is this which must be used in the transmission of wave-motion.

But the oscillatory discharge of a Leyden jar is precisely competent to apply to the ether these electromotive vibrations: it will shake it in the mode suitable for it to transmit; and accordingly, from a discharging circuit, waves of electrical distortion, or transverse waves, will spread in all directions at a pace depending on the properties of the medium.

Thus, then, even with a circuit of perfect conductivity the continuance of the discharge would be limited, the energy would be dissipated; not by friction, indeed—there would in such a circuit be no direct production of heat—it would be dissipated by radiation, dissipated in the same way as a hot body cooling, in the same way as a vibrating tuning-fork mounted on its resonant box. The energy of the vibrating body would be transferred gradually to the medium, and would by this be conveyed out and away, its final destination being a separate question, and depending on the nature and position of the material obstacles it meets with.

Velocity of Electrical Radiation.

The pace at which these radiation-waves travel depends, as we have said, solely on the properties of the medium, solely on the relation between its elasticity and its density. The elasticity considered must be of the kind concerned in the vibrations; but the vibrations are in this case electrical, and so electrical elasticity is the pertinent kind. This kind of elasticity is the only one the ether possesses of finite value, and its value can be measured by electrostatic experiments. Not absolutely, unfortunately: only the relative elasticity of the ether as modified by the proximity of gross substances has yet been measured: its reciprocal being called their specific inductive capacity, or dielectric constant, K. The absolute value of the quantity K is at present unknown, and so a convention has arisen whereby in air it is called 1. This convention is the basis of the artificial electrostatic system of units. No one supposes, or at least no one has a right to suppose, that its value is really 1. The only rational guess at its value is one by Sir William Thomson,[1] viz. $\frac{1}{842\ 8}$.

Whether known or not, the absolute value of the dielectric constant is manifestly a legitimate problem which may any year be solved.

The other thing on which the speed of radiation waves depends is the medium's density—its electric density, if so it must be distinguished. Here, again, we do not know its absolute value. Its relative or apparent amount inside different substances is measured by magnetic experiments, and called their specific magnetic capacity, or permeability, and is denoted by μ.

Being unknown, another convention has arisen, quite incompatible with the other convention just mentioned, that its value in air shall be called 1. This convention is the basis of the artificial electro-magnetic system of units —volts, ohms, amperes, farads, and the like. Both of these conventions cannot be true: no one has the least right to suppose either true. The only rational guess at ethereal free density is one by Sir William Thomson, viz. $9{\cdot}36 \times 10^{-19}$.

Very well, then; it being clearly understood that these two great ethereal constants, k or $\frac{1}{K}$, and μ, are neither of them at present known, but are both of them quite knowable, and may at any time become known, it remains to express the speed of wave transmission in terms of them. But it is well known that this speed is simply the square root of the ratio of elasticity to density, or

$$v = \sqrt{\frac{k}{\mu}}, \text{ or } \frac{1}{\sqrt{(K\mu)}}.$$

This then is the speed with which waves leave the discharging Leyden jar circuit, or any other circuit conveying alternating or varying currents, and travel out into space.

Not knowing either k or μ, we cannot calculate this

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[1] See Maxwell's "Electrical Researches of Cavendish," p. 104; see also p. 417.

[1] Trans. R. S. Edin., xxi. 60; see also article "Ether," in the "Encyc. Brit."

speed directly, but we can try to observe it experimentally.

The first and crudest way of making the attempt would be to arrange a secondary circuit near our oscillating primary circuit, and see how soon the disturbance reached it. For instance, we might take a nearly closed loop, make it face a Leyden jar circuit across a measured distance, and then look for any interval of time between the spark of the primary discharge and the induced spark of the secondary circuit, using a revolving mirror or what we please. But in this way we should hardly be able to detect any time at all : the propagation is too quick.

We might next make use of the principle of the electric telegraph, viz. the propagation of a disturbance round a single circuit from any one point of origin. Consider a large closed circuit, either conveying or not conveying a current : introduce at any one point a sudden change—a sudden E.M.F., for instance, or a sudden resistance if there be a current already. Out from that point a disturbance will spread into the ether, just as happens in air when a blow is struck or gun-cotton fired. A regular succession of disturbances would carve the ether into waves : a single disturbance will merely cause a pulse or shock ; but the rate of transmission is the same in either case, and we may watch for the reception of the pulse at a distant station. If the station has to be very distant in order to give an appreciable lapse of time, a speaking-tube is desirable to prevent spreading out in all directions—to concentrate the disturbance at the desired spot. What a speaking-tube is to sound, that is the wire of the circuit—the telegraph wire—to ethereal pulses.

It is a curious function, this of the telegraph wire: it does not *convey* the pulses, it directs them. They are conveyed wholly by the ether, at a pace determined by the properties of the ether, modified as it may be by the neighbourhood of gross matter. Any disturbance which enters the wires is rapidly dissipated into heat, and gets no further ; it is the insulating medium round it which transmits the pulses to the distant station.

All this was mentioned in Part III., and an attempt was made to explain the mechanism of the process, and to illustrate in an analogical way what is going on.

The point of the matter is that currents are not propelled by end-thrusts, like water in a pipe or air in a speaking-tube, but by lateral propulsion, as by a series of rotating wheels with their axes all at right angles to the wire surrounding it as a central core, and slipping with more or less friction at its surface. This is characteristic of ether modes in general : it does not convey longitudinal waves or end-thrust pulses, like sound, but it conveys transverse vibrations or lateral pulses, like light.

Without recapitulating further, we can perceive, then, that the transmission of the pulse round the circuit to its most distant parts depends mainly on the medium surrounding it. The process is somewhat as follows :—Consider two long straight parallel wires, freely suspended, and at some great distance joined together. At the near end of each, start equal opposite electromotive impulses, as by suddenly applying to them the poles of a battery ; or apply a succession of such pulses by means of an alternating machine. Out spread the pulses into space, starting in opposite phases from the two wires, so that at a distance from the wires the opposite pulses interfere with each other, and are practically non-existent, just as but little sound is audible at a distance from the two prongs of a freely suspended tuning-fork. But near the wires, and especially between them, the disturbance may be considerable. To each wire it spreads and is dissipated, and so a fresh supply of energy goes on continually arriving at the wires, always flowing in from outside, to make up the deficiency. If the wires are long enough hardly any energy may remain by the time their distant ends are reached ; but whatever there is will still be crowding in upon the wires and getting dissipated, unless by some mechanism it be diverted and utilized to effect some visible or audible or chemical change, and so to give the desired signal.

Now the pace at which this transmission of energy goes on in the direction of the wires is pretty much the same as in free space. There are various circumstances which can retard it ; there are none which can accelerate it. The circumstances which can retard it are, first, constriction of the medium by too great proximity of the two conducting wires : as, for instance, if they consisted of two flat ribbons close together with a mere film of dielectric between, or if one were a small-bore tube and the other its central axis or core. In such cases as this the general body of ether takes no part in the process, the energy has all to be transmitted by the constricted portion of dielectric, and the free propagation of ethereal pulses is interfered with : the propagation is no longer a true wave-propagation at all, but approximates more or less closely to a mere diffusion creep, rapid it may be, and yet without definite velocity, like the conduction of heat or the diffusion of a salt into water. One well-known effect of this is to merge successive disturbances into one another, so that their individuality, and consequently the distinctness of signalling, is lost.

Another circumstance which can modify rate of transmission of the pulses is ethereal inertia in the substance of the conducting wires, espečally extra great inertia, as, for instance, if they are made of iron. For the dissipation of energy does not go on accurately at their outer surface ; it has usually to penetrate to a certain depth, and until it is dissipated the fresh influx of energy from behind does not fully occur. Now, so long as the value of μ for the substance of the wires is the same as that of air or free space, no important retardation is thus caused, unless the wires are very thick ; but directly the inertia in the substance of the wires is one or two hundred times as big as that outside, it stands to reason that more time is required to get up the needful magnetic spin in its outer layers, and so the propagation of pulses is more or less retarded. At the same time this circumstance does not alter the character of the propagation, it does not change it from true wave velocity to a diffusion, it leaves its character unaltered ; and so the signals, though longer in coming, may arrive quite clear, independent, and distinct. It is much the same, indeed, as if the density of the surrounding medium had been slightly increased.

These, then, are the main circumstances which affect the rate of transmission of a pulse from one part of a closed circuit to another : extra inertia or so-called magnetic susceptibility in the conducting substance, especially in its outer layers ; and undue constriction or throttling of the medium through which the disturbance really has to go. Both these circumstances diminish rate of transmission, and one (the last mentioned) modifies the law and tends to obliterate individual features and to destroy distinctness.

Of course, besides these, the nature of the insulating medium will have an effect on the rate of propagation, but that is obvious all along ; it is precisely the rate at which any given medium transmits pulses that we want to know, and on which we are thinking of making experiments. If we use gutta-percha (more accurately the ether inside gutta-percha) as our transmitting medium in an experiment, we are not to go and pretend that we have obtained a result for air.

The circumstances we have considered as modifying the rate of transmission are both of them adventitious circumstances, independent of the nature of the medium, and they are entirely at our own disposal. If we like to throttle our medium, or to use thick iron wires, we can do so, but there is no compulsion : and if we wish to make the experiment in the simplest manner, we shall do no such thing. We shall use thin copper wires (the thinner the better), arranged parallel to one another a fair distance apart, and we shall then observe the time which an electromotive impulse communicated at one end takes to travel to the other. Instead of using two wires, we may if we like use what comes to much the same thing, viz. a single wire suspended at a reasonable height above the ground, as in a common land telegraph. Such a case as this is much the same as if two wires were used at a distance apart equal to about twice the height above the ground.

The experiment, if it could be accurately made, would result in the observation of a speed of propagation equal to 3×10^{10} centimetres per second. The actual speed in practice may be less than this, by reason of the various circumstances mentioned, but it can never be greater. This, then, is the rate of transmission of transverse impulses, and therefore of transverse waves, through ether as free as it can be easily obtained.

There are many methods known to physicists by which an indirect experimental determination of this velocity can be made. These methods are more easily practicable than the one described: they directly determine the ratio k/μ, or, what is the same thing, the product $K\mu$, and it is left to theory to say that this is really the velocity of electrical pulses in free ether. It is unnecessary to say more about them here. OLIVER J. LODGE.

(*To be continued.*)

A HISTORY OF THE AUGUST METEORS.

THE August meteor-shower has been more frequently observed than any other with which we are acquainted, and the modern history of this remarkable system includes many interesting circumstances. It has not, in recent times, given us displays equal in grandeur to periodical swarms like the Leonids of November 13 and Andromedes of November 27, being decidedly less rich in point of numbers. But what this stream lacks in this respect is compensated for by the *annual* visibility of the shower and by the intense brilliancy of some of its individual members. Every year the August meteors present a conspicuous appearance on the night following St. Lawrence's Day, and fire-balls of excessive lustre are now and then interspersed with the smallest perceptible shooting-stars of the system. The Leonids and Andromedes, which have rendered the month of November so famous in meteoric annals, can only reappear abundantly at intervals of thirty-three and (probably) thirteen years, whereas the Perseids of August are unfailing in their regular apparitions as the epoch comes round each year. On the night of the 10th the most casual observer will not fail to notice the surprising frequency of shooting-stars, and must remark their occasional brilliancy and the persistency of the phosphorescent after-glows which they generate during their rapid flights amongst the fixed stars.

The early history of the August meteors is vague and meagre in the extreme. Ancient writings are significantly mute as to the scientific aspect of meteor-showers. Doubtless in olden times these phenomena were equally as plentiful as at present, but amid the ignorance and superstition which prevailed they were little regarded. The prominent part which meteors play in the solar system was not suspected, hence no importance was attached to their appearance. They were supposed to be mere exhalations uncontrolled by fixed laws, and it is entirely due to modern science that their true character has been revealed, and that they have been raised to the dignity of bodies having a celestial origin, and probably also an extensive influence throughout the wide range of astronomical physics.

But former records, if void of particulars possessing a scientific utility, are yet often useful in supplying *dates*. Many old references to meteor-showers, though very imperfect in description, are, by the accordance of epoch, justly assumed to have been early exhibitions of the very same systems as those which have furnished some of the most imposing displays of recent years. In the catalogue of 315 meteoric showers compiled by Quetelet, a considerable proportion are probably identical with the August Perseids, and below we give the dates, up to a century ago, of these:—

Year.	Date.	Year.	Date.
811	July 25	926	July 27-30
820	,, 25-30	933	,, 25-30
824	,, 26-28	1243	Aug. 2
830	,, 26	1451	,, 7
833	,, 27	1709	,, 8
835	,, 26	1779	,, 9 and 10
841	,, 25-30	1781	,, 8
924	,, 27-30	1784	,, 6
925	,, 27-30	1789	,, 10

The dates in the ninth and tenth centuries are somewhat different from those in later years, but this does not negative the assumed relation, because they are brought nearly into agreement when the change of style in 1752 is allowed for. This proves the showers to have really occurred at a period early in August according to present reckoning. There may also be a slight alteration in the epoch of the swarm due to a shifting of the node, which, in its cumulative amount after many ages, might reach a considerable value. For the reasons assigned, the celebrated shower of Leonids which now takes place on November 13 was observed in October 902, and again on October 19, 1202, October 22, 1366, &c.

Muschenbroek, in 1762, announced the general fact that he had observed shooting-stars to be more plentiful in August than in any other month of the year. Further towards the close of the century this was in part confirmed by the apparition of many meteors on August 8 and 9. In 1806 and 1812, Dr. Forster, of Clapton, recorded in his "Calendar" that these phenomena were unusually abundant on August 10, and in the latter year he particularly noted the extraordinary length and phosphorescent aspect of the trains left in their wake. Subsequently the same epoch was amply corroborated; and in 1835, Quetelet definitely mentioned the 9th and 10th of August as the date of maximum annual display.

On August 9, 1837, M. Wartman, of Geneva, observed 82 of these meteors between 9 p.m. and midnight. In the following year, on August 10, observations were made at Geneva and at Planchettes, a village 62 miles north-east of Geneva, with the view of determining the heights and velocities of the meteors. A discussion of the results showed that the average elevation above the ground was 550 miles, and the velocity 220 miles, but these figures are now known to have been enormously in excess of the true values.

From 20 meteors observed in August 1863, Prof. A. S. Herschel determined the mean height as 81·6 miles at first appearance and 57·7 miles at disappearance, and the velocity was found to be 34·4 miles per second. From 27 meteors similarly observed in Italy between August 5 and 10, 1864, Secchi derived limiting heights of 76·6 and 49·7 miles; and, averaging these with the results obtained by Prof. Herschel in the preceding year, we get 78 to 54 miles, which may be adopted as representative values for the normal heights not only of the Perseids, but of shooting-stars generally.

Heis, Schmidt, Greg, and Herschel were amongst the first to methodically observe the August meteor-shower and determine its radiant point in the northern region of Perseus. In 1863, August 10, an unusual display was witnessed, for on this occasion the stream seems to have attained a degree of intensity not recorded either before or subsequently to that year. In 1871 there was also a very pronounced and abundant appearance of these meteors. In NATURE, vol. xx. p. 457 (September 11, 1879), will be found some details as to the relative number of August meteors counted in different years.

But the epoch of 1866 is perhaps the most eventful and interesting of all in the history of this notable group. Signor Schiaparelli, of Milan, in the course of some observations of the Perseids, was led to take up the investigation of the theory of shooting-stars. Cautiously sifting the available materials, and forming deductions from facts indicated by the best authorities on the subject, he was induced to the belief that meteors were small particles composing cosmical clouds. These clouds were, by the action of gravitation, spread out into streams, and their orbits formed, like those of comets, elongated conic sections. From a method explained by Prof. Erman, he computed the orbital elements of the August meteors and of certain other streams, and, comparing them with the orbits of comets, discovered two remarkable coincidences between the system of Perseids and Comet III. 1862, and the Leonids and Comet I. 1866. In each case the paths of the meteor group and comet were identical, and every circumstance favoured the inference that the two phenomena were physically identical, the meteors forming the dispersed material of the comet. The period of the Leonids (November 13), viz. 33¼ years, agreed precisely with that of their supposed parent comet. The period of the August display, however, remained doubtful, the ellipse being more elongated; but Schiaparelli adopted a cycle of rather more than 100 years, as best satisfying the observations, though the exact period is still doubtful.

Computation showed that the radiant point of meteoric particles following the track of Comet III. 1862 would be seen, on August 10, at R.A. 43°, Decl. 57½° N. In 1863, on August 10, Prof. Herschel had observed the meteors, and fixed their radiant at R.A. 44°, Decl. 56° N., a wonderfully close agreement, considering the difficulties attached to such observations. This, and other coincidences of orbit, removed all doubts as to the affinity of meteors and comets; and later evidence, especially that afforded by Biela's comet and the splendid meteor-showers of November 27, 1872 and 1885, has afforded convincing proofs as to the validity of the theory enunciated by the Italian astronomer.

Some interesting features in connection with the August meteors still, however, awaited further investigation. The visible duration of the shower was unknown. The radiant was thought to be diffused over a region extending from Perseus to Cassiopœia. Mr. R. P. Greg, in his "Table of Radiants" (*Monthly Notices*, 1872, p. 353), places it over the area from R.A. 50°–25°, Decl. 44° N., to R.A. 50°–65°, Decl. 56° N.; and Serpieri gave R.A. 50°–30°, Decl. 49°–64°. Mr. J. E. Clark, in 1874, undertook the projection of the tracks of about 2000 Perseids described in the "Luminous Meteor Reports" of the British Association, with the object of detecting motion in the radiant centre on successive days or hours of the night, but without definite success, though the observations suggested a progressive motion on succeeding nights similar to that noticed by Prof. Twining in 1859. In 1877 the shower was watched by the writer at Bristol on several nights, and the radiant was distinctly seen to take up a fresh position with every change of date. It moved from R.A. 40°, Decl. 56°, on August 5, to R.A. 60°, Decl. 59° N., on August 16. The fact was first announced in NATURE for August 30, 1877 (vol. xvi. p. 362), and many observations in subsequent years at the same station have fully confirmed the shifting of the radiant, and indicated the long duration of the shower. In the following table will be seen the position of the radiant at intervals of five days:—

	° °		° °
July 8...	3 + 49	August 2... ...	36 + 55
13...	11 + 50	7... ...	42 + 57
19...	19 + 51	12... ...	50 + 57
23...	25 + 52	17... ...	60 + 58
28...	31 + 54		

The whole duration extends, very probably, over the forty-five days from July 8 to August 22, and in the interval the radiant moves from 3° + 49° to 77° + 57°.

This cluster is evidently one of enormous width, and has doubtless undergone distortion by the effect of planetary perturbation. Some interesting facts in connection with this and other cometary meteor systems will be found in the *Sidereal Messenger* for April and May 1886. With regard to the August meteor-shower, it appears that a certain change in the position of the radiant ought theoretically to occur every night, but the observed displacement does not well accord with computation. On July 26 the Perseid radiant is about 4°, and on August 19 about 9°, from the radiant of its derivative comet (III. 1862); and these differences are doubtless to be referred to the disturbances exercised upon the original stream by the attraction of the earth. At every return of the group a vast number of the particles must obviously pass very near to us without being dissipated by the action of our atmosphere, and the paths of these will be affected to an extent that must alter the elements of their orbits.

Though the period of the August meteors has not yet been precisely ascertained, there is no question that the shower exhibits fluctuations from year to year as regards intensity, and that, like the two great systems of November, a certain cycle regulates its most brilliant displays. Future observations will determine the precise form of the orbit. The return of Comet III. 1862, or a recurrence of the very rich shower of August 1863, will decide the matter, but as the orbit is one of considerable eccentricity, several generations may yet elapse before the period is accurately ascertained. It is certain that many of the supposed variations in the perennial intensity of the display are more apparent than real, because the successive returns are witnessed under different conditions. Cloudy or misty weather sometimes interrupts observation; moonlight offers another impediment; occasionally, also, the maximum is attained in daylight, and passes unheeded. The same observer is not always enabled to maintain an outlook from positions equally favourable; and there are other circumstances which, with those mentioned, prove the difficulty of securing a series of observations fairly comparable with each other. Usually about 40 or 50 meteors per hour may be counted by one observer before midnight on August 10, but in the early morning hours of August 11 as many as 80 or 90, perhaps more, will be seen, as the radiant is then higher and better placed for the visible distribution of its meteors.

"The August meteors," though a general term capable of being applied to any showers observed in the month of August, is commonly employed in special reference to the Perseids of August 10. There are large numbers of minor displays visible in the same month, the radiant points of which are scattered profusely over the firmament. There are certainly more than 100 showers in contemporaneous action with the Perseids, and many of these are now pretty well known, a mass of observations having accumulated for this particular epoch.

In the present year the great August shower has not been especially brilliant, though many of its meteors have appeared under their customary aspect. At Bristol, on August 2, 42 shooting-stars were counted during the 2½ hours between 10h. 50m. and 13h. 21m., and 14 of these were Perseids from a centre at 35° + 54°. On August 5, 31 meteors were seen in a similar interval, including 11 Perseids. On August 8, in 3 hours from 10h. to 13h., 36 meteors were observed, and among these were 20 Perseids. The radiant, both on the 2nd and 5th, seemed to be at 42° + 57°. The few subsequent nights were overcast, but on the 13th a clear sky permitted watching, and during the 3½ hours from 10h. to 13h. 30m. 49 meteors were seen, of which 13 were Perseids from a radiant at 52° + 57°. On August 14, between 10h. and 13h., 25 meteors were noted, but there were only two Perseids amongst them.

On August 8, Mr. Booth, at Leeds, watched the eastern sky for 4½ hours, and saw 45 meteors, including 25 Perseids. The radiant was at about 42° + 57½°, and it will be observed that the proportion of Perseids to the total number of meteors observed was the same as noted at Bristol on that date. On August 13, Mr. Booth recorded 13 Perseids from a radiant at 51½° + 56°, thus confirming the displacement observed at Bristol.

On August 10, Mr. G. T. Davis, of Theale, near Reading, reports the sky was clear and many meteors were visible between 9.30 and 11 p.m., the majority being Perseids. The same observer recorded a number of paths on August 5 and 8, and a comparison of his results with similar observations at Bristol show that 7 meteors were doubly observed at the two stations. Their heights, &c., were computed by the writer as follows :—

Date. 1888. Aug.	Hour. G.M.T. h. m.	Mag.	Height at appearance Miles.	Height at disappearance Miles.	Length of real path. Miles.	Radiant point. ° °	Inclination to horizon. °
5	10 19	1-3	69	50	37	50+55	27½
5	10 30	3-4	69	48	38	39+57	34
5	10 42	3-4	68	48	24	43+51	29
8	10 6	3-5	70	59	28	66+56	23
8	10 10	3-4	65	52	38	319−13	20
8	10 21	3-3	43	28	26	40+60	35½
8	10 28	4-4	68	48	24	42+57	33½

The close agreement in the heights of these meteors (except in the case of No. 6 in the list, which was much nearer the earth's surface than usual) will be noticed. They were, with the exception of No. 6, which belonged to a radiant in Aquarius, all members of the August meteor system, though in several cases, notably that of No. 4, the path, as observed at Reading, was not exactly conformable to the radiant point of this shower.

The recent display has furnished us with a splendid fire-ball. It appeared on August 13 at 11h. 33m., and was seen by Mr. Booth at Leeds, by Mr. Monck at Dublin, by the writer at Bristol, and by several observers at Birmingham and other places. When near its disappearance the fire-ball acquired such brilliancy that it lit up the firmament like a vivid flash of lightning, and in the latter portion of its path there remained a comet-like streak which at Leeds and Birmingham continued visible for three minutes. The descriptions of this exceptionally fine meteor are in good agreement. It traversed a course above Yorkshire at normal heights; its brilliant streak had a mean elevation of 53 miles and length of 18 miles. No detonation appears to have been heard.

W. F. DENNING.

NOTES.

IT is proposed by the Organizing Committee of Section B that in the course of the approaching meeting of the British Association there shall be a discussion in that Section upon the subject of "Valency." Prof. Armstrong will open the debate, and it is hoped that several other eminent chemists will take part. In the immediate neighbourhood of Bath there are no industries specially interesting to chemists, but arrangements are in progress by which it is hoped that members will be admitted to some of the works in and about Bristol, which is only ten miles away.

THE autumnal meeting of the Iron and Steel Institute was opened in the University, Edinburgh, on Tuesday. A hearty reception was given to the members in the Senate Hall by the Lord Provost (Sir Thomas Clark), Sir William Muir (Principal of the University), Prof. Armstrong (the honorary secretary of the Reception Committee), and other dignitaries and officials of the University. The members having adjourned to the Examination Hall of the University to begin the business of the meeting, the President, Mr. Daniel Adamson, announced that Sir James Kitson had been nominated by the Council as the President for the next two years, and he hoped that that would meet with the approval of the members. The Institute had intended, he said, to go to America for their next autumnal gathering, but the visit had been postponed until 1890, as that was considered a more suitable time, especially as a kind invitation had been given them to visit Paris next year, when the Exhibition was on. They would thus have an opportunity of entertaining their American friends. Sir Lowthian Bell took the chair while a paper on a lever-testing machine, prepared by the President, was discussed. It described in detail a horizontal compound lever-testing machine. Mr. Wickslead (Leeds), Mr. G. C. Hemming (Yale and Towne Manufacturing Company, U.S.), Mr. Brown (of Brown Brothers, Leith), M. Gautier (Paris), Mr. Nursey (London), and Sir Lowthian Bell took part in the discussion. A paper on manganese steel, by Mr. R. C. Hadfield (Sheffield), proved specially interesting, as it formed a guide to the exhibits of this metal at the Glasgow Exhibition.

THE third International Congress of Inland Navigation was opened at Frankfort-on-the-Main on Monday. It began with a speech from the President, Herr von Bötticher, Minister of State, who greeted those present in the name of the German Emperor. The Congress is divided into three sections. The first studies the improvement of river navigability, the best kind of boat for river navigation, and the best means of propulsion for boats. The second section occupies itself with the economic advantages of ship canals penetrating into the interior from river mouths, their navigability, and keeping in good order. The third deals with the reform of the statistics of interior navigation, and with the relations between agriculture and navigation.

ON Monday a paper by Dr. Gamaleia, of Odessa, on the cure of cholera by inoculation, was read to the Paris Academy of Sciences by M. Pasteur. The following information on the subject is given by the Paris Correspondent of the *Times*. It appears that in 1886 Dr. Gamaleia came to Paris as delegate of the Odessa doctors, and studied the Pasteur method, with which he made himself thoroughly acquainted. On his return to Russia various institutions were founded under his care for the cure of hydrophobia, which have proved very valuable. Five years ago M. Pasteur endeavoured to discover a means of curing cholera by inoculation. At his request a mission was sent by the French Government to Alexandria while cholera prevailed there, to study the subject. Dr. Lhuiller, one of the mission, died of cholera, and M. Pasteur did not press the continuance of the investigations. The subject, however, was taken up by Dr. Gamaleia, who has discovered a method similar to that of M. Pasteur, by which it is believed cholera can be cured by the inoculation of the cholera virus. As yet experiments have only been made on animals, but no doubt is entertained that it will be possible to apply in a short time the same process to man. After reading the paper, M. Pasteur stated that Dr. Gamaleia had expressed his readiness to repeat the experiments at Paris in presence of a committee of the Academy of Sciences, and to try on himself the inoffensive and sufficient dose for human vaccination. He is ready to undertake a journey into countries where cholera prevails to prove the efficacy of his method. M. Pasteur added that he need scarcely say that he accepted with the greatest satisfaction the offer made by Dr. Gamaleia to conduct the experiments in his laboratory. The letter was referred to the committee, which has a prize of 100,000 francs in its hands for a cure for cholera, and it was arranged that the experiments should be postponed till November.

WE have already called attention (p. 359) to the address delivered by M. Janssen on July 23, at the French Academy of Sciences, on the late Jules Henri Debray. M. Debray was born at Amiens in 1827, and entered the Normal School in 1847. There he became the collaborator of the illustrious Sainte-Claire Deville, with whom his name will always be intimately associated. As M. Janssen said, it is by his researches on dissociation, in which he developed M. Deville's ideas, that M. Debray will be chiefly remembered. He succeeded M. Deville at the Paris Faculty of Sciences, and at the Normal School. M. Debray was also assayer to the "Garantie" of Paris, Vice-President of the Society for the Encouragement of National Industry, and a member of the Higher Council of Public Instruction and of the Consulting Committee of Arts and Manufactures. He was considered one of the most active and distinguished members of the Academy of Sciences. After a short illness he died on July 19.

WE regret to record the death of Mr. William H. Baily, Acting Palæontologist of the Geological Survey of Ireland. He was born at Bristol in 1819. In 1844, having held for some years an appointment in the Bristol Museum, Mr. Baily was attached by the late Sir Henry de la Beche to the Geological Survey of England. He acted first as a draughtsman, and afterwards as assistant naturalist under Edward Forbes and subsequently under Prof. Huxley. In 1857, Mr. Baily was transferred to the Irish branch of the Geological Survey as Palæontologist, and this office he held until his death. He was also Demonstrator in Palæontology to the Royal College of Science, Dublin. Mr. Baily often contributed to the Proceedings of the Royal Irish Academy, of the Linnean and Geological Societies of London, of the Royal Geological Society of Dublin, and of various kindred Societies in Europe and the United States. His most important work was his "Characteristic British Fossils," which was incomplete at the time of his death.

MR. SETH GREEN, whose death from paralysis of the brain is announced from New York, made a great reputation in connection with fish culture in the United States. He died at the age of seventy-one. Mr. Green was appointed in 1868 one of the Fish Commissioners of New York, and soon afterwards was made Superintendent of Fisheries in that State. He was decorated with two gold medals by the Société d'Acclimatation of Paris. Mr. Green was the author of "Trout Culture," 1870, and "Fish Hatching and Fish Catching," 1879.

NEUTRAL chloride of platinum has been obtained in fine permanent crystals of the composition $PtCl_4 . 4H_2O$ by M. Engel (*Bulletin de la Soc. Chim.*). The universally-employed chloride of platinum is, as is well known, in reality a chloroplatinate, $PtCl_4 . 2HCl . 6H_2O$; and the neutral chloride cannot be obtained from it by merely raising its temperature, which causes it to part with a portion of its chlorine in addition to the hydrochloric acid, leaving the lower chloride, $PtCl_2$. Some time ago, however, a neutral salt was prepared by Norton, who assigned to it the formula $PtCl_4 . 5H_2O$. Norton's method of preparation consisted in the addition of silver nitrate to the ordinary commercial chloride of platinum in the proportion of two molecules of the former to one of the latter. The composition of the precipitate appears never to have been thoroughly cleared up, but the filtered liquid was found to deposit crystals of the neutral chloride. As the whole subject appeared involved in a certain amount of doubt, Engel has repeated Norton's work, and finds that the neutral chloride *is* obtained under these conditions, but that the crystals contain only four molecules of water of crystallization. The reaction, moreover, is shown to proceed in the following manner:—

$$PtCl_4 . 2HCl + 2AgNO3 = 2AgCl + PtCl_4 + 2HNO_3.$$

The best mode of preparing the neutral chloride of platinum, according to Engel, consists in dissolving in a solution of the chloroplatinate the necessary quantity of oxide of platinum, prepared by Fremy's method, in order to neutralize the excess of hydrochloric acid. The filtered liquid, on evaporation, then deposits beautiful crystals of $PtCl_4 . 4H_2O$, permanent in the air, and not at all deliquescent like the chloroplatinate. The composition of these crystals was determined both by weighing the metallic platinum left on calcination of a known weight, and by estimation of the chlorine by fusion of the crystals with carbonate of potash and precipitation with silver nitrate. The water was, of course, given by difference. In spite of the stability of the chloroplatinate, it is a somewhat curious fact that the powdered crystals of the neutral chloride do not take up hydrochloric acid gas at ordinary temperatures. At about 50° C., however, the chloride partially liquefies under the influence of a dry current of the gas, forming the chloroplatinate. As might be expected from its non-deliquescence, the new chloride is very much less soluble in water than is the ordinary chloroplatinate.

THE Portuguese Government has given notice that from August 1 meteorological signals will be established at six semaphore stations along its coast, between the River Douro and Cape St. Vincent, and shown to passing vessels requiring information as to the state of the weather in the Bay of Biscay, at Gibraltar, and at Madeira. Each notice will indicate the time to which the information refers, the locality to which it has reference, and the direction and force of the wind, together with any other particulars which the Lisbon Observatory may consider it expedient to give. The signals will usually be made by flags, of the International Code of Signals, or by semaphore, when colours of flags would not be easily distinguished. This useful information is at present only to be obtained from very few countries.

IN the new number of the Journal of the Anthropological Institute there is an interesting note, by Mr. Basil Hall Chamberlain, on the Japanese "go-hei," or paper offerings to the Shinto gods. It has been thought by some European travellers that the Japanese, prompted by equal frugality and irreverence, offer paper to their gods because it is the cheapest article at hand. Mr. Chamberlain suggests a more reasonable explanation. Though paper is now used in the ceremonies of the Shinto religion, this was not so in days preceding the eighth century of the Christian era. The offerings then were made of two kinds of cloth—a white kind made of the paper-mulberry (*Broussonetia papyrifera*), and a blue kind made of hemp. Such cloth was the most precious article in the possession of a population to whom luxury and art were unknown. Later on, when Chinese civilization had brought a variety of manufactures in its train, hempen cloth ceased to be regarded as a treasure worthy of the divine acceptance; and, frugality perhaps helping, and partly also in accordance with that law of progress from the actual to the symbolical which characterizes all religions, paper began to be used instead. Mr. Chamberlain is unable to determine the date of the change, Shinto having suffered such an eclipse from the eighth to the seventeenth century that little regarding its mediæval history has been preserved. During all that time, Buddhism reigned supreme. Speaking of the general character of Shinto as a national religion, Mr. Chamberlain says that even native commentators, over anxious as they are to magnify everything Japanese at the expense of everything foreign, acknowledge that it has no moral system, no body of views of any kind save worship of the gods who were the ancestors of the Imperial House. For this reason Shinto collapsed utterly at the touch of Buddhism, and it fails to support itself now, when an attempt is being made to revive it for political purposes. It has nothing in it that appeals to the religious instincts of the people.

MESSRS. GEORGE PHILIP AND SON announce that they have made arrangements for the publication in December next of

"The Educational Annual," a handy reference volume of about 200 crown octavo pages on educational subjects, which is likely to prove a convenience to school managers, teachers, and others interested in the promotion of national education. It is proposed to review elementary education, technical education, agricultural education, industrial, reformatory, truant, and ragged schools, secondary education, and, generally, the purpose and work of the Education Department, the Science and Art Department, the training of teachers, and the teachers' organizations.

MESSRS. SONNENSCHEIN AND CO. will issue shortly a translation of Moritz Hauptmann's "Nature of Harmony and Metre." The work consists of three parts. The first part considers the evolution of harmony from acoustics, taking as basis the Hegelian theory of sound. In the second part the author discusses metre and rhythm, which are respectively analogous to harmony and melody. The last part of the book is concerned with the union of metre and harmony—that is, harmony and melody in concrete combination with metre and rhythm.

A SPECIMEN of the golden mullet (*Mugil auratus*, Risso), 320 mm. in length, has been caught at Stromstad, on the southwest coast of Sweden. Only once before has a specimen of this fish been caught on the Swedish coast.

THE authorities of the Mason Science College, Birmingham, have issued the syllabus of day classes to be held during the session 1888–89.

ACCORDING to the *American Naturalist*, the proposed site of the National Zoological Park at Washington is one of great beauty, and even grandeur. It is in the valley of Rock Creek, just beyond the city limits, and at two points walls of rock rise to a height of over 80 feet. The Rock Creek will afford what the *American Naturalist* describes as "unrivalled facilities" for the care of aquatic mammals and birds of all kinds. Nearly the whole tract is covered by a fine growth of forest trees.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented Mr. William Norman; a Lesser White-nosed Monkey (*Cercopithecus petaurista* ♂) from West Africa, presented by Mr. W. Blandford Griffith; a Tiger (*Felis tigris* ♂) from India, presented by Sir E. C. Buck, C.M.Z.S.; a Bengal Cat (*Felis bengalensis*) from India, presented by Mr. W. L. Sclater, F.Z.S.; a Black-backed Piping Crow (*Gymnorhina leuconota*), two Leadbeater's Cockatoos (*Cacatua leadbeateri*) from Australia, a Common Magpie (*Pica rustica*), four Common Herons (*Ardea cinerea*), British, two Himalayan Monauls (*Lophophorus impeyanus* ♂ ♂) from the Himalayas, two Gold Pheasants (*Thaumalea picta* ♂ ♀), two Silver Pheasants (*Euplocamus nycthemerus* ♂ ♀), two Mandarin Ducks (*Æx galericulata*) from China, a Javan Pea-fowl (*Pavo spicifer* ♂) from Java, two Common Pea-fowls (*Pavo cristatus* ♂ ♀) from India, a Rose-crested Cockatoo (*Cacatua moluccensis*) from Moluccas, a Hyacinthine Macaw (*Ara hyacinthina*), a Blue and Yellow Macaw (*Ara ararauna*) from South America, a Great Eagle Owl (*Bubo maximus*), European, presented by Mr. Charles Clifton, F.Z.S.; a Bare-eyed Cockatoo (*Cacatua gymnopis*) from North Australia, presented by Mrs. Fishlock; an Imperial Eagle (*Aquila imperalis*) from Morocco, presented by Mrs. Ernest H. Forwood; two American Box Tortoises (*Terrapene carinata*), two Alligator Terrapins (*Chelydra serpentina*), a Speckled Terrapin (*Clemmys guttata*), four Sculptured Terrapins (*Clemmys insculpta*) from North America, presented by Prof. O. C. Marsh, C.M.Z.S.; a Horned Lizard (*Phrynosoma cornutum*) from North America, presented by Master Howard Sexton; six Guinea Pigs (*Cavia porcellus*, var.), presented by Mr. R. F. Bennett; a Common Kingfisher (*Alcedo ispida*), British, deposited; a New Zealand Parrakeet (*Cyanorhamphus novæ-zealandiæ*) from New Zealand, purchased; two Chinchillas (*Chinchilla lanigera*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

COMET 1888 *c* (BROOKS).—Dr. H. Kreutz (*Astr. Nachr.*, No. 2853) has computed the following elements and ephemeris for this comet from observations made at Vienna on August 9, and at Strassburg on August 10 and 11. The middle place was represented closely.

$$T = 1888 \text{ July } 16{,}\ 1982, \text{ Berlin M.T.}$$

$$\left.\begin{aligned}\omega &= 34^\circ\ 36'{\cdot}90\\ \Omega &= 94\ \ 59{\cdot}69\\ \iota &= 71\ \ 25{\cdot}07\end{aligned}\right\}\ \text{Mean Eq. } 1888{\cdot}0.$$

$$\log q = 9{\cdot}92444$$

$$\begin{aligned}x &= [9{\cdot}51743]\, r \,.\, \sin (v + 229^\circ\ 57'{\cdot}12)\\ y &= [9{\cdot}99943]\, r \,.\, \sin (v + 148\ \ 23{\cdot}72)\\ z &= [9{\cdot}97573]\, r \,.\, \sin (v + \ \ 59\ \ 24{\cdot}32)\end{aligned}$$

Ephemeris for Berlin Midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Aug. 23	12 5 53	42 14·0 N.	0·0390	0·2201	0·74
25	12 19 20	41 26·1			
27	12 32 20	40 33·4	0·0568	0·2254	0·67
29	12 44 50	39 36·5			
31	12 56 49	38 36·1	0·0746	0·2326	0·60
Sept. 2	13 8 17	37 32·8			
4	13 19 16	36 27·2	0·0921	0·2413	0·53
6	13 29 45	35 19·9			
8	13 39 46	34 11·3 N.	0·1094	0·2514	0·47

The brightness on August 9 is taken as unity.

YALE COLLEGE OBSERVATORY.—The Reports of this Observatory for the last two years have recently been published. That for the year 1886–87 notes the retirement of Mr. Orray T. Sherman, who had charge of the Thermometric Bureau up to the date of his resignation in November 1886, and the renewal of subscriptions for the support of the work with the heliometer for another period of three years. Prof. Loomis had borne the expense of printing and distributing Dr. Elkin's memoir upon the Pleiades, and a second grant of 600 dollars had been made from the Bache Fund to enable Mr. Asaph Hall, Jun., the Assistant Astronomer at the Observatory, to carry on his observations of Titan for the determination of the mass of Saturn. Dr. Elkin had continued his heliometer measures for the determination of the mean parallax of the first magnitude stars; and the Report for 1887–88 records the completion of this work, and gives the results for the ten stars observed. These are as follows:—

Aldebaran	+ 0″·116 ± 0″·029	α Leonis	+ 0″·093 ± 0″·048
Capella	+ 0·107 ± 0·047	Arcturus	+ 0·018 ± 0·022
α Orionis	− 0·009 ± 0·049	α Lyræ	+ 0·034 ± 0·045
Procyon	+ 0·266 ± 0·047	α Aquilæ	+ 0·199 ± 0·047
Pollux	+ 0·068 ± 0·047	α Cygni	− 0·042 ± 0·047

The probable errors include an estimation of the probable systematic error of the measures, and are not as usual confined to the mere casual error of observation.

The results for Procyon and α Aquilæ are in close accord with those obtained by Auwers and Wagner for the first star, and by W. Struve for the second; and that for Aldebaran agrees with Prof. Asaph Hall's value; the value found by O. Struve—viz. + 0″·516—would appear, therefore, to be erroneous. But Dr. Elkin's parallax for α Lyræ is much smaller than the results which have been hitherto obtained by other observers, and which give in the mean a parallax quite five times as great as he has found. But the most remarkable result is that obtained for Arcturus, the practically insensible parallax of which seems in such strong contrast to its large proper motion. Dr. Elkin is well satisfied that the parallax of this star is extremely small, for his value depends upon eighty-nine observations and on five pairs of comparison stars, all in reasonable agreement.

The mean of the ten parallaxes gives for the mean parallax of a first magnitude star—

$$+ 0''{\cdot}089 \pm 0''{\cdot}015,$$

a result according well with the values deduced by Gyldén (0″·084) and Peters (0″·102).

The heliometer is at present engaged on a triangulation of stars near the North Pole for Prof. Pickering, but the last three months of the present year it is to be employed in the determination of the solar parallax during the extremely favourable opposition of Iris. Measures of the diameters of the sun and of Mars, measures of certain double stars, the investigation of the parallaxes of 6 B Cygni, and of 18115/22 Lalande, are amongst the other labours of the Observatory. Mr. Hall has nearly completed the reduction of his measures of Titan.

GRAVITATION IN THE STELLAR SYSTEMS.—Prof. Asaph Hall supplies an interesting paper on "The Extension of the Law of Gravitation to Stellar Systems," in *Gould's Astronomical Journal*, No. 177, to which Dr. Elkin's new value of the parallax of Arcturus might afford a most striking illustration. Prof. Hall shows that there is a theoretical difficulty in proving the law of Newton for double stars which we cannot overcome, though the probability of the existence of this law can be increased as more double star orbits, and those very differently situated, are determined. Still, even then, before the universality of the law can be inferred, there remains the difficulty of the so-called "runaway" stars, like Groombridge 1830, stars moving through space with the speed of a comet at perihelion, and yet with no visible attracting body near them. Of these Prof. Hall supplies a list. But if Dr. Elkin's value of the parallax of Arcturus be accepted, that star would outstrip any of those given in this table. For its speed in the direction at right angles to the line of sight would be 373 miles per second, a speed compared with which its speed in the line of sight, as given by Dr. Huggins, 55 miles per second, becomes small. Prof. Hall concludes, therefore, that though Newton's law is one of the greatest generalizations of science, it is better and safer "to await further knowledge before we proceed, as Kant has done, to construct the universe according to this law."

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 AUGUST 26—SEPTEMBER 1.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 26

Sun rises, 5h. 5m.; souths, 12h. 1m. 28·9s.; sets, 18h. 57m.: right asc. on meridian, 10h. 22·1m.; decl. 10° 11′ N. Sidereal Time at Sunset, 17h. 19m.

Moon (at Last Quarter August 29, 14h.) rises, 20h. 54m.*; souths, 3h. 22m.; sets, 10h. 2m.: right asc. on meridian, 1h. 41·6m.; decl. 5° 0′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	5 14	12 13	19 12	10 33·8	10 52 N.
Venus	6 14	12 51	19 28	11 11·5	6 42 N.
Mars	12 27	16 48	21 9	15 9·6	19 12 S.
Jupiter	13 2	17 23	21 44	15 44·5	19 8 S.
Saturn	3 6	10 43	18 20	9 3·5	17 33 N.
Uranus	8 59	14 35	20 11	12 56·1	5 20 S.
Neptune..	21 56*	5 43	13 30	4 2·3	18 59 N.

* Indicates that the rising is that of the preceding evening.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′		h. m.	
R Arietis	2 9·8	24 32 N.	Aug. 26,		*M*
R Ceti	2 20·3	0 41 S.	,, 27,		*M*
λ Tauri	3 54·5	12 10 N.	,, 27,	22 41	*m*
			,, 31,	21 33	*m*
U Monocerotis	7 25·5	9 33 S.	,, 27,		*m*
R Virginis	12 32·8	7 36 N.	,, 31,		*M*
S Boötis	14 19·1	54 19 N.	,, 27,		*M*
δ Libræ	14 55·0	8 4 S.	,, 30,	22 8	*m*
W Scorpii	16 5·2	19 51 S.	,, 28,		*M*
W Ophiuchi	16 15·4	7 26 S.	,, 28,		*M*
R Draconis	16 32·4	67 0 N.	,, 30,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 30,	0 28	*m*
			,, 30,	20 36	*m*
W Sagittarii	17 57·9	29 35 S.	,, 27,	0 0	*M*
Z Sagittarii	18 14·8	18 55 S.	,, 27,	1 0	*M*
			,, 31,	0 0	*m*
U Sagittarii	18 25·3	19 12 S.	,, 26,	0 0	*M*
β Lyræ	18 46·0	33 14 N.	,, 26,	1 0	m_2
R Aquilæ	19 1·0	8 4 N.	,, 29,		*m*
X Cygni	20 39·0	35 11 N.	Sept. 1,	3 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Occultation of Star by the Moon (visible at Greenwich).

Aug.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.	
			h. m.	h. m.	°	°
26	ξ² Ceti	4	23 20	0 22†	98	238

† Occurs on the following morning.

Meteor-Showers.

	R.A.	Decl.	
	°	°	
	6	11 N.	Swift.
Near β Trianguli	30	35 N.	
,, 33 Cygni	305	54 N.	Swift, bright. Sept. 1.
,, δ Cephei	336	58 N.	Swift.

GEOGRAPHICAL NOTES.

THE *Times* printed on Tuesday the substance of communications received from Mr. Joseph Thomson, dated from the city of Morocco, July 22. Mr. Thomson writes in the highest spirits, and with evident satisfaction at the results he has so far attained; for much of the country through which he has had to pass is in a state of rebellion, and the local authorities have done more to hinder than to help him. Mr. Thomson sailed from Tangier to Casablanca, and thence travelled overland to Mogador. After three weeks' preparation there he made his final start, and, as he states, soon discovered that the greatest danger to his success would not be the mountaineers nor even the opposition of the Government officials, but the half-dozen men who formed the *personnel* of his small party. Mr. Thomson's past experience in Africa enabled him to deal effectively with this difficulty. By a series of surprises and cleverly-planned excursions he has been able to enter the mountain fastnesses of Morocco and do more than any previous traveller has done. From Demnat he made two extremely interesting trips into the lower ranges, visiting some remarkable caves and equally remarkable ruins, and one of the most wonderful natural bridge-aqueducts in the world. Geologically and geographically these trips are alike important. They were followed by a dart across the main axis of the Atlas to the district of Tiluit, which lies in the basin of the Draa. Here he spent a very delightful ten days, though virtually a prisoner. As the tribes further west on the southern slope were in revolt, Mr. Thomson was compelled to return to the northern plains. Starting once more, he crossed the mountains by a pass a little south of Jebel Tizah, ascended by Hooker, and reached Gindafy safely. He was able to make a trip up a wonderful cañon, which he declares rivals those of America for depth and grandeur, and ascended a mountain, where he and his party were confined to their tents until it suited them to go back to their starting-point. Here, unfortunately, Mr. Thomson's young companion, Mr. Crichton Browne, was stung by a scorpion, and they were compelled to return, happily by a new route. Though laid up for a period, fortunately in time Mr. Crichton Browne recovered. From his previous starting-point Mr. Thomson scored another great triumph. He crossed the mountains once more, and ascended with no small danger and difficulty the highest peak of the Atlas Range north of Amsiviz, a height of 12,500 feet—the highest peak, by 1500 feet, ever attained. This he describes as the most interesting of all his trips, and he enjoyed it thoroughly, though he had to sleep on the ground and was glad to make a meal on walnuts. On his return, Mr. Thomson deemed it advisable to go into the town of Morocco to recruit and wait the arrival of further supplies from the coast. He intended to resume work in a few days after the date of his letter. He proposed first to make for the Urika River and penetrate the mountains up its course. He will then work his way round to Mogador, which he expects to reach about the end of August. There probably his work of exploration will end, though he may make one or two short trips into the interior and down to Agadir. The return route to Tangier will probably be from Mogador to the city of Morocco, thence to Mazagan on the coast, and on to Casablanca and Rabat. Then he will leave the sea again and go to Mequinez and Fez, reaching Tangier about the end of the year. The *Times* understands that his contributions to various branches of science, especially to botany, will be of the highest value.

A LETTER from Cayenne to the *Temps* states that M. Coudreau, who has recently explored Guiana, arrived there last month after having travelled for eleven months in the western range of the Tumuc-Humac Mountains, between the source of the Itany and that of the Camopy. Starting by the Maroni, M. Coudreau, after having gone up the Itany and explored the region which it waters, came down to the coast by Maronimi-Crique, which is a very large tributary of the Maroni River. M. Coudreau is the first Frenchman who has passed a consecutive winter and summer in the Tumuc-Humac Mountains, and though he did not himself suffer very much from the effects of the expedition, the same cannot be said of his companions, as the only European who accompanied him was brought near to death's door by fever, from which most of the natives also suffered. M. Coudreau escaped with nothing worse than rheumatism, and he says that the climate of the Western Tumuc-Humac is not bad. The result of 1200 observations taken by him puts the mean temperature at 70°, and the country is a magnificent one; but the difficulty of reaching it is very great owing to the uncertainty of communication with the coast. M. Coudreau and his companions, when they had exhausted their provisions, had to go and live out in the open with the Indians, leading the same kind of existence, and depending for food upon the game, fish, and fruit that they could shoot, fish, and gather. For eight months M. Coudreau lived the regular native life, and he had become so accustomed to it that he was very popular with the Rucuyennes, whose language he had learned to speak, and he induced the *pamenchi* (captain) of the tribe and four of his lieutenants to accompany him to Cayenne, where their arrival created a great sensation, as the people of the town did not believe in their existence. M. Gerville-Réache, the Governor of the colony, received them with great hospitality, and made them several presents. The most important fact brought out by M. Coudreau is the existence in Upper Guiana, which is acknowledged French territory, of sixteen new Indian tribes, forming a group of at least 20,000 persons; and these Indians are not, as was supposed, mere nomads, living upon the produce of their guns and fishing-nets, but are sedentary in their habits, and have attained a certain degree of civilization. M. Coudreau is about to start on a fresh expedition to the Appruague and the Oyapack, and does not expect to get back before next spring.

THE GASES OF THE BLOOD.[1]

II.

THE next step was the discovery of the important part performed in respiration by the colouring matter of the red blood corpuscles. Chemically, these corpuscles consist of about 30 or 40 per cent. of solid matter. These solids contain only about 1 per cent. of inorganic salts, chiefly those of potash; whilst the remainder are almost entirely organic. Analysis has shown that 100 parts of dry organic matter contain of hæmoglobin, the colouring matter, no less than 90·54 per cent.: of proteid substances, 8·67; of lecithin, 0·54; and of cholesterine, 0·25. The colouring matter, hæmoglobin, was first obtained in a crystalline state by Funke in 1853, and subsequently by Lehmann. It has been analyzed by Hoppe-Seyler and Carl Schmidt, with the result of showing that it has a perfectly constant composition. Hoppe-Seyler's analysis first appeared in 1868. It is now well known to be the most complicated of organic substances, having a formula, as deduced, from the analyses I have just referred to, by Preyer (1871), of

$$C_{600}H_{960}N_{154}FeS_3O_{179}.$$

In 1862, Hoppe-Seyler noticed the remarkable spectrum produced by the absorption of light by a very dilute solution of blood. Immediately thereafter, the subject was investigated by Prof. Stokes, of Cambridge, and communicated to the Royal Society in 1864. If white light be transmitted through a thin stratum of blood, two distinct absorption bands will be seen. One of these bands next D is narrower than the other, has more sharply defined edges, and is undoubtedly blacker. "Its centre," as described by Dr. Gamgee ("Physiological Chemistry," p. 97), "corresponds with wave-length 579,[2] and it may conveniently be distinguished as the absorption band, *α*, in the spectrum of oxyhæmoglobin. The second of the absorption bands—that is, the one next to E—which we shall designate *β*, is broader, has less sharply defined edges, and is not so dark as *α*. Its centre corresponds approximately to wave-length 553·8. On diluting very largely with water, nearly the whole of the spectrum appears beautifully clear, except where the two absorption bands are situated. If dilution be pursued far enough, even these disappear; before they disappear they look like faint shadows obscuring the limited part of the spectrum which they occupy. The last to disappear is the band *α*. The two absorption bands are seen most distinctly when a stratum of 1 cm. thick of a solution containing 1 part of hæmoglobin in 1000 is examined; they are still perceptible when the solution contains only 1 part of hæmoglobin in 10,000 of water."

Suppose, on the other hand, we begin with a solution of blood in ten times its volume of water; we then find that such a solution cuts off the more refrangible part of the spectrum, leaving nothing except the red, "or, rather, those rays having a wave-length greater than about 600 millionths of a millimetre." On diluting further, the effects, as well described by Prof. Gamgee, are as follows:—"If now the blood solution be rendered much more dilute, so as to contain 8 per cent. of hæmoglobin, on examining a spectrum 1 centimetre wide the spectrum becomes distinct up to Fraunhofer's line D (wave-length 589)—that is, the red, orange, and yellow are seen, and in addition also a portion of the green, between *b* and F. Immediately beyond D, and between it and *b*, however (between wave-lengths 595 and 518), the absorption is intense."

These facts were observed by Hoppe-Seyler. Prof. Stokes made the very important contribution of observing that the spectrum was altered by the action of reducing agents. Hoppe-Seyler had observed that the colouring matter, so far as the spectrum was concerned, was unaffected by alkaline carbonates, and caustic ammonia, but was almost immediately decomposed by acids, and also slowly by caustic fixed alkalies, the coloured product of decomposition being hæmatin, the spectrum of which was known. Prof. Stokes was led to investigate the subject from its physiological interest, as may be observed on quoting his own words in the classical research already referred to. "But it seemed to me to be a point of special interest to inquire whether we could imitate the change of colour of arterial into that of venous blood, on the supposition that it arises from reduction."

He found that—

"If to a solution of proto-sulphate of iron enough tartaric acid be added to prevent precipitation by alkalies, and a small quantity of the solution, previously rendered alkaline by either ammonia or carbonate of soda, be added to a solution of blood, the colour is almost instantly changed to a much more purple-red as seen in small thicknesses, and a much darker red than before as seen in greater thickness. The change of colour which recalls the difference between arterial and venous blood is striking enough, but the change in the absorption spectrum is far more decisive. The two highly characteristic dark bands seen before are now replaced by a single band, somewhat broader and less sharply defined at its edges than either of the former, and occupying nearly the position of the bright band separating the dark bands of the original solution. The fluid is more transparent for the blue and less so for the green than it was before. If the thickness be increased till the whole of the spectrum more refrangible than the red be on the point of disappearing, the last part to remain is green, a little beyond the fixed line *b*, in the case of the original solution, and blue some way beyond F, in the case of the modified fluid."

From these observations, Prof. Stokes was led to the important conclusion that—

"The colouring matter of blood, like indigo, is capable of existing in two states of oxidation, distinguishable by a difference of colour and a fundamental difference in the action on the spectrum. It may be made to pass from the more to the less oxidized by the action of suitable reducing agents, and recovers its oxygen by absorption from the air."

To the colouring matter of the blood Prof. Stokes gave the name of cruorine, and described it in its two states of oxidation as scarlet cruorine and purple cruorine. The name hæmoglobin, given to it by Hoppe-Seyler, is generally employed. When united with oxygen it is called oxyhæmoglobin, and when in the reduced state it is termed reduced hæmoglobin, or simply hæmoglobin.

The spectroscopic evidence is, therefore, complete. Hoppe-Seyler, Hüfner, and Preyer have shown also that pure crystallized hæmoglobin absorbs and retains in combination a quantity of oxygen equal to that contained in a volume of blood holding the same amount of hæmoglobin. Thus, 1 gramme of hæmoglobin absorbs 1·56 cubic centimetre of oxygen at 0° C. and 760 milli-

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[1] Address to the British Medical Association at its annual meeting at Glasgow. Delivered on August 10 in the Natural Philosophy class-room, University of Glasgow, by John Gray McKendrick, M.D., LL.D., F.R.SS.L and E., F.R.C.P.E., Professor of the Institutes of Medicine in the University of Glasgow. Continued from p. 382.

[2] Dr. Gamgee gives the measurements of the wave-lengths in millionths, not in ten-millionths of a millimetre.

metres pressure; and, as the average amount of hæmoglobin in blood is about 14 per cent., it follows that 1·56 × 14 = 21·8 cubic centimetres of oxygen would be retained by 100 cubic centimetres of blood. This agrees closely with the fact that about 20 volumes of oxygen can be obtained from 100 volumes of blood. According to Pflüger, arterial blood is saturated with oxygen to the extent of nine-tenths, while Hüfner gives the figure at fourteen-fifteenths. By shaking blood with air, its oxygen contents can be increased to the extent of from 1 to 2 volumes per cent.

These important researches, the results of which have been amply corroborated, have given an explanation of the function of the red blood corpuscles as regards respiration. The hæmoglobin of the venous blood in the pulmonary artery absorbs oxygen, becoming oxyhæmoglobin. This is carried to the tissues, where the oxygen is given up, the hæmoglobin being reduced. Thus, the colouring matter of the red blood corpuscles is constantly engaged in conveying oxygen from the lungs to the tissues. Probably the union of hæmoglobin with oxygen, and its separation from it, are examples of dissociation—that is, of a chemical decomposition or synthesis, effected entirely by physical conditions; but data regarding this important question are still wanting. If the union of oxygen with the colouring matter is an example of oxidation, it must be attended with the evolution of heat, but, so far as I know, this has not been measured. In co-operation with my friend, Mr. J. T. Bottomley, I have recently been able to detect, by means of a thermo-electric arrangement, a rise of temperature on the formation of oxyhæmoglobin. We mean to prosecute our researches in this direction. If heat were produced in considerable amount, the arterial blood returned from the lungs to the left auricle would be hotter than the blood brought to the right auricle by the veins. This, however, is not the case, as the blood on the right side of the heart is decidedly warmer than the blood on the left—a fact usually accounted for by the large influx of warm blood coming from the liver. The heat-exchanges in the lungs are of a very complicated kind. Thus, heat will be set free by the formation of oxyhæmoglobin; but, on the other hand, it will be absorbed by the escape of carbonic acid, and by the formation of aqueous vapour, and a portion will be used in heating the air of respiration. The fact that the blood in the left auricle is colder than that of the right auricle is, therefore, the result of a complicated series of heat-exchanges, not easy to follow.

Our knowledge as to the state of the carbonic acid in the blood is not so reliable. In the first place, it is certain that almost the whole of the carbonic acid which may be obtained exists in the plasma. Defibrinated blood gives up only a little more carbonic acid than the same amount of serum of the same blood. Blood serum gives up to the vacuum about 30 volumes per cent. of carbonic acid; but a small part—according to Pflüger, about 6 volumes per cent.—is given up only after adding an organic or mineral acid. This smaller part is chemically bound, just as carbonic acid is united to carbonates, from which it can be expelled only by a stronger organic or mineral acid. The ash of serum yields about one-seventh of its weight of sodium; this is chiefly united to carbonic acid to form carbonates, and a part of the carbonic acid of the blood is united to those salts. It has been ascertained, however, that defibrinated blood, or even serum containing a large number of blood corpuscles, will yield a large amount of carbonic acid, even without the addition of an acid. Thus, defibrinated blood will will yield 40 volumes per cent. of carbonic acid—that is, 34 volumes which would be also given up by the serum of the same blood (without an acid), and 6 volumes which would be yielded after the addition of an acid. Something, therefore, exists in defibrinated blood which acts like an acid in the sense of setting free the 6 volumes of carbonic acid. Possibly the vacuum may cause a partial decomposition of a portion of the hæmoglobin, and, as suggested by Hoppe-Seyler, acid substances may thus be formed.

But what is the condition of the remaining 30 volumes per cent. of carbonic acid which are obtained by the vacuum alone? A portion of this is probably simply absorbed by the serum; this part escapes in proportion to the decrease of pressure, and it may be considered to be physically absorbed. A second part of this carbonic acid must exist in chemical combination, as is indicated by the fact that blood serum takes up far more carbonic acid than is absorbed by pure water. On the other hand, this chemical combination is only a loose one, because it is readily dissolved by the vacuum. There can be no doubt that a part of this carbonic acid is loosely bound to carbonate of soda, Na_2CO_3, in the serum, probably to acid carbonate of soda, $NaHCO_3$. This compound exists only at a certain pressure. On a fall of pressure, it decomposes into sodium carbonate and carbonic acid, the latter becoming free. A third part of this carbonic acid is probably loosely bound chemically to disodium phosphate, Na_2HPO_4, a salt which also occurs in the blood serum. Fernet has shown that it binds two molecules of carbonic acid to one molecule of phosphoric acid. This salt occurs in considerable quantity only in the blood of Carnivora and Omnivora, while in that of Herbivora, such as in the ox and calf, only traces exist. It cannot be supposed in the latter instances to hold much carbonic acid in chemical combination. There must exist, therefore, other chemical substances for the attachment of the carbonic acid of the blood, and it has been suggested that a part may be connected with the albumin of the plasma.

According to Zuntz, the blood corpuscles themselves retain a part of the carbonic acid, as the total blood is able to take up far more carbonic acid out of a gaseous mixture rich in carbonic acid, or consisting of pure carbonic acid, than can be absorbed by the serum of the same quantity of blood. No compound, however, of carbonic acid with the blood corpuscles is known.

The nitrogen which is contained in the blood to the amount of from 1·8 to 2 volumes per cent., is probably simply absorbed, for even water is able to absorb to 2 volumes per cent. of this gas.

If we then regard the blood as a respiratory medium having gases in solution, we have next to consider what is known of the breathing of the tissues themselves. Spallanzani was undoubtedly the first to observe that animals of a comparatively simple type used oxygen and gave up carbonic acid. But he went further, and showed that various tissues and animal fluids, such as the blood, the skin, and portions of other organs, acted in a similar way. These observations were made before the beginning of the present century, but they appear to have attracted little or no attention until the researches of Georg Liebig on the respiration of muscle, published in 1850. He showed that fresh muscular tissue consumed oxygen and gave up carbonic acid. In 1856, Matteucci made an important advance, by observing that muscular contraction was attended by an increased consumption of oxygen, and an increased elimination of carbonic acid. Since then, Claude Bernard and Paul Bert, more especially the latter, have made numerous observations regarding this matter. Paul Bert found that muscular tissue has the greatest absorptive power. Thus we arrive at the grand conclusion that the living body is an aggregate of living particles, each of which breathes in the respiratory medium passing from the blood.

As the blood, containing oxygen united with the colouring matter (hæmoglobin), passes slowly through the capillaries, fluid matter transudes through the walls of the vessels, and bathes the surrounding tissues. The pressure or tension of the oxygen in this fluid being greater than the tension of the oxygen in the tissues themselves, in consequence of the oxygen becoming at once a part of the living protoplasmic substance, oxygen is set free from the hæmoglobin, and is appropriated by the living tissues, becoming part of their protoplasm. Whilst alive, or at all events whilst actively discharging their functions, as in the contraction of a muscle, or in those changes we term secretion in a cell, the living protoplasm undergoes rapid decompositions, leading to the formation of comparatively simple substances. Amongst these is carbonic acid. As it has been ascertained that the tension of the carbonic acid in the lymph is less than its tension in venous blood, it is difficult at first sight to account for the absorption of carbonic acid by venous blood; but its tension is higher than that of carbonic acid in arterial blood, and it must be remembered that the lymph has had the opportunity, both in the connective tissue and in the lymphatic vessels, of modifying its tension by close contact with arterial blood. Strassburg fixes the tension of the carbonic acid in the tissues as equal to 45 mm. of mercury, while that of the venous blood is only 41 mm. We may assume that as the carbonic acid is set free, it is absorbed by the blood, uniting loosely with the carbonates and phosphates of that fluid, thus converting it from the arterial into the venous condition. This constitutes respiration of tissue.

In connection with the respiration of tissue, as determined by the analysis of the blood gases and of the gases of respiration, there arises the interesting question of the ratio between the amount of oxygen absorbed and the amount of carbonic acid produced, and very striking contrasts among animals have thus been determined. Thus in Herbivora the ratio of the oxygen absorbed to the carbonic acid produced, or the respiratory quotient, as it is termed by Pflüger, $\frac{CO_2}{O}$ amounts to from 0· to 1·0, while in

Carnivora it is from 0·75 to 0·8. Omnivora, of which man may be taken as the example, come between $\frac{CO_2}{O} = 0·87$. The quotient is greater in proportion to the amount of carbohydrate in the diet, whether the animals are Carnivora, Herbivora, or Omnivora. The respiratory quotient becomes the same, about 0·75, in starving animals, a proof that the oxidations are kept up at the cost of the body itself, or, in other words, the starving animal is carnivorous. The intensity of respiration in different animals is well shown in the following table, in which the amount of oxygen used is given per kilogramme of body-weight per hour (Dr. Immanuel Munk, "Physiologie des Menschen und der Säugethiere," 1888, p. 82).

Animal.	O in grammes.	Respiratory Quotient, $\frac{CO_2}{O}$.
Cat	1·007	0·77
Dog	1·183	0·75
Rabbit	0·918	0·92
Hen	1·300	0·93
Small singing birds ...	11·360	0·78
Frog	0·084	0·63
Cockchafer	1·019	0·81
Man	0·417	0·78
Horse	0·563	0·97
Ox	0·552	0·98
Sheep	0·490	0·98

Smaller animals therefore have, as a rule, a greater intensity of respiration than larger ones. In small singing birds the intensity is very remarkable, and it will be seen that they require ten times as much oxygen as a hen. On the other hand, the intensity is low in cold-blooded animals. Thus a frog requires 135 times less oxygen than a small singing bird. The need of oxygen is therefore very different in different animals. Thus a guinea-pig soon dies with convulsions in a space containing a small amount of oxygen, while a frog will remain alive for many hours in a space quite free of oxygen. It is well known that fishes and aquatic animals generally require only a small amount of oxygen, and this is in consonance with the fact that sea-water contains only small quantities of this gas. Thus, according to the elaborate researches of my friend, Prof. Dittmar, on the gases of the sea-water brought home by the *Challenger* Expedition, collected in many parts of the great oceans, and from varying depths :—"The ocean can contain nowhere more than 15·6 c.c. of nitrogen, or more than 8·18 c.c. oxygen per litre ; and the nitrogen will never fall below 8·55 c.c. We cannot make a similar assertion in regard to the oxygen, because its theoretical minimum of 4·30 c.c per litre is liable to further diminution by processes of life and putrefaction and processes of oxidation" (Dittmar, Proceedings of Phil. Soc. of Glasgow, vol. xvi. p. 61). As a matter of fact, a sample of water from a depth of 2875 fathoms gave only 0·6 c.c. per litre of oxygen, while one from a depth of 1500 fathoms gave 2·04 c.c. per litre. Taking 15° C. as an average temperature, one litre of sea-water would contain only 5·31 c.c. of dissolved oxygen—that is, about 0·5 c.c. in 100 c.c. Contrast this with arterial blood, which contains 20 c.c. of oxygen in 100 c.c. of blood, or there are about forty times as much oxygen in arterial blood as in sea-water. At great depths the quantity of oxygen is very much less, and yet many forms of life exist at these great depths. Fishes have been dredged from a depth of 2750 fathoms, where the amount of oxygen was probably not so much as 0·06 c.c. per 100 c.c., or 300 times less than that of arterial blood. Making allowance for the smaller quantity of oxygen in the blood of a fish than that of a mammal, it will still be evident that the blood of the fish must contain much more oxygen than exists in the same volume of sea-water. No doubt we must remember that the water is constantly renewed, and that the oxygen in it is in the state of solution, or, in other words, in a liquid state. But the question remains, where do these deep-sea creatures obtain the oxygen? Probably by a method of storage. Biot has found in the swimming-bladder of such fishes 70 volumes per cent. of pure oxygen, a gas in which a glowing splinter of wood is relit. This oxygen probably oxygenates the blood of the fish when it plunges into the dark and almost airless depths of the ocean.

Aquatic breathers, however, if they live in a medium containing little oxygen, have the advantage that they are not troubled with free carbonic acid. One of the most striking facts discovered by the *Challenger* chemists is that sea-water contains no free carbonic acid, except in some situations where the gas is given off by volcanic action from the crust of the earth forming the sea-bed. In ordinary sea-water there is no free carbonic acid, because any carbonic acid formed is at once absorbed by the excess of alkaline base present. Thus the fish breathes on the principle of Fleuss's diving apparatus, in which the carbonic acid formed is absorbed by an alkaline solution. There is nothing new under the sun. The fish obtains the oxygen from the sea-water, no doubt, by the chemical affinity of its hæmoglobin, which snatches every molecule of oxygen it may meet with, while it gets rid of its carbonic acid easily, because there is not only no tension of carbonic acid in the sea-water to prevent its escape, but there is always enough of base in the sea-water to seize hold of the carbonic acid the moment it is formed. If we could get rid of the carbonic acid of the air of expiration as easily, we could live in an atmosphere containing a much smaller percentage of oxygen.

I have now placed before you the generally accepted doctrines regarding the chemical and physical problems of respiration. But one has only to examine them closely to find that there are still many difficulties in the way of a satisfactory explanation of the function. For example, is the union of hæmoglobin with oxygen a chemical or a physical process? If oxyhæmoglobin is a chemical substance, how can the oxygen be so readily removed by means of the air-pump? On the other hand, if it is a physical combination, why is the oxygen not absorbed according to the law of pressure? It is important to note that, as a matter of fact, hæmoglobin absorbs a quantity of oxygen nearly constant for ordinary temperatures, whatever may be the amount of oxygen present in the mixture of gases to which it is exposed. This is true so long as the amount of oxygen does not fall below a certain minimum, and it clearly points to the union of the hæmoglobin with the oxygen being a chemical union. Suppose we diminish the amount of oxygen in the air breathed, the partial pressure of the gas is of course also diminished, but it is evident that we might diminish the total pressure instead of diminishing the amount of oxygen. To avoid difficulties in respiration, when one is obliged to breathe an air deficient in oxygen, we ought to increase the pressure at which the air is breathed ; and, on the other hand, to avoid danger in breathing air under a low pressure, we ought theoretically to increase the richness of the air in oxygen. Thus, with a pressure of 760 mm. the air should contain, as it normally does, 21 per cent. of oxygen, while with a pressure of 340 mm. it should contain 46 per cent., and with a pressure of 250 mm. it should contain as much as 63 per cent. On this basis a pressure of 5 atmospheres should be associated with an atmosphere containing about 3 per cent. of oxygen. By increasing the pressure, we increase the quantity of oxygen by weight in a given volume.

The explanation is that in all of these cases the partial pressure of the oxygen is nearly the same—that is, not far from 157 mm. of mercury, and the general law is that for all kinds of breathing the pressure of the oxygen should be nearly that of the oxygen in ordinary atmospheric air. Whilst the absorption of oxygen by the hæmoglobin has nothing directly to do with the pressure, it is striking that any atmosphere contains enough oxygen by weight for the hæmoglobin in the blood, when the partial pressure of the oxygen is near 157 mm. On each side of this median line life can be supported with considerable differences of pressure. Thus the pressure may be gradually reduced until the point of the dissociation of oxyhæmoglobin is reached—that is to say, down to about $\frac{1}{10}$ of an atmosphere. On the other hand, animals may breathe an atmosphere containing two or three times the normal amount of oxygen without appearing to be affected. This was first noticed by Regnault and Reiset, and the observation has been much extended by Paul Bert. The latter distinguished physiologist found that an increase even up to 8 or 10 atmospheres did not produce any apparent effect, but on reaching the enormous pressure of 20 atmospheres, death, with severe tetanic convulsions, was the result. He also showed that the additional increment of oxygen absorbed by the blood under the influence of each atmosphere of added pressure was very small. Thus, with a pressure of 1 atmosphere the amount of oxygen absorbed by the blood was about 20 per cent. by volume, a pressure of 2 atmospheres caused an increase of only 0·9 per cent., of 3 atmospheres 0·7 per cent,, of 4 atmospheres 0·6 per cent., of 5 atmospheres 0·5 per cent., of 6 atmospheres 0·2 per cent., of 7 atmospheres 0·2 per cent., of 8 atmospheres 0·1 per cent., of 9 atmospheres 0·1 per cent., and of 10 atmospheres 0·1 per cent. Thus from 1 atmosphere to 10 atmospheres the increase was only to the extent of 3·4 per cent., so that the blood now contained 23·4 per cent. by volume instead of 20 per cent. These facts indicate that when all the hæmoglobin has been satisfied with oxygen it becomes indifferent, within limits, to any additional oxygen that may be forced into the blood under pressure, and thus the blood of animals breathing an atmosphere richer in oxygen than ordinary air is not more highly oxygenated than normal blood. The practical result also follows that it is of no use in the treatment of disease to cause patients to breathe an atmosphere richer in oxygen than ordinary air, because, at ordinary atmospheric pressure, no more oxygen can thus be caused to enter the blood, and if it be desirable to hyperoxygenate the blood, this can only be done by breathing oxygen, under a pressure of three or four atmospheres, in a chamber in which the body of the patient is subjected to the same pressure.

In this connection it is important to notice the enormous absorptive surface for oxygen presented by the red blood corpuscles of man. There are about 5,000,000 red corpuscles in each cubic millimetre. Each corpuscle has a superficial area of 0·000128 square millimetre. Taking the blood in the body of a man of average size at 4·5 litres, that is 4,500,000 cubic millimetres, the number of corpuscles is about 22,500,000,000,000, and this would give a superficial area of 2,880,000,000 square millimetres, or 2880 square metres, or about 3151 square yards—that is to say, the absorptive area of the blood corpuscles is equal to that of a square having each side about 56 yards. The hæmoglobin in a red blood corpuscle amounts to about $\frac{12}{13}$ of its weight. The blood of a man of average size may be taken at 4536 grammes, or about 10 pounds. Such blood contains about 13·083 per cent. of hæmoglobin, and 4536 grammes will contain about 593 grammes of hæmoglobin, or about 1$\frac{1}{3}$ pound. As regards the iron, which is supposed to be an essential constituent of hæmoglobin, 100 grammes of blood contain 0·0546 gramme. It follows that the total amount, 4536 grammes, contain about 2·48 grammes, or nearly 39 grains. Twenty-five minims of the tinctura ferri perchloridi contain about 1 grain of pure iron, so it will be seen that not many doses are required to introduce into the body an amount of iron as large as exists in the whole of the blood.

The absorption of oxygen, therefore, probably takes place as follows: the inspired air is separated in the alveoli of the lung by delicate epithelial cells and the endothelial wall of the pulmonary capillaries from the blood which circulates in the latter. The exchange of gas takes place through these thin porous membranes, so that the velocity of the transit must be practically instantaneous. As the oxygen is bound loosely to the hæmoglobin of the corpuscles, the laws of diffusion can have only a secondary influence on its passage, and only so far as it has to pass into the plasma so as to reach the blood-corpuscles. The plasma will absorb, at 35° C., about 2 volumes per cent., if we take the coefficient absorption of the plasma as equal to that of distilled water. Many of the blood corpuscles of the pulmonary blood have just returned from the tissues with their hæmoglobin in the reduced state, and the latter at once withdraws oxygen from the plasma. In an instant more oxygen passes out of the pulmonary air into the plasma, from which the oxygen is again quickly withdrawn by the hæmoglobin of the corpuscles, and so on. It is interesting to note that, if the oxygen did not exist in loose chemical combination, it would only be absorbed, and its amount would depend on the barometrical pressure at the moment, and would follow each fluctuation of pressure through a range, say, of one-fourteenth of the total pressure. Such an arrangement could not fail in affecting health. If, on ascending a high mountain, say 15,000 to 20,000 feet above the level of the sea, the pressure sank to nearly one-half, the blood would then contain only half its normal quantity of oxygen, and disturbances in the functions of the body would be inevitable. High-flying birds, soaring in regions of the air where the pressure falls below half an atmosphere, would suffer from want of oxygen; but in deep mines and on high mountains men and animals live in a state of health, and the quick-breathing bird has a sufficient amount of oxygen for its marvellous expenditure of energy, because the amount of oxygen in the blood is independent of the factor which exercises an immediate influence on the gas contents of the fluid—namely, the partial pressure. Kempner has also proved that so soon as the amount of oxygen in the respiratory air sinks only a few per cent. below the normal, the consumption of oxygen by the tissues and the formation of carbonic acid also fall in consequence of the processes of oxidation in the body becoming less active.

It is a remarkable fact that, in certain circumstances, tissues and even organs may continue their functions with little or no oxygen. Thus, as quoted, Max Marckwald, in his work on the "Innervation of Respiration in the Rabbit" (translated by T. A. Haig, with introduction by Dr. McKendrick; Blackie and Son, 1888): "Kronecker and MacGuire found that the heart of the frog pulsates just as powerfully with blood deprived of its gases as with that containing oxygen, while the blood of asphyxia, or blood containing reduced hæmoglobin, soon stops its action."

Further, Kronecker has found that dogs bear the substitution of two-thirds to even three-fourths of their blood by 0·6 per cent. solution of common salt, and Von Ott withdrew 14/15 of the blood of a dog, and replaced the same with serum from the horse, free from corpuscles. For the first day or two after the transfusion the dog had only 1/55 part of the normal number of red blood corpuscles, so that it had only 1/55 part of its normal amount of oxygen. But this dog showed no symptoms except weakness and somnolency, nor did it suffer from distress of breathing, a remarkable fact when we consider that the blood of an asphyxiated dog still contains 3 per cent. of oxygen, and that it may show great distress of breathing when there is still one-sixth part of the normal amount of oxygen in its blood.

The conditions regulating the exchange of carbonic acid are quite different. We have seen that the carbonic acid is almost exclusively contained in the blood plasma, the smaller part being simply absorbed, and the greater part chemically bound, a portion existing in a fairly firm combination with a sodic carbonate of the plasma, and another portion in a loose, easily decomposable combination with the acid sodium carbonate, and a third portion with the sodium phosphate. Carbonic acid is contained in air only in traces, and its tension in the air is almost nothing. The air contained in the lungs is not wholly expelled by each respiration, but a part of the air of expiration, rich in carbonic acid, always remains in the lung. It is evident, then, that by the mixing of the air of inspiration with the air in the alveoli, the latter will become richer in oxygen and poorer in carbonic acid. The air in the alveoli, however, will always contain more carbonic acid than atmospheric air. Pflüger and Wolffberg have found the amount of carbonic acid in alveolar air to be about 3·5 volumes per cent., therefore its tension will be $\frac{3 \cdot 5 \times 760}{100} = 27$ mm. of mercury. The tension of the carbonic acid in the blood of the right ventricle (which may be taken as representing venous pulmonary blood) amounts, according to Strassburg, to 5·4 per cent. = 41 mm. of mercury, and is 14 mm. higher than that in the alveoli. Carbonic acid will, therefore, pass by diffusion from the blood into the alveolar air until the tension of the carbonic acid has become the same in the blood and in alveolar air. Before the state of equilibrium is reached, expiration begins and removes a part of the air out of the alveoli, so that the tension of the carbonic acid again becomes less than that in the blood. During the expiration and the following pause, the elimination of carbonic acid continues. This physical arrangement has the advantage for diffusion, that by expiration the whole air is not driven out of the lungs, for, if expiration had emptied the lungs of air, the diffusion would have ceased altogether during expiration and the following pause, and diffusion have been possible only during inspiration. There would thus have been an incomplete separation of the carbonic acid from the pulmonary blood. But as air remains in the lungs, the stream of diffusion between pulmonary blood and pulmonary air goes on steadily, and fluctuations occur only in regard to its velocity (Munk).

Any account of the gaseous constituents of the blood would be incomplete without a reference to the ingenious theory recently advanced by Prof. Ernst Fleischl von Marxow, of Vienna, and explained and illustrated in his work "Die Bedeutung des Herzschlages für die Athmung; Eine Neue Theorie des Respiration," a work distinguished alike by the power of applying a profound knowledge of physics to physiological problems, and by a keen and subtle dialectic. The author starts with the antagonistic statements that of all animal substances, hæmoglobin is the one which possesses the greatest affinity for oxygen, *or* that substances exist in the animal body which, at least occasionally, have a greater chemical affinity for oxygen than hæmoglobin possesses. If the tissues have a greater affinity for oxygen than hæmoglobin has, how is it that in the blood of animals that have died of asphyxia there is still a considerable quantity, in some cases as much as 5 volumes per 100 volumes, of oxygen? It is well known that the blood of such animals invariably shows the spectrum of oxyhæmoglobin. The tissues, then, do not use up all the oxygen of the oxyhæmoglobin, and they cannot, therefore, have a stronger affinity for the oxygen than hæmoglobin has. On the other hand, as the tissues undoubtedly seize hold of the oxygen, and rob the hæmoglobin of it, it would appear as if they really had a stronger affinity for the oxygen. There is thus a contradiction according to Fleischl von Marxow, and it shows that our theories as to the ultimate chemical changes of respiration are not valid.

It might be objected at this point that the death of an animal from asphyxia, while oxygen still remains in its blood, is no proof that the tissues have lost their power of removing oxygen from oxyhæmoglobin. It only indicates that certain tissues, probably those of the nervous centres, require more oxygen than is supplied to them; and, therefore, this part of the bodily mechanism is arrested, with the result of somatic death. Other tissues still live, and use up oxygen so long as their vitality lasts. At the same time, I am willing to admit that it is a striking circumstance that the nervous tissues stop working before they have exhausted every atom of oxygen in the blood.

But if tissues have, as all admit, an affinity for oxygen, and if, at the same time we grant, for the sake of argument, that this affinity is not strong enough to dissociate the oxygen from the oxyhæmoglobin, can we perceive any physical action which would, in the first place, perform the work of dissociation, and then present the oxygen to the tissues in a form in which they would readily take it up? Ernst Fleischl von Marxow holds that he has discovered such an action or agency in the stroke of the heart. He founds his theory on some remarkable experiments, which may be readily repeated with an ordinary tight-fitting hypodermic syringe. (1) Immerse the syringe wholly in water, so as to exclude air. Place one finger over the nozzle, draw up the piston for about half the length of the syringe, and then suddenly remove the finger from the nozzle. The water will rush in, and gas will be given off in considerable amount, the water being quite frothy for a short time. This is what one would expect. (2) Then carefully empty the syringe of air and gently draw it half full of water; then place the finger on the nozzle and draw the piston up a little, so as to leave a vacuum above the water. In these circumstances a few large bubbles of gas will come off, but the water will not froth. (3) Empty the syringe thoroughly, fill it half full of water, raise it obliquely so that the knob at the end of the handle of the piston is above the water, strike the knob sharply with a piece of wood, using the latter as a mallet; then draw the piston up a little, so as to leave a vacuum above the fluid. You will now observe that so large an amount of gas is given off as to cause the fluid to froth. In this experiment, the percussion stroke has evidently altered the mode in which the gas escapes when a vacuum has been formed above it. These experiments may also be done by using a long barometer tube, with a stop-cock at one end, and an india-rubber tube communicating with a movable mercury cistern (a bulb) at the other. By lowering and depressing the bulb, a Torricellian vacuum may be formed, and water may be admitted, as with the syringe. Of the effects of percussion, in these circumstances, there can be no doubt, and the experiments are extremely interesting from the physical point of view. Fleischl von Marxow holds that when gases are dissolved in fluids the condition is analogous to the solution of crystalloids. If a fluid containing gas is shaken, more especially by a sudden sharp stroke, the close connection between the molecules of the fluid and of the gas is rent asunder, and the gas molecules lie outside and between the molecules of fluid. A shock, therefore, converts a real solution into a solution in which the fluid and gaseous molecules are in juxtaposition; and, if a vacuum is formed soon after the stroke, small bubbles of gas make their appearance more readily than if a stroke had not been given.

He then applies this theory to the phenomena of the circulation and of respiration. Starting with the query why the stroke of the heart should be so sudden and violent, when a much slower and more prolonged rhythmic movement would have been sufficient to keep up the tension in the arterial system on which the movement of the fluid depends, he boldly advances the opinion that it serves for the separation of the gases. The blood is kept in motion by a series of quick, sudden strokes, because, for the taking up of the oxygen by the tissues, and the elimination of carbonic acid by the lungs, it is not sufficient that the blood runs steadily through the systemic and pulmonary circulations; and, therefore, a short, hard stroke is given to it immediately before it enters the lungs and immediately after it has left the lungs. These strokes liberate the gases from a state of solution, and they become mixed with the fluid in a state of fine dispersion. This condition of fine dispersion is favourable for the elimination of the carbonic acid by the lungs, and for the using up of oxygen by the tissues.

Fleischl von Marxow then proceeds to state that loose chemical combinations may also be dissolved by shocks, the gas passing into a condition of fine molecular dispersion, and that a quick repetition of the shocks prevents a recombination. As examples of such loose combinations, he cites oxyhæmoglobin and the compounds of carbonic acid with the salts of the plasma. It is here, in my opinion, that the theory fails, from want of experimental evidence. There is no proof that shocks, such as those of the contraction of the right and left ventricles, can liberate gases from loose chemical combinations such as those with which we have to deal, and it is somewhat strained to point to the explosion of certain compounds excited by strong mechanical shocks or by vibratory impulses.

Some of the applications of the theory are very striking. For example, Fleischl von Marxow suggests that asphyxia occurs before the oxygen has disappeared from the blood, because it is held by the hæmoglobin so firmly that the tissues cannot obtain it. Thus suppose no oxygen is admitted by respiration. It is well known that all the blood in the body passes through the heart and lungs in the time of one complete circulation—that is, in about twenty seconds; and we have it on the authority of Pflüger that in this time one-third of the oxygen is used up by the tissues. According to the percussion theory, the stroke of the left ventricle arterializes the blood—that is, liberates the oxygen from the hæmoglobin—and this arterialized blood is carried to the tissues. The hæmoglobin does not get sufficient time to recombine with the oxygen, because of the successive strokes of the heart and the vibrating thrill kept up in the arterial ramifications. The free oxygen is used up by the tissues in the capillary circulation, to the extent of one-third. After leaving the capillaries, the two-thirds of oxygen again recombine with the hæmoglobin, and in this condition return to the heart, along with one-third of hæmoglobin that has lost its oxygen. In ordinary circumstances this one-third would again obtain oxygen from the alveoli of the lungs; but if all the oxygen there has been used up, of course it cannot obtain any oxygen. The blood flows from the lungs to the left ventricle, when it is again arterialized, and again sent out through the arteries; but as there is now a large amount of free hæmoglobin present in the capillary circulation, it will seize hold of a part of the oxygen, and the tissues will obtain less than the usual supply. With each successive circulation, the amount of oxygen available for the tissues will become less and less, until the tissues receive none, because all the oxygen set free by each beat of the left ventricle is seized hold of in the capillary circulation by the reduced hæmoglobin. The tissues die from want of oxygen, because there is too much reduced hæmoglobin present, a substance having a greater affinity for oxygen than the tissues possess, a result that would probably occur, as in drowning, in the time of six or eight complete circulations—that is, in three or four minutes.

Time will not allow me to refer further to this ingenious theory, which still requires the proof that such shocks as those of the heart can liberate gases from the compounds that exist in the blood. In my opinion, Fleischel von Marxow exaggerates the importance of the shock, while he under-estimates the evidence of the spectroscope, which always shows the spectrum of oxyhæmoglobin even in arterial blood drawn from the neighbourhood of the heart, and kept from contact with the air. Nor can I accept his statement that the force of the stroke of the heart is practically the same in all classes of warm-blooded animals, and one can hardly imagine the feeble stroke of the left ventricle of a mouse would be sufficient to liberate the oxygen from the oxyhæmoglobin of its blood. Further, it may be urged that the conditions of the experiments with the syringe are very unlike those of the circulation, more especially in the fact that the walls of the syringe are rigid, while those of the heart and vessels are yielding and elastic. Again, when an organ is supplied with a solution of oxyhæmoglobin from a pressure bottle, by a process of transfusion, the tissues will reduce the oxyhæmoglobin, and take up the oxygen without any kind of percussion action being brought into play.

Physiologists, however, cannot but treat with the greatest respect the experiments and reasoning of a physicist so able as Fleischel von Marxow is known to be, and the theory will be thoroughly tested in every detail. I may be allowed to contribute an expression of deep interest in this brilliant speculation, and to say that I entirely agree with its author in accepting the suggestions of teleology in the investigations of such problems.

While the rigid investigation of facts is no doubt one of the great methods of science, we must not forget that by asking questions as to the use or value of a particular physiological arrangement, we may obtain light as to the road along which investigations are to be pursued. This is the guiding star of Fleischl von Marxow's speculation, and it has led him and other physiologists to scrutinize anew the theories of respiration now in vogue.

In this address we have had abundant evidence of the fact that physiology, in the solution of some of her problems, depends entirely upon the methods of chemistry and physics. The air-pump, the special advantages of the mercurial air-pump, the methods devised for collecting and analyzing the gases of the blood, the spectroscope, have all contributed important facts to our knowledge of respiration. The narrative placed before you also illustrates in a striking manner the relation of modern physiology to the physiology of our forefathers. The latter were engaged in observing and explaining the more obvious phenomena, whilst the modern physiologists are pushing their researches further, and are endeavouring to study the hidden phenomena, which, like a second order, lie behind these. I need scarcely add that even the results of modern research are not to be regarded as final. Although we see a little further and more clearly than those who went before, there is still uncertainty as to fact and obscurity as to explanation in most departments of physiological science, and not least as regards the function of respiration. Enough has been said to show that in the study of respiratory mechanisms we meet with numerous examples of the same wonderful adaptation of organic structure to physical conditions as may be traced in the mechanism of the eye and of the ear. The structure of a lung or of a gill is just as much adapted for the play of the physical laws regulating gases as the retina is tuned to the vibrations of the ether, or as the organ of Corti responds sympathetically to the waves of musical tone.

List of Experiments in illustration of the Lecture.

1. Appearance of blood after having been shaken with carbonic acid.
2. Appearance of blood after having been shaken with hydrogen.
3. Appearance of blood after having been shaken with nitrogen.
4. Appearance of blood after having been shaken with oxygen.
5. *Fac-simile* model of Leeuwenhoek's syringe, by which gases were first demonstrated in the blood.
6. Absorption of ammonia by water.
7. Gases escaping from water in Torricellian vacuum.
8. Gases escaping from blood in Torricellian vacuum.
9. Spectrum of oxyhæmoglobin shown by electric light.
10. Spectrum of reduced hæmoglobin; the reduction effected by ammonium sulphide.
11. Spectrum of oxyhæmoglobin changing into that of reduced hæmoglobin by heating blood *in vacuo*.
12. Demonstration of a new gas-pump for the physiological lecture table (Figs. 1, 2, and 3).
13. Demonstration of the use of Pflüger's gas-pump.
14. Collection of blood-gases and demonstration of the existence of carbonic acid and of oxygen.
15. Carbonic acid collected from a solution of carbonate of soda *in vacuo*.
16. Method, by use of thermo-electric piles with galvanometer, of observing thermal changes attending formation of oxyhæmoglobin.
17. Demonstration of Fleischl von Marxow's experiments, not with a syringe, but with the fluid in a Torricellian vacuum so arranged as to receive a shock.

Dr. McKendrick asks us to direct the attention of our readers to a statement in his address which he wishes to correct. He stated: "If the union of oxygen with the colouring matter is an example of oxidation, it must be attended with the evolution of heat, but, so far as I know, this has not been measured." He then referred to a method by which Mr. J. T. Bottomley and he had been able to observe the heat produced. Dr. McKendrick was not then aware of an important research on this subject conducted in 1871 by his friend Dr. Arthur Gamgee, and contained in a Report to the British Association for the Advancement of Science in 1871. Dr. Gamgee, both by the use of thermometers and by thermo-electric arrangements, demonstrated the important fact that an evolution of heat accompanies the union of oxygen with hæmoglobin, and in the Report referred to there is ample evidence that the research was conducted with great skill and with an appreciation of the difficulties to be surmounted. He arrived at the conclusion "that the mean rise of temperature during the absorption of oxygen amounted to 0°·0976 C. The maximum heating found was 0°·111 C., and the minimum 0°·083 C."

MOLECULAR PHYSICS: AN ATTEMPT AT A COMPREHENSIVE DYNAMICAL TREATMENT OF PHYSICAL AND CHEMICAL FORCES.[1]

I.

THE author states that his attention was drawn to the subject in the first place by personal intercourse with Sir William Thomson, and by his opening address to the Mathematical and Physical Section of the British Association at the Montreal meeting in 1884, followed by the study of the lithographed report of his lectures on "Molecular Dynamics" at the Johns Hopkins University.

The opening paragraph of the paper contains a restatement of the portions of Thomson's theory applicable to the explanation of optical phenomena. Thomson did not succeed in arriving at a satisfactory explanation of the fact that metallic reflection and double refraction are accompanied by little or no dispersion. The author believes that he has overcome this difficulty by a more complete discussion of the formulæ by expansion in series. He then proceeds to apply the theory to the explanation of chemical phenomena on a purely dynamical basis, and arrives at a method of determining the spectrum of a compound from the spectra of its constituents.

The second portion of the paper is quite independent of the first, and also of Thomson's theories, except that it gives a complete explanation of the manner in which the ether vibrations can be taken up by the molecules of a body.

The author endeavours to explain electrical phenomena by transverse vibrations of the ether, which are very small compared to the diameter of a molecule or of an atom, and one of the most remarkable and interesting results of his investigation is that the theory leads to Weber's law expressing the mutual action of two electric currents, subject to a restriction which excludes exactly those cases the consideration of which led Helmholtz to the conclusion that the law was untenable. A further confirmation of the theory is given by its explanation of a number of other phenomena, such as fluorescence, magnetism, and diamagnetism, and the electro-magnetic rotation of the plane of polarization.

PART I.—LIGHT, HEAT, AND CHEMICAL AFFINITY.

§ 1.—*The Internal Structure of Molecules.*[2]

The ether is assumed to fill the whole of space, and to be everywhere of equal elasticity and density. It is further assumed that, with respect to vibrations of periods comparable with those of light-waves, the ether behaves like a perfectly elastic solid; while with respect to slower vibrations, such as those due to the motion of gaseous molecules, it behaves like a perfect fluid, so that the molecules can traverse it freely.

A molecule is supposed, on Thomson's[3] theory, to consist of a solid core inclosed within a series of spherical shells. Between the core and the innermost shell there is supposed to be an elastic action of a nature which might be represented by a series of symmetrically disposed elastic springs.

A similar elastic action is supposed to take place between every pair of adjacent shells, and also between the outermost shell and the external ether.

Let j be the number of shells in a molecule, and let their masses, beginning with the outermost one, be

$$\frac{M_1}{4\pi^2}, \frac{M_2}{4\pi^2}, \ldots \frac{M_j}{4\pi^2}.$$

The centres of the core and shells may be supposed to lie in a straight line and to be capable of oscillations along this line. The elastic force between each pair of shells is assumed to be proportional to the relative displacement of their centres; and that between the outermost shell and the external ether, proportional

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[1] A Paper read before the Physico-Economic Society of Königsberg, by Prof. F. Lindemann, on April 5, 1888.

[2] The author generally uses the term molecule to denote either an atom or a molecule except when he is considering chemical compounds.—G. W. T.

[3] "Lectures on Molecular Dynamics and the Wave Theory of Light," by Sir William Thomson. (Baltimore, 1884.)

to the displacement of the centre relatively to the external ether. Let $x_1, x_2, \ldots x_j$, be the absolute displacement of the j shells, and ξ the displacement of the ether; and let $c_1, c_2, \ldots c_j$, be the magnitudes of the elastic forces. We then have the following equations:—

$$\frac{M_1}{4\pi^2}\frac{d^2x_1}{dt^2} = c_1(\xi - x_1) - c_2(x_1 - x_2),$$
$$\ldots (1)$$
$$\frac{M_2}{4\pi^2}\frac{d^2x_2}{dt^2} = c_2(x_1 - x_2) - c_3(x_2 - x_3),$$
$$\ldots\ldots\ldots\ldots\ldots\ldots$$

Let the point ξ have a periodic motion given by

$$\xi = a\cos\frac{2\pi t}{T} \quad \ldots\ldots\ldots (2)$$

Then this motion will gradually be communicated to the centres of the shells in a manner which has been fully worked out by Thomson. The value of T will vary, and after a certain interval a steady condition will be arrived at in which all the points will have periodic motions, so that

$$x_i = a_i \cos\frac{2\pi t}{T} \quad \ldots\ldots\ldots (3)$$

where T is now arbitrary.

Writing $a_1 = M_i/T^2 - c_i - c_{i+1}$, equations (1) give

$$-\frac{c_1\xi}{x_1} = a_1 - \cfrac{c_2^2}{a_2 - \cfrac{c_3^2}{a_3 - \cdots - \cfrac{c_{j-1}^2}{a_{j-1} - \cfrac{c_j^2}{a_j}}}},$$

which may be written in the form—

$$-\frac{x_1}{c_1\xi} = \frac{T^2}{m_1}\left\{\frac{K_1^2R_1}{K_1^2 - T^2} + \frac{K_2^2R_2}{K_2^2 - T^2} + \ldots + \frac{K_j^2R_j}{K_j^2 - T^2}\right\} \quad . (4)$$

The constant R_1 represents the ratio of the energy of the shell m_i to the total energy of the system. The quantity K_i is determined by the condition that when $T = K_i$ the ether remains at rest, or $\xi = 0$; and it may be called a critical period of the molecule, which will accordingly have j critical periods, and the molecule may undergo vibrations corresponding to any or all of them simultaneously without affecting the external ether.

Instead of this somewhat artificial structure, the molecule may be regarded as consisting of a sphere filled with continuous matter of density varying with the radius, the density having different values for each of j assigned values of the radius, but though this would be a simpler physical representation, it would lead to great difficulties in the mathematical treatment, though the results would necessarily be of a similar nature to those obtained for the discrete molecule, and it is therefore preferable to retain this representation.

To apply the theory to transparent media let $M_i/4\pi^2$ represent the thickness instead of the mass of a shell, and let $\rho/4\pi^2$ and $l/4\pi^2$ be the density and elasticity respectively of the ether.

The vibrations of the ether will then be given by the equation

$$\rho\frac{d^2\xi}{dt^2} = l\frac{d^2\xi}{dx^2} \quad \ldots\ldots\ldots (5)$$

And the vibrations of the outermost shell will, in virtue of the assumptions which have been made, be connected with those of the neighbouring ether particle ξ by an equation of the form

$$\rho\frac{d^2\xi}{dt^2} = l\frac{d^2\xi}{dx^2} + c_1(x_1 - \xi)4\pi^2 \quad \ldots\ldots (6)$$

in which c_1 only differs from its former value by an unimportant factor. The axis of x is here supposed to be perpendicular to the line of centres, or diameter, of the molecule.

Suppose a light-wave in a direction perpendicular to this axis, and given by the equation

$$\xi = a\sin 2\pi\left(\frac{x}{\lambda} - \frac{t}{T}\right) \quad \ldots\ldots (7)$$

to strike the molecule; then on the assumption that within a definite interval only one wave strikes the molecule, or that the diameter of the molecule is small in comparison with the wave-length, where μ is the index of refraction of the medium, and v the velocity of the wave in it, equation (6) gives the equation

$$\mu^2 = \frac{1}{v^2} = \frac{T^2}{\lambda^2} = \frac{1}{l}\left\{\rho - c_1T^2\left(l - \frac{x_1}{\xi}\right)\right\}$$
$$= \frac{1}{l}\left\{\rho - c_1T^2\left[1 + c_1\frac{T^2}{m_1}\sum\frac{K_i^2R_i}{K_i^2 - T^2}\right]\right\} \quad . \quad . (8)$$

expressing the index of refraction as a function of the period of vibration of the ray. For waves of period equal to one of the critical periods of the molecule, μ becomes infinite, so that the medium is opaque for such waves, which are entirely absorbed in increasing the energy of the internal vibrations of the molecules. The critical periods of the molecule are therefore the vibration-periods of the dark lines of its absorption spectrum.

§ 2.—*The Index of Refraction as a Function of the Wave-Length.*

As a preliminary to the more general investigation, it will be advisable to trace the dependence of the index of refraction upon the period of vibration in the simple cases $j = 1$ and $j = 2$.

For $j = 1$ the molecule will consist of a core and a single shell, and equation (8) will reduce to

$$\mu^2 = \frac{\rho}{l} - \frac{c_1T^2}{l} - \frac{c_1^2T^4}{lm_1}\frac{K^2R}{K^2 - T^2} \quad \ldots (9)$$

Writing

$$\frac{\rho}{l} = a, \ -\frac{c_1}{l} = \beta, \ -\frac{c_1^2K^2R}{lm_1} = \gamma, \ T^2 = x, \ \mu^2 = y,$$

this may be written in the form

$$y(K^2 - x) = (a + \beta x)(K^2 - x) + \gamma x^2 \quad . \quad . (10)$$

the equation of a hyperbola having the asymptotes

$$x = K_1^2, y = (\beta - \gamma)x + a - \gamma K^2 \quad . \quad . \quad . (11)$$

The former represents the single critical period, and the latter practically determines by its direction whether the index of refraction increases or diminishes as T, the period of vibration, increases, and this the more exactly the more nearly the curve coincides with its asymptotes—that is, the more nearly the value of its determinant, which reduces to $-\gamma K^2/4$ approaches the value zero.

There will therefore be three cases to consider—

(*a*) $\beta - \gamma > 0$, μ increases as T increases.

(*b*) $\beta - \gamma = 0$, μ approximately constant.

(*c*) $\beta - \gamma < 0$, μ diminishes as T increases.

There will be two expansions for μ^2 in powers of T, viz.:

For $T < K$,

$$\mu^2 = a + \beta x + \frac{\gamma x^2}{K^2}\left\{1 + \frac{x}{K^2} + \frac{x^2}{K^4} + \&c.\right\}$$
$$= \frac{\rho}{l} - \frac{c_1}{l}T^2 - \frac{c_1^2R}{lm_1}T^2\left\{1 + \frac{T^2}{K^2} + \frac{T^4}{K^4} + \&c.\right\} \quad . (12)$$

For $T > K$,

$$\mu^2 = a + \beta x - \gamma x\left\{1 + \frac{K^2}{x} + \frac{K^4}{x^2} + \&c,\right\}$$
$$= \frac{\rho}{l} + \frac{c_1^2K^4R}{lm_1} - \frac{c_1}{l}\left(1 - \frac{c_1K^2R}{m_1}\right)T^2$$
$$+ \frac{c_1^2K^6R}{lm_1T}\left\{1 + \frac{K^2}{T^2} + \&c.\right\} \quad . (12a)$$

The coefficient of T^2 must be very small in order that the formulæ may be in accordance with experimental results.

Both the equations (12) and (12*a*) give, as a first approximation to the relation between wave-length and period of vibration in the medium considered—

$$T = \lambda\sqrt{\frac{\rho}{l} + \frac{c_1^2K^4R}{lm_1}} \quad \ldots (13)$$

But λ is approximately proportional to T, so that

$$\mu^2 = A + B\lambda^2 + \frac{C\lambda^4}{\lambda^2 - \lambda_0^2},$$

where λ_0 is the wave-length corresponding to the period $T = K$. This agrees with the results of Helmholtz's theory, and with experiment.[1]

For values of T not in the neighbourhood of K, the hyperbola

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[1] Wüllner's "Experimental-Physik," vol. ii. p. 161, fourth edition.

may be replaced by its non-vertical asymptote, and then it follows from (11) that

$$\mu^2 = \frac{\rho}{l} + \frac{c_1{}^2\mathrm{K}^4\mathrm{R}}{lm_1} - \frac{c_1}{l}\left(1 - \frac{c_1{}^2\mathrm{K}^2\mathrm{R}}{m_1}\right)\mathrm{T}^2 \quad . \; . \; . \; (13a)$$

the right-hand expression consisting of the first two terms of (12*a*). When $j = 2$, or the molecule consists of a core and two shells, equation (8) becomes

$$\mu^2 = \frac{\rho}{l} - \frac{c_1}{l}\mathrm{T}^2 - \frac{c_1{}^2\mathrm{T}^4}{lm_1}\left(\frac{\mathrm{K}_1{}^2\mathrm{R}_1}{\mathrm{K}_1{}^2 - \mathrm{T}^2} + \frac{\mathrm{K}_2{}^2\mathrm{R}_2}{\mathrm{K}_2{}^2 - \mathrm{T}^2}\right) . \; (14)$$

or

$$y = \alpha + \beta x + \frac{\gamma x^2}{\mathrm{K}_1{}^2 - x} + \frac{\delta x^2}{\mathrm{K}_2{}^2 - x},$$

where x, y, α, β, γ, have the same meanings as before, and $\delta = -c_1{}^2\mathrm{R}_2/lm_1$. The curve is therefore of the third order with two vertical asymptotes, $x = \mathrm{K}_1$, and $x = \mathrm{K}_2$, and a third given by the equation

$$y = \alpha - \gamma\mathrm{K}_1{}^2 - \delta\mathrm{K}_2{}^2 + (\beta - \gamma - \delta)x \quad . \; . \; . \; (15)$$

If the curve nearly coincides with its asymptotes, μ^2 will be given approximately in terms of T^2 by (15), except near the critical periods, and as before there will be three cases, viz. :—

(*a*) $\beta - \gamma - \delta > 0$, μ increases as T increases.
(*b*) $\beta - \gamma - \delta = 0$, μ approximately constant.
(*c*) $\beta - \gamma - \delta < 0$, μ diminishes as T increases.

Near the critical periods μ^2 will always diminish as T increases.

When the condition (*a*) is fulfilled, and the curve does not approximately coincide with its asymptotes, μ may continue to decrease as T increases throughout the whole branch of the curve between the two vertical asymptotes, the curve running from the upper left-hand to the lower right-hand side.

The expansions in powers of T will be different for the three branches, viz. :—

For $\mathrm{T} < \mathrm{K}_1$,

$$\mu^2 = \alpha + \beta x + x^2\left(\frac{\gamma}{\mathrm{K}_1{}^2} + \frac{\delta}{\mathrm{K}_2{}^2}\right) + x^3\left(\frac{\gamma}{\mathrm{K}_1{}^4} + \frac{\delta}{\mathrm{K}_2{}^4}\right) + \&\text{c.} \quad . \; . \; (16)$$

For $\mathrm{T} > \mathrm{K}_2$,

$$\mu^2 = \alpha - \gamma\mathrm{K}_1{}^2 - \delta\mathrm{K}_2{}^2 + (\beta - \gamma - \delta)\,x - \frac{1}{x}(\gamma\mathrm{K}_1{}^4 + \delta\mathrm{K}_2{}^4)$$
$$- \frac{1}{x^2}(\gamma\mathrm{K}_1{}^6 + \delta\mathrm{K}_2{}^6) + \&\text{c.} \; . \; . \; . \; . \; . \; (16a)$$

For $\mathrm{K}_1 < \mathrm{T} < \mathrm{K}_2$,

$$\mu^2 = \alpha - \gamma\mathrm{K}_1{}^2 + (\beta - \gamma)\,x - \frac{\gamma\mathrm{K}_2{}^4}{x} + \frac{\delta x^2}{\mathrm{K}_1{}^2} - \frac{\gamma\mathrm{K}_1{}^6}{x^2}$$
$$+ \frac{\delta x^3}{\mathrm{K}_1{}^4} - \frac{\gamma\mathrm{K}_1{}^8}{x^3} + \&\text{c.} \quad . \; . \; . \; . \; . \; (16b)$$

The first terms of (16*a*) are identical with the right-hand side of (15), and therefore if the curve nearly coincides with its asymptotes, it will closely approximate to the curve (14), except near the critical periods. This explains why Cauchy's expansion of μ^2 in descending powers of T, or of λ, gives approximately correct results. In this expansion the coefficient of T^2 vanishes if the asymptote is parallel to the axis of x, viz. if $\beta = \gamma + \delta$, or if

$$m_1 = c_1(\mathrm{K}_1{}^2\mathrm{R}_1 + \mathrm{K}_2{}^2\mathrm{R}_2) \; . \; . \; . \; . \; . \; . \; (17)$$

If $\delta = 0$ it reduces to the preceding case; the curve breaking up into the asymptote $x = \mathrm{K}_2{}^2$, and a hyperbola. If $\gamma = 0$ it breaks up into the asymptote $x = \mathrm{K}_1{}^2$ and a hyperbola.

In general, with a greater number of critical periods, if the curve is of the order n, it will have $n - 1$ vertical, and one other asymptote. To the left of the first vertical asymptote and to the right of the last there will be a hyperbolic branch, and between every two of them will be a branch of the curve proceeding from the upper left-hand to the lower right-hand side, either falling continuously or reaching a minimum, then rising to a maximum, and again falling and approaching the next asymptote. There will be n distinct expansions for μ^2 in powers of T^2, one for each branch of the curve. In many cases the curve, except near the critical periods, will approximately coincide with its non-vertical asymptote, and there will then be the three cases, (*a*), (*b*), (*c*), to consider, as in the previous examples.

§ 3.—*Dispersion and Reflection.*

It is well known that the spectrum of light of a given kind depends on the function of T^2 serving to express μ^2. The dispersion in a refracting medium will be designated as normal when, except near the critical periods, μ^2 diminishes without limit as T^2 increases, and anomalous when μ^2 increases without limit, or passes through a series of maxima and minima. In the first case the colours of the spectrum will appear in their "natural" order, the smaller values of T^2 corresponding to the blue, and the larger values to the red end of the spectrum. In the examples considered in § 2 the dispersion will accordingly be normal in case (*c*), and anomalous in case (*d*), while in case (*b*) the spectrum will be compressed into a line.

When the dispersion is anomalous throughout, the colours will appear in the inverse of the natural order, but it will be otherwise when it is alternately normal and anomalous.

Consider, for example, the non-vertical asymptote in case (*c*). Then if there are only two critical periods there will be to the left of the asymptote $x = \mathrm{K}_1{}^2$, a hyperbolic branch, along which μ^2 will decrease continuously, giving normal dispersion at the blue end of the spectrum above the axis of x. Below this axis μ^2 will be negative, and therefore μ will be imaginary, so that light of the corresponding period will be entirely reflected by the medium. From the point of intersection of the branch of the curve with the axis of x to the point $x = \mathrm{K}_1{}^2$ there will therefore be a dark space or absorption band. To the right of this point μ^2 will again decrease from positive infinity to a minimum.

Suppose this to be at a position for which $x = p$ above the axis of x, the curve will then rise to a maximum, say for $x = q$. For $p < \mathrm{T}^2 < q$ the light will then be more strongly refracted than for $\mathrm{T}^2 < p$, and therefore the corresponding colours will be displaced, and may overlap the colours for which $\mathrm{T}^2 < p$. There will therefore be a dark band at the part of the spectrum which should be occupied by them, but this is not now an absorption band, and may be made to disappear by further dispersion. For $\mathrm{T}^2 < q$ the dispersion will be normal up to the intersection of the branch with the axis of x, from which a dark band will extend to the point $x = \mathrm{K}_2{}^2$, after which the dispersion will again become normal.

Phenomena of this kind have been observed by Kundt and others, and the fact that they follow from the formulæ was considered by Thomson to afford important confirmation of the theory. In fact, taking T proportional to λ, the preceding equations do not differ essentially from those obtained from quite different phenomena by Sellmayer, von Helmholtz, Lommel, and Ketteler, and which have been shown to be in complete accordance with experiment.[1]

Sir William Thomson, in his Baltimore lectures, came to the conclusion that according to his theory metallic reflection would necessarily cause dispersion. This would be the case if there were only a single expansion for μ^2, but in the case of most of the metals there are so many lines, distributed over the whole spectrum, that there is no reason for selecting any one in preference to the others. The fact that all the colours are reflected to practically the same extent, which means that μ^2 must be a negative constant, may be completely explained by the assumptions that the corresponding curve of the nth order approximates very closely to its n asymptotes, and that the single non-vertical asymptote is very nearly parallel to the axis $\mu = 0$. The essential portion of the curve may then be replaced by its horizontal asymptote, as in the cases previously considered, in which $\beta - \gamma$ and $\beta - \gamma - \delta$ respectively were assumed to be nearly zero. The non-existence of dispersion does not therefore afford an objection to the theory.

It is easy to see that by a suitable choice of the disposable constants, the curve may be made to practically coincide with its asymptotes, for consider the curve of the third order given by (14). This may be written in the form

$$(\mathrm{K}_1{}^2 - x)(\mathrm{K}_2{}^2 - x)(y - \alpha - \beta x) = \gamma x^2(\mathrm{K}_2{}^2 - x) + \delta x^2(\mathrm{K}_1{}^2 - x);$$

or

$$(\mathrm{K}_1{}^2 - x)(\mathrm{K}_2{}^2 - x)(y - \alpha - \beta x + \gamma\mathrm{K}_1{}^2 + \delta\mathrm{K}_2{}^2 + \gamma x + \delta x)$$
$$= x^3(\gamma + \delta - \gamma\mathrm{K}_1{}^2 - \delta\mathrm{K}_2{}^2) - x(\gamma\mathrm{K}_1{}^4 + \delta\mathrm{K}_2{}^4)$$
$$+ \mathrm{K}_1{}^2\mathrm{K}_2{}^2(\gamma\mathrm{K}_1{}^2 + \delta\mathrm{K}_2{}^2),$$

and it is evident that when $\mathrm{K}_1{}^2$ and $\mathrm{K}_2{}^2$ are given, the right-hand member may be made to vanish by taking γ and δ small enough, and the required condition will then be fulfilled, since the left-hand member equated to zero represents the three asymptotes.

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[1] See Wüllner, "Experimental-Physik," vol. ii. pp. 106 and 169, fourth edition. An outline of the various theories of reflection and refraction will be found in the British Association Reports for 1885 and 1887.

§ 4.—*Spectra of Luminous Gases.*

It was first shown by Kirchhoff that glowing gases emit light of the same wave-length, and therefore also of the same period, as that which they absorb.

In the modern theory of gases it is assumed that the molecules of a luminous gas move over a certain distance, the length of the "free path," in straight lines, until they collide with other molecules, or with the sides of the containing vessel, when they move off rectilinearly in another direction.

At every collision the molecule is subjected to an elastic impulse in a direction passing through its centre, causing internal elastic vibrations. The periods of these vibrations could, on the analogy of a corresponding problem in the theory of elasticity, be calculated from a transcendental equation, if the interior of the molecule were uniformly filled with matter; according to Thomson's theory of molecular structure they are determined *a priori*, being the critical periods of the molecule. In fact, during the collisions the external shells only are in contact, but the surrounding ether remains unaffected, and therefore the external vibrations must be of such a nature that $\xi = 0$ (§ 1), which is the condition determining the critical periods. But according to § 1 these periods determine the wave-length of the light absorbed. Thus Kirchhoff's law is a consequence of the theory.

It has hitherto been assumed that the vibrations in a molecule, arising from the collisions, take place along a fixed diameter, and therefore that the vibrations due to one encounter are not disturbed by a later one in another direction. If the temperature or the density of the gas is so great that the encounters follow one another very rapidly, the investigation of § 1 is no longer applicable, and light-waves of other than the critical periods will be emitted. If a second encounter takes place only after the vibration due to the first has nearly subsided, the period of the emitted light will only differ slightly from a critical period. As the density and temperature increase, the bright lines will therefore gradually increase in width.[1] If a molecule receives impulses in different directions in rapid succession, very few of the vibrations will have the critical periods, and therefore the dark spaces between the bright lines will ultimately disappear, and the spectrum become continuous, as is well known to be experimentally true.

§ 5.—*Applications to the Theory of Heat.*

It will be of interest to see what explanation Thomson's molecular hypothesis can give of the manner in which the velocity of gaseous molecules can be increased by the action of heat, as has been assumed in what precedes.

The energy due to the internal molecular vibrations cannot possibly exceed a definite maximum value, for the amplitudes and therefore the velocities of the centres of the shells must have fixed upper limits, since the shells must remain one within the other. This maximum may be attained either for vibrations of a single critical period, or of all the critical periods. Suppose this maximum value to have been nearly reached, then any further disturbance of the internal equilibrium, tending to increase the amplitude of motion of one of the centres beyond the maximum value possible while the centre of gravity remains fixed, will necessarily displace the centre of gravity, whether the disturbance be due to a wave of light or to a mechanical impulse.

This leads to the general and fundamental proposition that "A molecule will begin to move as soon as the energy of its internal vibrations has attained its maximum value, supposing the external influences to which the attainment of the maximum is due continue to act.[2]

The internal equilibrium of a molecule may be disturbed either by light or heat, the disturbance in the case of light being due to its action on the critical periods of the molecule. A medium will therefore be heated when traversed by light-rays; the rays of the critical periods set the molecular shells in vibration, and when the internal energy has reached its maximum value, the centres of gravity of the molecules will begin to move, and this motion will be perceived as heat.

The energy of internal motions therefore accounts for a portion of the internal work of the mechanical theory of heat.[1]

The external work is effected by the motion of the centres of gravity of the atoms, and this takes place in different and known ways in solid, liquid, and gaseous bodies. Heat may act on a medium either by radiation or conduction. Radiant heat differs from light only in its action on our senses, so that what has been said about light will apply also to radiant heat. In the case of conduction of heat the process is exactly the reverse. The external work of the medium emitting the heat will be transmitted directly to the medium receiving it by contact—that is, by collisions of molecules.[2]

The disturbance of the internal equilibrium of the molecules is here merely a secondary effect, but in this case also the internal energy will gradually increase to the maximum value.[3]

The emission of light by a sufficiently heated solid is explained as in the case of gases, but the spectrum in the case of the solid is continuous.

Just as the action of heat may produce such violent molecular motion as to cause the emission of all possible kinds of light, so the action of light may produce a molecular motion giving rise to a special kind of light. This will only happen, however, when the molecule (owing to specially favourable values of the constants c_i and m_i) is specially susceptible to some among its critical periods. In this way the phenomenon of fluorescence may be explained. G. W. DE TUNZELMANN.

(*To be continued.*)

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 21.—"On the Determination of the Photometric Intensity of the Coronal Light during the Solar Eclipse of August 28–29, 1886. Preliminary Notice." By Captain W. de W. Abney, C.B., R.E., F.R.S., and T. E. Thorpe, Ph.D., F.R.S.

Attempts to measure the brightness of the corona were made by Pickering in 1870, and by Langley and Smith, independently, in 1878, with the result of showing that the amount of emitted light as observed at various eclipses, may vary within comparatively wide limits. These observations have been discussed by Harkness ("Washington Observations for 1876," Appendix III.) and they are again discussed in the present paper. Combining the observations, it appears that the total light of the corona in 1878 was 0·072 of that of a standard candle at 1 foot distance, or 3·8 times that of the full moon, or 0·0000069 of that of the sun. It further appears from the photographs that the coronal light varied inversely as the square of the distance from the sun's limb. Probably the brightest part of the corona was about 15 times brighter than the surface of the full moon, or 37,000 times fainter than the surface of the sun.

The instruments employed by the authors in the measurement of the coronal light on the occasion of the solar eclipse of August 28–29, 1886, were three in number. The first was constructed to measure the comparative brightness of the corona at different distances from the moon's limb. The second was designed to measure the total brightness of the corona, excluding as far as possible the sky effect. The third was intended to measure the brightness of the sky in the direction of the eclipsed sun. In all three methods the principle of the Bunsen photometric method was adopted, and in each the comparison-light was a small glow-

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[1] This result may be expressed by saying that the characteristic constant c_i of the molecule is a function of the temperature. It is preferable to regard the ideal spectrum, whether due to emission or absorption, as something definitely fixed; external circumstances merely assisting or hindering its formation.

[2] Sir W. Thomson also points out ("Lectures," p. 280) that a considerable increase in the internal vibrations of a molecule must set it in motion, and therefore cause a production of heat.

[1] The discrepancies occurring in the determination of the atomic weights of gases may therefore be explained by assuming that internal work is done by the motions of the atoms, instead of assuming, as would otherwise be necessary, that the internal work is only done by the motions of the molecules and a decrease in the attractive force between them. For "motion of the atoms" we should have to substitute "motion of the inner spherical shells."

[2] For the method of deducing the differential equation of heat-conduction from these considerations, see F. Neumann, "Vorlesungen über die Theorie der Elasticität," § 59.

[3] Dulong's law of atomic heat gives some information respecting the relative value of this maximum. This law states that the quantity of internal work due to heating is approximately the same, at any rate when in the gaseous state, for elementary bodies which are ordinarily solid or liquid, a given number of atoms always requiring the same quantity of heat to produce a given rise of temperature. It follows, then, that for these elements the maximum internal energy is very nearly the same. Carbon, silicon, sulphur, and phosphorus behave exceptionally in this, as in many other respects, and the law is not generally true for the elements which are ordinarily gaseous. Since the maximum value of the internal energy depends on the diameter of the molecule, as well as on the constants c_i and m_i, it may perhaps be concluded that the diameter of the molecules of these elements are approximately equal.

lamp previously standardized by a method already described by one of the authors in conjunction with General Festing. In the first two methods the photometer-screen was fixed, the intensity of the comparison-light being adjusted by one of Varley's carbon resistances; in the third the glow-lamp was maintained at a constant brightness, the position of the screen being adjusted along a graduated photometer bar, as in the ordinary Bunsen method. Full details of the construction of the several pieces of apparatus are given in the original paper.

The observations during the eclipse were made at Hog Island, a small islet at the south end of Grenada, in lat. 12° 0′ N. and long. 61° 43′ 45″ W., with the assistance of Captain Archer and Lieutenants Douglas and Bairnsfather of H.M.S. *Fantôme*. The duration of totality at the place of observation was about 230 seconds, but measurements were possible only during 160 seconds, at the expiration of which time the corona was clouded over. A careful discussion of the three sets of measurements renders it almost certain that the corona was partially obscured by haze during the last 100 seconds that it was actually visible. Selecting the observations made during the first minute, which are perfectly concordant, the authors obtain six measurements of the photometric intensity of the coronal light at varying distances from the sun's limb, from which they are able to deduce a first approximation to the law which connects the intensity of the light with the distance from the limb.

The observations with the integrating apparatus made independently by Lieutenants Douglas and Bairnsfather, agree very closely. It appears from their measurements that the total light of the corona in the 1886 eclipse was—

Douglas	0·0123	standard candle
Bairnsfather	0·0125	,,
Mean . . .	0·0124	,,

at a distance of 1 foot.

In comparing these observations with those made during the 1878 eclipse, it must be remembered that the conditions of observation on the two occasions were widely different. The observations in the West Indies were made at the sea's level, in a perfectly humid atmosphere and with the sun at no greater altitude than 19°. Prof. Langley, in 1878, observed from the summit of Pike's Peak in the Rocky Mountains at an altitude of 14,000 feet, in a relatively dry atmosphere and with the sun at an altitude of 39°.

From observations on the transmission of sunlight through the earth's atmosphere (Abney, Phil. Trans., A, clxxviii (1887), 251) one of the authors has developed the law of the extinction of light, and, by applying the necessary factors, it is found that the intensity of the light during the 1886 eclipse, as observed at Grenada, is almost exactly half of that of which would have been transmitted from a corona of the same intrinsic brightness when observed at Pike's Peak. Hence to make the observations of Prof. Langley comparable with those of the authors, the numbers denoting the photometric intensity of the corona in 1878 must be halved. The result appears, therefore, that whereas in 1878 the brightness of the corona was 0·0305 of a standard candle at a distance of 1 foot, in 1886 it was only 0·0124 of a candle at the same distance. Several of the observers of the West Indian eclipse (including one of the authors) were also present at the eclipse of 1878, and they concur in the opinion that the darkness during the 1886 eclipse was very much greater than in that of 1878. The graduations on instruments, chronometer faces, &c., which were easily read in 1878, were barely visible in 1886. In explanation of this difference in luminous intensity it must not be forgotten that the 1878 eclipse was not very far removed from a period of maximum disturbance, whereas in 1886 we were approaching a period of minimum disturbance.

PARIS.

Academy of Sciences, August 6.—M. Janssen, President, in the chair.—Fresh experiments on the fixation of nitrogen by certain vegetable soils and plants, by M. Berthelot. These researches, made with three different kinds of argillaceous soil and with plants of the leguminous family, fully confirm the results of previous studies. The fundamental fact that both plants and soil absorb nitrogen under the most diverse conditions is now placed beyond all reasonable doubt. So certain does the author consider this conclusion, that he declines all further discussion on the subject of certain recent negative experiments carried out under defective conditions.—On a recent change in the views of meteorologists regarding gyratory movements, by M. H. Faye. The author claims that the new school of meteorologists, represented by Messrs. Loomis, Meldrum, and Douglas Archibald (see NATURE, June 14, p. 149), shows a tendency to accept his conclusions on certain points at issue. These authorities already admit that the cyclonic movements originate, not on the surface of the earth as had long been contended, but in the higher atmospheric regions, a position irreconcilable with their hypothesis of an ascending, but in full accordance with M. Faye's view of a descending motion.—Summary of the solar observations made at the Royal Observatory of the Collegio Romano during the second quarter of 1888, by M. P. Tacchini. These observations show an increase of the solar spots in May and June, and of the protuberances in April. The general inference is that the relation between these two orders of phenomena is less intimate than might be supposed from previous observations.—On a new apparatus for studying the friction of fluids, by M. M. Couette. This method, differing from those of Coulomb and Poiseuille hitherto employed, is based on the principle indicated by Dr. Margules in 1881 (*Wiener Berichte*, 2nd series, vol. lxxxiii. p. 588). It has the advantage of controlling Navier's theory for very thin tubes and slow discharge, and of operating on gases at constant pressure.—On levulose, by MM. E. Jungfleisch and L. Grimbert.—On the malonates of potassa and soda, by M. G. Massol.—On the hydrates of methane and ethylene, by M. Villard.—On experimental tetanus, by M. Rietsch.—M. A. de Schulten describes a process by which he has succeeded in producing the crystallized anhydrous sulphates of cadmium and zinc (artificial zincosite); and M. A. Poincaré shows how are produced the barometric movements corresponding to the displacement of the moon in declination.—The present number contains the text of the address delivered by the President, M. Janssen, at the unveiling of the monument raised by the city of Tours to the memory of General Meusnier on July 29, 1888.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Nature and the Bible: J. Davis, 2nd edition (Houlston).—Earth Knowledge, Part 2: W. J. Harrison and H. R. Wakefield (Blackie).—The Elementary Geometry of Conics: C. Taylor, 5th edition (Bell).—The Bacon-Shakspere Question: C. Stopes (Johnson).—Curve Pictures of London: A. B. MacDowall (Low).—Great Circle Sailing: R. A. Proctor (Longmans).—Fifty Years of Economic Botany: J. W. Ellis.—Journal of the Royal Microscopical Society, August (Williams and Norgate).—Proceedings of the Liverpool Geological Society, vol. v. Part 4 (Liverpool).—Brain, Part 42 (Macmillan).—Bulletin de l'Académie Royale des Sciences de Belgique, 1888, No. 7 (Brussels).—Quarterly Journal of the Geological Society, August (Longmans).—Meteorologische Beobachtungen in Deutschland, 1886 (Hamburg).

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