THURSDAY, MARCH 22, 1877

BRITISH MANUFACTURING INDUSTRIES

British Manufacturing Industries. Edited by G. Phillips Bevan, F.G.S. "Hosiery and Lace," by the late W. Felkin, Nottingham; "Carpets," by Christopher Dresser, Ph. D.; "Dyeing and Bleaching," by T. Sims (second edition); "Pottery," by L. Arnoux, of Minton's Factory; "Glass and Silicates," by Prof. Barff, M.A.; "Furniture and Woodwork," by J. H. Pollen, M.A. (second edition). (London: Stanford, 1877.)

Industrial Classes and Industrial Statistics. By G. Phillips Bevan, F.G.S.. Vol. 1., "Mining, Metals, Chemicals, Ceramics, Glass, and Paper." Vol. 2., "Textiles and Clothing, Food, Sundry Industries." (London: Stanford, 1876-7.)

THE first edition of the first two volumes has been already noticed in these columns. The speedy issue of a second edition bears out the favourable opinion previously expressed with regard to them, and of this series of short comprehensive essays on British manufactures generally. There can be no doubt that any one wishing to take a general view of any of the subjects on which they treat will gain a good idea of their principles from a perusal of these volumes.

The latter two volumes, dealing as they do with the condition of the British industrial classes considered as *workers*, form an appropriate pendant to the "British Manufacturing Industries," in which the nature of the *work* which they perform has been already spoken of. Though, as Mr. Bevan says, the British workman "has from a political point of view . . . been frequently written and talked about—too much so, indeed, for his own good or for the good of the country—his social condition as dependent upon, or connected with, his special branch of labour," is comparatively seldom inquired into. This topic forms the subject of the present work.

In treating the subject one or more chapters are devoted to each of the divisions named above. A description of the character of the work in each department is given, together with the conditions whether healthy or otherwise, under which it is performed. This is supplemented by statistics as to the numbers employed in the various industries, the quantity of material manufactured, its value, the wages of the workman or workwoman, the effects of various industries on the rate of mortality, and some account of working-class legislation and federation. The distribution of the various industries over the country is graphically shown by two maps in each volume.

The statistics are very full and complete, and being compiled mostly from official sources, may be taken as accurate and reliable. With regard to the introduction of this profusion of figures the author remarks that he has done so "in the hope of showing, how intimately allied is the development of our manufactures with the state of wages and the general condition of the working classes." There can be no doubt that Mr. Bevan's volumes clearly show that our English operatives are in better condition generally now than at any previous period, and this is so whatever be the standpoint from which they are regarded.

In reading the descriptions of the various handicrafts and under what conditions they are executed, one sees that there is much yet to be done in some branches to render the work still easier of execution and less unhealthy. Science has done much for the coal-miner; but more remains to be done in order that he may be better protected from sudden outbursts of fire-damp or choke-damp. It is evident from the quotation of Dr. Angus Smith's analyses of the air from metal mines, that some further legislative enactment is required to compel the more thorough ventilation of such mines. Blast-furnace workmen, it is true, do not work under very unhealthy or dangerous conditions; but there are one or two points in which there is room for improvement. For example, the "chargers" more particularly, are now and again rendered insensible by breathing escaping carbonic oxide, sometimes with fatal results; while once in a while, from the fall of the material in the furnace after "scaffolding," explosions occur in the "hearth" resulting in the forcible expulsion of the front or side of the furnace and of the molten metal, which occasionally envelops the workmen, who in some instances have been literally roasted to death. Possibly at some future period the dangers to health and life arising from these causes may be diminished or entirely obviated by the application of scientific principles.

The injurious effects and diseases resulting from the vapour of ordinary phosphorus and "phosphoric fumes" in match-making are fully pointed out (and the remedy in the use of red phosphorus), also those of oxide of zinc, sparks of metal and dust, poisonous gases, powdered glass and emery, and poisonous colours, in brass, needle, chemical, glass and emery paper, and paper-hanging manufactures. In the second volume the "sizing" of cotton goods, "singeing" of fabrics generally, "heckling" of flax, preparation of tobacco, use of "Scheele's green" and similar preparations in flower-making, and the closeness and general want of ventilation in workrooms, in dress-making, &c., are shown to result prejudicially to the health of the worker. Affections of the eyes from close work, as in watch and lace making, are not lost sight of. Neither are deformities resulting from working in constrained and unnatural positions.

In this connection it may perhaps reasonably be doubted—as the result of actual experience—whether "the effects of chlorine are transient and less serious" than follow from the inhalation of sulphuretted hydrogen. Weldon's chlorine-process is noted as an improvement on the old process, while Deacon's—which may fairly rank with it—is left unnoticed. On the same page a small matter needs correction; "mangan*i*te" and "permanga-n*i*te" should, of course, be mangan*a*te and permanga-n*a*te. Mr. Bevan might be puzzled, perhaps, to show how tobacco acts as food, under which head he classes it.

A list is given of the legislative Acts hitherto passed affecting industries, which shows that the amelioration of the condition of the worker to its present improved state has not been an easy task. The trade societies are classed as trades unions, friendly societies, and co-operative societies; of the first the opinion is given that they are "at most a doubtful blessing," while the latter are considered as both useful and excellent.

The abundant statistics given cannot fail to be of great service in many ways. The volumes altogether are very readable, and throughout the statements are usually reliable. Should a second edition be required it might possibly be improved by the addition of a "table of contents." W. H. W.

THE GERM THEORY

The Germ Theory Applied to the Explanation of the Phenomena of Disease. "*The Specific Fevers.*" By T. Maclagan, M.D. (London: Macmillan and Co., 1876.)

IN his preface the author states that "one object which he has in view is to rescue the germ theory of disease from a false and misleading position, and to give to it its true and legitimate standing as a pathological question." The subject discussed is whether the propagation of germs in the system can produce specific fevers. He believes it can, and assumes that all contagia are living organisms, probably albuminous, reproducing their kind, living for a considerable period, speedily perishing when freely exposed to the atmosphere, and so minute as to elude the highest powers of the microscope.

If, however, the particles in sheep-pox, small-pox, and vaccine, be the infecting matter, they are easily seen by the microscope, and ought therefore to be found in the blood, but such is not the case.

Dr. Maclagan holds that "all microzymes are not contagia, but all contagia may be microzymes." The fact that the contagia fluids are most potent when fresh, and that their virulence diminishes as bacteria increase in them is explained by saying that disease-germs are more minute organisms than bacteria, and are the food on which bacteria live. According to this view, bacteria not only do not constitute infection, but destroy it.

Dr. Maclagan says "that the chief action of an organism on its environment is the consumption of nitrogen and water. A disease-germ is a parasite, and requires a special *nidus* as well as nitrogen and water; the parasite finds a *something* in its *nidus*—the second factor—the parasite being the first. Without this second factor no bad result follows the reception of the contagium." Different periods of incubation are accounted for by the varying amount of the second factor and the number of germs imbibed; incubation itself by germ-growth and reproduction; and the onset of the symptoms by the germs becoming mature.

The consumption of nitrogen by the contagium particles causes wasting of the tissues, *i.e.*, the organisms eat the albumen intended to nourish the body. They also drink largely of water from the liquor sanguinis, which, being rich in soda, explains why soda-salts are often absent from the excretions during fever. The same retention of soda-salts, however, often happens in acute pneumonia, which has no relation to infectious disease.

Increased elimination of urea is explained thus:—"The increased consumption of liquor sanguinis by the contagium particles leads to increased formation of retrogressive albumen and of urea." It seems by this that contagium particles have livers and kidneys, and excrete urea. Diminished excretion of urea is held as "due to consumption by the contagium particles of the water requisite to enable the kidneys to perform their excretory function." What then, we ask, becomes of the water consumed by the contagium particles? Do their kidneys excrete urea in excess with limited water, while those of the patient are unable to do so? or do the contagium particles not consume water when the urea is increased as well as when it is diminished? In fever the quantity of water drank by the patients is very great, but that, according to Dr. Maclagan, is because the quantity of contagium particles is also very great. We find, however, the same symptoms in symptomatic fevers, with no contagium particles present, and we have great difficulty in believing that ultra-microscopic organisms in a person's blood could consume several tumblerfuls of water in twenty-four hours.

The heat of specific fevers is partly ascribed to the propagation of the contagium causing increased consumption of tissue. But increase of living matter causes the disappearance of heat, not its production. Again, the author states that the fecundation of the organisms may be accompanied by an elevation of temperature analogous to that which occurs under similar circumstances in other organisms.

As regards treatment, at page 163 the following occurs: "If we were to bleed, to purge, to give antimony to, or even *simply to withhold food and water* from, all the cases of typhus and enteric fever which occur, there can be no doubt that we should find the mortality from those diseases greatly increased." Dr. Maclagan is right here, for by *simply withholding food and water*, there can be no doubt that he would greatly increase the mortality by starving his patients to death. He, however, believes that fever patients should be supplied with nitrogen and water to compensate for what the organisms consume.

The cessation of the fever and its specificity are attributed—the first, to the organisms, as parasites, requiring a special nidus which contains a suitable pabulum, and when the latter is exhausted the fever ceases; the second, to a local lesion in the *nidus*, which is the part where fecundation of the organism takes place. In small pox this *nidus* and lesion is in the skin, in typhoid in the bowels, and so on. It is well-known, however, that many medicines act on special parts of the body, and yet we do not think of calling them parasites which require a special *nidus*. The author gives many other plausible, and some very unusual explanations of febrile phenomena by means of the germ theory, all of which, we believe, are far more clearly and rationally explicable on the physico-chemical theory. The writing of this book must have cost Dr. Maclagan much trouble. We have read it very carefully, and commend the author's honesty in stating his views, but question if the work will go far in realising the object for which it was ostensibly written.

OUR BOOK SHELF

A Manual of Cinchona Cultivation in India. By George King, M.B., F.L.S., Superintendent of the Royal Botanical Garden, Calcutta, and of Cinchona Cultivation in Bengal. (Calcutta, 1876.)

THIS manual is another contribution to the numerous books, papers, and articles that have appeared of late years on the subject of cinchona. Varied as these contributions have been, and valuable each one in itself, this manual brings together much that is useful, not only on the scientific aspect of the subject, but also on the harvesting of the bark crop in India, as well as on the commercial value of the Indian cinchona plantations. The manual will probably find its largest circulation amongst owners of land who have embarked in the cultivation of cinchona as a commercial enterprise, or those who intend doing so, Chapter iv. being devoted entirely to cultivation: and this part of the subject is treated of very fully; the author giving the various details of suitability of climate, temperature, rainfall, elevation, soil, drainage, &c., together with the more practical operations of preparing the ground, sowing seeds, propagation, planting, and other matters of a similar character, which, from the nature of Dr. King's position as superintendent of the Government cinchona plantations, must be trustworthy, if not from his own practical experience, certainly from the fact of his being able to command the opinions of the best men in this important branch. The same may be said of Chapter v., on the "mode of harvesting the bark crop." Turning to Chapter vii. on the "local manufacture of a cinchona febrifuge," we come to what is interesting and important to the whole community, namely, some of the practical results of the cinchona introduction into India, in the production of a cheap but efficient febrifuge. This preparation, which Mr. Broughton, the Government quinologist calls amorphous quinine, consists of the total alkaloids of cinchona bark, in the form of a non-crystalline powder, mixed to some extent with the resin and red colouring matter so abundant in red bark. "This alkaloid," we are told, "has been accepted by the medical profession in the Madras Presidency, as a remedy in malarious fevers, scarcely, if at all, inferior to quinine." About 600 lbs. of this substance was produced in the Neilgherry factory up the end of the year 1872-73, but the process of manufacture was found too costly, and the factory was accordingly closed. A more simple process was commenced in Sikkim, by Mr. Wood, who arrived in India in 1873, and by this process at the present time, about a ton per week of dry red bark is being worked up. The bark, hitherto so utilised, has been chiefly derived from thinnings and prunings, undertaken from time to time in the interests of the trees. By the end of the current financial year (1875-76) about 32,000 ounces of alkaloid will have been turned out. Next year a much larger quantity will be yielded. It has been calculated that of this efficient febrifuge there can soon be yielded from three to four tons annually, at a cost of rather less than one rupee per ounce.

Some interesting appendices are attached to the Manual—one shows the stock of trees in the Neilgherry cinchona plantation, another the stock in the Sikkim plantations, another the meteorology of the same plantations, and the last one gives the opinions of medical men holding important positions in India, on the efficacy of the cinchona febrifuge. With the manual are also issued three extra pages, descriptive of the process at present used for manufacturing the above substance, by Mr. C. H. Wood, the Government quinologist.

J. R. J.

Die Euganeen. Bau und Geschichte eines Vulkanes. Von Dr. Ed. Reyer. (Wien, 1877.)

THIS is Dr. Reyer's first publication, and we gladly acknowledge it to be a very promising one. The subject, a minute geological treatise of the Euganean Mountains near Padua, illustrated by a well-drawn map, hardly calls for a lengthy notice on our part, but the little work is attractively written, and testifies to the complete mastery the author possesses over his subject. He minutely describes the structure of these mountains, then dwells upon the consequences he draws from this regarding their geological history, and raises before the eyes of the reader an interesting picture of times long past, and of forms long extinct. Dr. Reyer's language has the advantage of being clear and to the point, and free from all unnecessary ornament. We have pleasure in recommending the book to our readers, and hope that it may soon be followed by another production from Dr. Reyer's pen.

Die Erde und ihre Völker: ein geographisches Hausbuch. Von Friedrich von Hellwald. Erster Band. Zweite Auflage. (Stuttgart: Spemann, 1877.)

THIS work has met with deserved popularity in Germany. Dr. Hellwald is known as one of the most accomplished living geographers, and is well fitted to undertake the compilation of a work like the present. It will, we believe, be completed in two volumes, the volume before us dealing with America and Africa. The author follows to some extent the method of Reclus in his *magnum opus*, though, of course, on a smaller scale. He takes the great divisions of the land and water one after another, and in a thoroughly interesting and clear style, summarises all that is known of them on the basis of the latest discoveries, and under a variety of well-selected heads. The work, so far as we have tested it, is up to the latest date, and we know of no more trustworthy, interesting, and handy compendium of geographical information. Some of the illustrations might bear improvement, especially in the case of North America, where, we think, a freer use might have been made of the magnificent illustrations in the U.S. Survey publications. On the whole, however, the work is a valuable "family book," as it is meant to be, and we should think would prove of considerable service to teachers of geography. We have no doubt that many would welcome an English edition of the work.

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. No notice is taken of anonymous communications.*]

Science Fellowships at Oxford

YOUR correspondent, Mr. Charles Wade, is an undergraduate of Magdalen College, and makes the very natural mistake of supposing that fellowships once assigned to natural science are, like the class of college prizes with which he is more familiar, namely, the scholarships, regarded by the colleges giving them as in a certain sense appropriated for future vacancies, to the subject which has once been connected with them. This is not the case, and accordingly your readers will find that Mr. Wade's enumeration of twelve fellowships, as assigned to natural science at Oxford, is erroneous, whilst the statement of "an Oxford Man" that only five fellowships are at this moment held as rewards for proficiency in natural science, is correct. From Mr. Wade's list must be removed the three Lee's readerships at Christ Church, which are not of the nature of ordinary fellowships, but are special foundations and enumerated by "an Oxford Man" with the professorships. Of the nine remaining on Mr. Wade's list, one at Merton does not exist, nor does that at Corpus, nor that at Pembroke, whilst that at Brasenose was not offered purely and simply for physical science. Hence there are but five fellowships at Oxford now held for natural science, or six if we count that at Brasenose.

Since I have no reason to ingratiate myself either with those who defend or those who attack the abuses of Oxford, I shall not imitate Mr. Wade, but sign myself

SOCIUS

Spectra of Metalloids

IN a recent number of NATURE (vol. xv., p. 401) I gave a short abstract of a paper by Messrs. Angström and Thalen, on which I should like to make a few remarks. It is known that Plücker first drew attention to the fact that one body may have different spectra, and he seemed inclined to attribute these spectra to different allotropic states of the element. Later on, however, attempts were made to give another explanation of the phenomenon. It is against these attempts that Angström and Thalen chiefly protest in their paper. They use, however, the word element, in a different sense from that in which it is generally used. An elementary body, they say, can only have one spectrum. We are aware that bodies, as iodine and sulphur, can give two spectra, but then the band spectrum is due to an allotropic state, which, from a spectroscopic point of view, behaves like a compound body. It would seem from this and other remarks, that, from a spectroscopic point of view at least, they consider an element to be not only a body which cannot be decomposed into two different bodies, but a body which cannot be resolved into any simpler molecular state. I have no objection against this if it is always clearly understood that our authors include allotropic states under the denomination of compounds. For instance, they lay great stress on the fact that a spectrum of fluted bands is always characteristic of a compound body. According to their definition of a compound this is perfectly correct, for no doubt the band spectrum belongs to a more complicated molecular state, but they cannot bring this argument forward as tending to show that Swan's spectrum of the candle belongs to a hydrocarbon, or that the fluted spectrum of nitrogen belongs to an oxide of nitrogen. The fact simply means that the molecule which gives these band spectra is to the molecule which gives the line spectra as the molecule which gives the absorption bands of iodine is to the molecule which gives the lines of iodine.

There is no doubt that we must be exceedingly careful, especially working with Geissler's tubes, not to ascribe to an element a spectrum which really belongs to a combination of that element with some other body present. The question what spectra an element really has must be settled in each individual case by careful experiments. Let us examine the two examples chosen by Messrs. Angström and Thalen. The first is carbon. Watts has already shown that the spectrum marked by him originally No. 2, really belongs to an oxide of carbon. The only spectrum under discussion is therefore Swan's spectrum of the candle. On this point Attfield's experiments are entirely conclusive. They have been amply confirmed by Watts and others. I take one case out of many. The flame of dry cyanogen gas shows the same spectrum brilliantly. The onus of the proof that a hydrocarbon can here be present lies entirely with those who make that assertion. Messrs. Thalen and Angström in the present paper assert that this spectrum is due to acetylene. In the year 1871 Prof. Angström published a paper, in which he tried to show that Wüllner's second spectrum of hydrogen really belongs to acetylene. Other experimenters have confirmed this fact. In order to escape admitting that carbon has two spectra, Messrs. Angström and Thalen are forced therefore to assume that acetylene has two spectra.

The chief object of this letter is to say a few words on the spectrum of nitrogen. In the year 1872 I published a paper in which I gave an experiment tending to show that the band spectrum of nitrogen really belongs to an oxide of nitrogen. The experiment was this: Clean pieces of sodium were heated in a tube containing nitrogen; the band spectrum then disappeared, and another spectrum came out, which I then thought to be identical with the lines of nitrogen. The experiments were repeated by Stearne and Wüllner; they also found that the bands disappeared, but the lines of nitrogen did not come out. I convinced myself that what I had seen was not the line spectrum of nitrogen, but the disappearance of the bands alone seemed to me to be an object of further investigation. Mr. Salet at last gave a full and correct explanation of the experiment. Nitrogen is absorbed by sodium under the influence of the electric spark, and the lines I had seen were the lines of sodium. As Mr. Salet has shown, my measurements agree better with the lines of sodium than with the lines of nitrogen. The bands of nitrogen remained if care was taken that the spark did not touch the sodium. If I have refrained hitherto from acknowledging the justice of Mr. Salet's conclusions, it is only due to the fact that I felt a natural curiosity to repeat his experiments; I have not yet been able to do so, but I have no doubt what the result would be. Mr. Salet's paper was only published after Prof. Angström's death, and I cannot help thinking that the professor would have considered his experiments conclusive against the assumption that the bands of nitrogen are due to an oxide of nitrogen. The only argument which Messrs. Angström and Thalen bring forward to support their theory is that in a tube containing rarefied air which showed the bands of nitrogen when the spark passed, nitric oxide was formed. But surely nitrous acid fumes are produced by sparks showing the line spectrum of nitrogen. Ozone is formed by sparks giving the lines of oxygen, yet we do not conclude that the line spectra of nitrogen and oxygen are due to nitrous acid and ozone.

If anyone still believes that an element can only have one spectrum at the temperature of the electric spark, I propose to him the following problem:—Let him take the three gases, carbonic acid, acetylene, and oxygen. If he investigates their spectra carefully he will at least find ten different spectra (two of them I only discovered lately). Out of carbonic acid alone he can obtain six. Let him find a sufficient number of possible compounds to account for all these spectra. ARTHUR SCHUSTER

The Annual Parliamentary Grant for Meteorology

THE Meteorological Department of the Board of Trade was, as is well known, constituted by Government in 1856 with the object of collecting and discussing facts and observations too numerous to be collected and discussed by private persons. The Department continued for ten years under the sole direction of Admiral Fitzroy, who, by his self-denying exertions and enthusiasm, and a genius for developing meteorology in certain of its practical applications, gave a great and withal healthy impetus to a sound study of the laws of weather.

In consequence of the recommendations of a committee of inquiry appointed after Admiral Fitzroy's death to review the results of the labours of the department, its control was transferred to a Committee of the Royal Society, who, in return for an annual grant of 10,000*l*., agreed to carry out the duties connected with the office. The Committee were left perfectly free in their method and in their choice of labour, the only condition attached to the grant being that an annual account be rendered to Parliament of the expenditure and of the results obtained in each year.

The support of the public was freely given to the Committee in the work they had undertaken, but in the course of a few years an opinion took root and gradually extended to the effect that the methods of inquiry adopted by the Committee and the work of the Meteorological Office were so seriously faulty as to call for inquiry. To some of these points attention was drawn in NATURE (vol. xii. p. 101), your criticism being limited to little more than the baldest statement of facts, which anyone could easily examine for himself, a criticism which, so far as we are aware, still remains unanswered.

Upwards of a year ago the Lords of H.M. Treasury, seeing that the Meteorological Committee had received nearly 100,000*l*., considered that the time had arrived for an inquiry, the grant being so considerable that they did not think they could be justified in continuing it for any lengthened period without satisfying themselves that the results obtained were such as to warrant the application of so large a sum of public money. A Treasury Commission was accordingly appointed on November 2, 1875, to inquire into the work of the Meteorological Committee, particularly that portion of it referring to storm warnings; and, in the event of their deciding to recommend the continuance of the grant, to consider further upon what system it may be best administered. In connection with the latter part of the inquiry the Lords of the Treasury gave expression to their wish that the claims of the Scottish Meteorological Society for aid from the State should receive the consideration of the Commission. The Commission consisted of Sir W. Stirling Maxwell, Chairman, Mr. Brassey, M.P., Mr. Lingen, permanent Secretary of the Treasury, Mr. Farrer, permanent Secretary of the Board of Trade, Dr. Hooker, President of the Royal Society, Mr. F. Galton, and Gen. Strachey. Considering the many scientific questions of a strictly technical character which were to be dealt with, it is to be regretted that such names as Sir G. B. Airy and Prof. Balfour Stewart were not placed on the Commission; and it was perhaps unfortunate that Mr. Galton and Gen. Strachey were on it, seeing that they were also members of the Meteorological Committee whose work was to be inquired into by the Commission. The name of Mr. Milne Home, chairman of the Council of the Scottish Meteorological Society, was on November 29 added to the Commission.

On looking over the Report of the Commission, I am surprised to find an inattention to several important matters remitted to them by the Treasury. I do not find, for instance, that the methods adopted by the Meteorological Committee for the observation of the temperature of the British Isles, to which serious objection has been taken, and the character of the work of the office, to which also serious objections have been made, have been inquired into; and I find that the consideration of the claims of the Scottish Meteorological Society for aid from the State have been all but ignored in the Report of the Commission.

Passing on, however, to the recommendations of the Report we find that it recommends that the annual grant be increased from 10,000*l*. to 14,500*l*.; that, at least provisionally, some assistance be given to the Scottish Meteorological Society; that ocean meteorology be transferred to the hydrographical department of the Admiralty; that the present system of collecting daily information by telegraph and of issuing storm-warnings be continued, an endeavour being made to put into clear shape, for the information of the public, the maxims or principles upon which storm-warnings in future are to be given; that the issuing of daily weather-charts be continued; that a certain number of continuously self-recording stations be retained; that the present system of supplementing self-recording observations by returns from eye-observers, that is, from ordinary meteorological stations, be continued; and, since the science of meteorology at present stands in need of hypothesis and discussion at least as much as, if not more than, of observation, that a part of the annual grant be appropriated to special researches, it resting with the Meteorological Council to select the investigators and fix the remuneration. Several of the recommendations are not put in the clearest shape in the Report, whilst others, such as those relating to the practical applications of the science, which concern the investigation of the relations of weather to health and agriculture, are so expressed as to suggest the idea that the nature of the problems involved in these large national questions has not been apprehended by the Commission, and consequently no provision is made in the Report for their proper investigation.

But by far the most important recommendations are those which refer to the constitution and action of the Meteorological Council, or new governing body, who are to be entrusted with the control of the grant, and the relation of the Council to the meteorological societies, which call for the gravest consideration. The services of the Meteorological Committee of the Royal Society are not to be continued, as suggested by some of its members when under examination before the Commission, the reason assigned being that it is not to be expected that they will continue to give much valuable time to the work under the existing conditions. It is proposed that the Royal Society be invited to recommend to the Government persons eminent in science who shall constitute the Meteorological Council, that they be fewer in number than the present Committee, and that they be remunerated in the shape of fees for attendance. This proposed reform as regards the body to be entrusted with the control of the grant in future is thus more nominal than real, and it is not improbable, looking at the phraseology of this part of the Report, that the Meteorological Council will be substantially the same as those who form the present Meteorological Committee, the only difference being that they will be fewer in number.

The value of the work of the meteorological societies is acknowledged, and it is recommended that co-operation with them be fostered to the utmost. The amount, however, of the assistance which the Commission recommends to be given to these societies out of the Parliamentary Grant, is indicated in these words of the Report (Art. 23):—"No payments should be made to them, except for results sought for by the Council." In other words, no assistance, whatever, is to be given to the meteorological societies out of the grant, because, these payments being only for services rendered to the Meteorological Council, cannot be regarded as grants to the societies, and they will neither aid them in conducting their own operations, nor remunerate them for the services which have long been rendered, and continue to be rendered, to Government Departments.

It is not in this way that foreign countries foster the prosecution of meteorology by their different nationalities. Thus, Hungary has its separate grant distinct from that of Austria proper; Norway has its grant distinct from Sweden; many of the Germanic States have their separate grants; and, as the readers of NATURE are aware, France is divided into meteorological departments, all of which are subsidised by the State, and their operations are aided, but not controlled, by the central office in Paris. In all cases, this division of work and responsibility is productive of greater economy and efficiency in carrying on meteorological research. Voluntary local effort is in this way evoked in a degree and to an extent not otherwise possible, and the healthful principle of competition and mutual criticism, so desirable in the present state of the science, is called into active play. Surely some provision ought therefore to be made by our own Government, to subsidise these societies, it being on all hands admitted that voluntary subscriptions alone are inadequate for their permanent efficient maintenance. In no other way can they be placed in a position to discharge the duty of a public department in collecting statistics for the elucidation of the climatology of a country in its details and practical applications.

In the estimated expense of the Meteorological Office on its proposed new footing, I observe that 1,500*l.* is set down annually for "New Land Stations," and in the appended outline of "Duties of Future Council," they are to place themselves in postal and therefore direct communication with about 150 secondary stations in the United Kingdom. What does this mean? Is it intended quietly but surely to supersede, and in a few years supplant the meteorological societies—a course which the Report not only permits to be done if the Council be so minded, but also includes in the grant an annual sum of 1,500*l.* which may be so applied?

It cannot be supposed that any council, composed of persons eminent in science, could be formed at present, possessing the knowledge and technical training required to direct the conduct of the whole field of meteorological research, both physical and climatological. Certainly, keeping in mind the serious mistakes made under the *régime* of the Meteorological Committee, and the manner in which the work of the office has been conducted, to both of which attention may here again be drawn, no other result can reasonably be expected than that the programme, as sketched in the Report of the Commission, is seriously in fault. The fact that the work was not efficiently controlled, was no doubt one of the main reasons of appointing the Commission; and, therefore, virtually to reappoint the same controlling body, only under another name, armed with the means and powers as proposed by the Commission, could scarcely fail to result in work of a more or less unsatisfactory nature, and, besides, in an unnecessarily increased expenditure of public money.

It is difficult to see how this result can be obviated, except by remitting the whole question of local climatology, including its practical applications, to bodies located in each of the three kingdoms, these societies being at the same time intrusted with supplying the Registrar-General, as has been done in the past, and such bodies as the Medical Council and the agricultural societies of the United Kingdom, with the information they may require. If 1,000*l.* were given for this work to each of the societies referred to, and 1,500*l.* to the Admiralty for the work of ocean meteorology, there would still be 10,000*l.* for the central office in London, to be devoted to the issue of storm warnings and to the prosecution of the more purely physical researches of meteorology, of which the science at present stands so much in need, and which is so emphatically the proper work of the central office: and in this case it need scarcely be added that each of the societies would necessarily be represented on the Meteorological Council. ZETA

Centralism in Spectroscopy

PERMIT me to repeat that happy remark of yours, and happy because so true and of wider application than the one meteorological case which called it forth, on p. 427; viz.: "We think centralisation hurtful to science, and we regret that 1,000*l.* a-year has not been granted to Scotland, by which a healthy rivalry would have been gained."

In the Anniversary Report of the Royal Astronomical Society for last month will be seen a statement that the Royal Observatory, Greenwich, amongst a vast deal of other most undoubtedly admirable work, is also now having a large spectroscope made for itself on a totally new plan. How many spectroscopes that observatory has had made for itself during the last twenty years I do not know! and I, for one single individual in the nation, do not grudge this new one if it should realise only half of the promises made for it.

In the same Anniversary Report there is also a statement that the Royal Observatory, Edinburgh, has been in want of a proper spectroscope for its special local observations for years and years past; and if at length there is one there now, almost good enough for the required purposes, it is because such a one has been recently made at the private expense of the Astronomer-Royal for Scotland, although his salary is less than that of many a clerk in London.

Of this, also, I am told there is no reason to complain, because I accepted the situation in its poverty-stricken condition, though when the nation itself was also poverty-stricken as compared with its present truly heaven-favoured condition. But what I do, merely as an individual, complain of is—that if the new Greenwich spectroscope is to be the *only one* which the centralisation of the British Government in London allows to be built up at the expense of the whole nation, out of taxes levied in Ireland and Scotland as well as England, that it is being made on a principle which goes against the laws of Sir Isaac Newton and nature, and which, though it *may* give with "two or at most three half-prisms" more dispersion than was before obtained by "ten whole ones," does so at the cost of *all definition*, and will be certainly allowed at Greenwich, as well as everywhere else, to be a mistaken step in modern spectroscopy before another anniversary of the Royal Astronomical Society takes place. PIAZZI SMYTH,

Edinburgh, March 17 Astronomer-Royal for Scotland

Greenwich as a Meteorological Observatory

IN NATURE (vol. xv. p. 421) there appeared a brief abstract of the presidential address of Mr. H. S. Eaton to the Meteorological Society of London on February 21. The increase of temperature at Greenwich in recent years is stated to be in reality due to local causes and not to secular variation, to which it has, as he thinks, been erroneously assigned. The effect of the growth of the population of London from 900,000 at the commencent of the century to 3,500,000 at the present time, and the still greater increase in the comparative consumption of coal, Mr. Eaton considers to be manifested by the rise in the average temperature of the air at the Royal Observatory, and for this reason it is concluded that Greenwich is not a suitable place for a meteorological observatory of the first order.

If the view enunciated by Mr. Eaton be correct, it is evident that the temperature of Greenwich during recent years has been in excess of that of surrounding districts. Is this view borne out by observation? Taking the figures for a number of places in the south-east of England whose mean temperatures have been calculated for the same thirteen years ending 1869, and adding the usual correction for height above the sea, we obtain the following results as their mean winter, mean summer, and mean annual temperatures; Greenwich, 40°·4, 63°·1, and 51°·1; Camden Town, London, 40°·4, 63°·3, and 51°·1; Royston, 40°·5, 62°·3, and 50°·8; Colchester, 39°·4, 62°·8, and 50°·6; Worthing, 41°·1, 61″·2, and 50°·7; Osborne, 42°·0, 62°·5, and 51°·8; Aldershott, 40°·9, 62° 6, and 51°·2; and Oxford, 40°·6, 61°·3, and 50°·4. A simple inspection of these figures is sufficient to show that the consumption of fuel and the vast population of London cannot be said to have had an appreciable influence on the temperature as recorded at the Royal Observatory, and that if the Greenwich observations show a rise of temperature during recent years, the whole of the south-east of England has shared in that rise. This result deduced from observations is such as might have been expected when the position of the thermometers at Greenwich and the mode of escape of the artificially heated air by chimneys into the free atmosphere is taken into consideration. It follows, therefore, that, so far at least as regards the temperature observations, the conclusion drawn as to the future of our great national Observatory as a contributor to the higher meteorological researches is not supported by the facts of observation.

ALEXANDER BUCHAN

Atmospheric Currents

I AM glad to have obtained from such exponents as Capt. Digby Murray and Mr. Murphy a clear statement of the old orthodox creed respecting the movements of the atmosphere.

The former, it is true, finds a difficulty in accepting Maury's belief that the currents cut one another in "curdles" in the equatorial calms, but none in adopting the same as regards the tropical calms, and his view may therefore, as I suppose, be taken as a modification of that which is graphically represented on Plate I. in the "Physical Geography of the Sea."

The question at issue between Capt. Digby Murray and myself amounts to this: Are rapid polar and equatorial upper currents observed over the region of tropical calms? Mr. Murphy's theoretical question appears to me to involve the inquiry—Is the force of the trades derived from the earth's rotation?

In tracing the course of the air particles along the route which he describes, the late Commodore Maury observes that this course is determined in certain particulars by "some reason which does not appear to have been very satisfactorily explained by philosophers." The latter do not as yet seem to have got rid of all the difficulties with which his theory is beset, which rather grow with its development.

I would beg the philosophers to look closely at the actual course of the atmospheric currents as shown by synoptic charts, not by charts of prevailing winds and mean pressures, which represent conditions never found at any one time in nature. The distinction between the "great currents" and the "temporary currents" is important enough, but it amounts to little more than that between mean winds and actual winds; and to explain the mean winds on one principle and the actual winds on the opposite involves a fallacy.

Again and again we see a more or less irregular belt of high pressures, having central calms, extending across the North Atlantic. From the southern edge of this belt we may follow a particle of air in its course to and from the equatorial district of low pressure, also an irregular belt in the middle of which calms exist. The movement originates in the defect of pressure near the equator at the level occupied by the particle. Its velocity is governed by the steepness of the gradient; and its direction, in relation to the surfaces traversed, is affected by the increasing velocity of rotation of those surfaces. In the Doldrums it arrives at a district at which the gradient becomes zero and the horizontal movement has consequently disappeared; but a vertical movement has now been acquired from the difference in the tension of the particles above and beneath, a difference derived from solar heat. When the particle has arrived at a position in which this difference disappears the vertical movement vanishes, and a new horizontal movement commences owing to the defect of pressures on the polar side at the level then reached, and the direction of this movement is also affected in relation to the surfaces by their decreasing velocity of rotation. Where does the new movement terminate? Obviously in some district between the equator and the pole where horizontal pressures on all sides of the particle at its then level are equable, but where a vertical movement has been acquired, the particles near the earth's surface starting on their journey towards the equator. The *onus disputandi* lies with those who deny that such a district is presented by either of the belts of tropical calms.

We now look at the polar side of the calm belt of Cancer, and for this purpose we may take almost any, *e.g.*, of Capt. Hoffmeyer's charts. We see in the majority of cases an aggregate of cyclonic circulations around local barometric minima, interfering and imperfect, and commonly becoming more so as they are propagated towards the pole. But we see no "polar depression" *distinct* from these, on which, as represented in the chart, we can lay the finger and say, "This is the result of centrifugal force; those are due to steam power." Within these systems an upward movement of the air occurs, owing to vapour condensation and the liberation of heat. Consequently towards these the particles of air near the earth's surface at the poleward edge of the tropical calms begin to travel, the earth's rotation deflecting their course in relation to the surfaces traversed. And from these, at a certain elevation, the particles return to the tropical calms for the same reason as that which determines the upper currents over the trades.

From the phenomena observed in the northern hemisphere I argue, *mutatis mutandis*, to those of the southern, and I expect the argument to be admitted by one who, like Mr. Murphy, attributes much less influence than I do to the work of water-vapour, and who even thinks that the mean movements of our atmosphere would be unaffected by the removal of all the water of our globe.

On some occasions pressure is high over all the North Atlantic on the polar side of the tropic, the anticyclones apparently extending nearly to the pole. In these cases we have no surface counter-trades over that district, yet the north-east trades continue to blow on the southward of the tropic as usual.

I repeat that all movements of the atmosphere originate in differences of pressure derived directly and indirectly from solar heat, and not in the force of the earth's rotation. And I must add that it seems to me very strange that any one while regarding the trades and their upper-currents as simply the *effects* of pressure differences in the lower latitudes, should maintain that the south-west and north-west winds of the temperate zones are simply the *causes* of the pressure differences in the higher latitudes. It would be just as logical to regard the south-west and north-west winds as due to pressure distribution, and the trades as the compensation for their eastward movement. W. CLEMENT LEY

March 10

Electrical Phenomenon

LAST night I noticed a powerful development of electricity in a curious manner. I had thrown a piece of common, thick, white, unglazed paper upon a low fire which was tolerably full of ashes. When it was charred so as to be black and brittle, I happened to take it up and break bits off. To my astonishment they stuck firmly to my fingers. I broke off two pieces each an inch long, and resting them on the tips of my two fore-fingers, each was capable of rotating (though with very great friction). When brought near each other they repelled each other forcibly. I experimented with these pieces for several minutes without perceiving any diminution in their electrification. Both sides of the paper seemed to be in the same condition. I then laid them down, and left the room to fetch a piece of sealing-wax to test the nature of the electricity. But by the time I returned, all trace of electricity was gone, and by no means could I repeat the experiment so as to get the slightest charge of electricity.

It is more than probable that the electricity was developed by the chemical action of combustion of the coals, and that the hot air rising up and brushing past the paper acted as a carrier of electricity of one kind to the paper, and of the opposite kind from it, until it acquired a very high potential. But it would be interesting to learn exactly in what manner this action takes place, and whether the electrification was positive or negative.

Andersonian College, Glasgow GEORGE FORBES

Strange Star.—Meteor

ON going out last Saturday evening about 8.55 P.M. my attention was arrested by a large deep red star in Serpens which I had never seen before. Its magnitude was greater than Arcturus, though its deep colour made it seem less bright. About ten minutes afterwards I saw it increase and diminish in magnitude two or three times producing the effect similar to a "flashing" light on the coast, after which it suddenly disappeared.

On the same evening, at 9.56, I saw a very fine meteor of a bright pale blue colour with coruscations of ruby colour at the nucleus. Its course was from Gemini over Aldebaran, disappearing below Pleiades. Of a long pine-cone shape, duration about three seconds. W. M.

Gunnersbury, March 19

SCIENCE IN GERMANY[1]

THIS book forms a continuation of the researches which in vol. i. treated of the season dimorphism as the result partly of exterior influences and partly of atavism. The present (second) volume comprises: (1) the origin of the markings upon caterpillars; (2) on the phyletic parallelism in metamorphous species; (3) on the transformation of the Mexican Axolotl into an Amblystoma; (4) on the mechanical conception of nature. The third treatise was published separately some time ago and was reviewed in NATURE; here we particularly wish to draw the attention of the lovers of natural science to the first paper. Weismann tries in the treatise in question to prove by his observations, and the deductions therefrom, that exterior influences and natural development or adaptation (Naturzuechtung) only can be the causes of the markings upon caterpillars. The observations referred to were made upon the caterpillars of several genera of *Sphingidæ*, and relate to the history of their development.

I. *Chærocampa.*—The caterpillars of *Ch. elpenor*, which have just left the ova, show no markings of any kind in this first stage, being of a uniform greenish colour; after the first change of skin (second stage) they show a bright longitudinal streak on each side, between the dorsal line and the line of breathing apertures (stigmata). To this first streak Weismann gives the name of "subdorsal streak." In the third stage eye-shaped spots form in the fourth and fifth segments, inside of these streaks, and these are completely developed in the following stages, *i.e.*, after the subsequent changes of skin, while at the same time the subdorsal streak decreases and leaves only imperfect traces. In the fifth stage the greenish colour changes to a brownish one, and the horn at the tail of the caterpillar becomes shorter. In the sixth and last stage the other segments begin to show eye-spots. but these are not developed to perfection. *Ch. porcellus* shows the same form and development of the larva, with the only difference that most of the phenomena occur one stage earlier than with *Ch. elpenor*. This conformity and accord of both genera in the order in which the markings upon the caterpillars appear and are developed, lead to the conclusion that the markings were acquired in the same order during the progress of development (phylogeny) of these caterpillars; the oldest form (*a*) therefore showed no markings at all even when perfectly developed; the following form (*b*) had only the subdorsal streaks; then in form (*c*) the eye-spots occurred in the fourth and fifth segments, and finally in all segments (form *d*). It is probable further, that of the two genera of caterpillars now living, *Ch. elpenor* is the original, *i.e.*, older form, on account of its still showing the different stages of development in their completeness; the younger or more advanced form, viz., *Ch. porcellus*, proves that each new marking, acquired during the progress of development, appears first in the later stages and then gradually extends to the earlier stages. The whole of this view is well supported by the markings upon the complete form of the other species belonging to the genus *Chærocampa*, of which the development of the larva is still unknown in its different stages. These other species may be divided into three groups, corresponding to forms *b*, *c*, and *d* of the phylogeny in such a manner that wherever the subdorsal streak remains in perfection, the eye-spots are not developed, and wherever these show themselves the subdorsal streak is decreasing. Form *a* (first group) is known in the full-grown caterpillars of three species (*Ch. syriaca*, *Daraspa myron*, and *D. chærilus*); to the second group (form *c*) belong the above described *Ch. elpenor* and *Ch. porcellus*, together with several others; the last group (form *d*), which shows completely developed eye-spots on all segments, is even more numerously represented by *Ch. bisecta*, *oldenlandiæ*, *alecto*, *actæus*, *tersa*, and *celerio*. The species of the genus *Chærocampa* which Weismann examined, therefore represent three phylogenetic stages of development, and it is interesting that the tropical species are the most advanced ones. It is probable, indeed, of one species, viz., *Ch. celerio*, that in Europe it shows form *c* in the markings of its caterpillars, while in India the larvæ of the same species have already attained form *d*.

II. In a similar way the author shows that markings upon the larvæ of the genus *Deilephila*, to which the well-known *D. euphorbiæ* (commonly called Sphinx) belongs, have passed through seven phylogenetic stages of development, viz., (1) caterpillars without markings; (2) with a subdorsal streak; (3) with a ring-shaped spot upon the last segment but one; (4) with similar but not altogether perfect spots upon all segments; (5) with eleven perfect ring-spots upon the subdorsal streak; (6) with these ring-spots but without the streak; (7) with a double row of ring-spots. Nowhere in the development any deviation from this order is noticed, and the living species of this genus form five groups, the markings of their full-grown caterpillars corresponding to the phylogenetic forms Nos. 3 to 7.

III. A somewhat smaller number of stages of development is apparent in the genera *Smerinthus*, *Macroglossa*, *Pterogon*, *Sphinx*, *Anceryx*, which Weismann investigated less extensively than those mentioned before; however, he points out that upon their larvæ the simple subdorsal streak combines, in the course of development, with other longitudinal or oblique streaks, or becomes less distinct as the others increase in intensity.

Now Weismann considers that the remarkable conformities in the development of all the larva markings he investigated is the surest proof that we are dealing with a phenomenon of inheritance. Indeed three laws may be said to be established by these conformities, viz., (1) the development begins with the simple and progresses to the more complicated markings; (2) new markings first appear in the last stage of individual development; (3) these new markings then gradually pass backwards to the earlier stages and thus replace the older ones, causing them to disappear entirely. Weismann gives the following explanation of the phenomenon referred to in the second law:—Supposing that the respective markings are of use to the caterpillars, that therefore they are retained in subsequent generations by natural adaptation, this use can only be real if the caterpillars are big enough to resemble the different parts of the plants on which they feed, and thus escape being noticed by their enemies; and if a sufficient lapse of time is given for carrying this protection into effect. Both these conditions, however, are united in the last stage of development, where the caterpillars have attained the necessary size, and which is the longest of all stages. The use of the colour of caterpillars, and the markings upon them, is also perfectly evident. The younger ones are green as long as during the day they remain on the leaves of the plants they feed on; they do not then form a contrast with the colour of the leaves themselves. The older caterpillars remain green if the thick foliage of the plants protects them under all circumstances; if, however, the foliage is less dense, so that the caterpillars, as soon as they have grown bigger than the leaves, can be easily distinguished among them, they leave the green leaves in the day-time, and try to hide on the stems of the plants and among withered leaves; in that case, to complete the protection, their colour changes from green to brown. The biological value of the characteristic markings upon caterpillars, quite independently of their colour, may be recognised from the fact that

[1] Weismann, "Studien zur Descendenztheorie" ("Researches on the Descent Theory"). Vol. ii. On the last Causes of Transmutations.

the caterpillars which live permanently in the dark, or those of the Microlepidoptera, just as little as those of the first stages of development of most butterflies, have no markings at all; their small size, or their habit of hiding themselves, sufficiently protect them from their enemies; they, therefore, need no markings to insure their safety. When the caterpillars are getting bigger the longitudinal streaks become useful, as through them they do not contrast so much with long-shaped leaves, fir-needles, or stems. The caterpillars with longitudinal streaks, such as those of *Satyridæ*, *Pieridæ*, &c., almost without exception live on fir-trees, grass, or plants growing among grass. The oblique streaks in the segments of other green caterpillars imitate the lateral ribs of the large leaves upon which these species live. The eye- and ring-shaped spots form another means of protection. On the one hand, they may imitate the berries of the plants on which the caterpillars feed, and protect the latter, inasmuch as the berries are still unfit to be eaten at that particular time (*Deilephila hippophaes*). On the other hand, the spots, greatly resembling eyes, most decidedly act as means of frightening the enemies of the larvæ; this is particularly the case with the *Chærocampa* species, as, whenever any danger threatens them they draw their foremost segments into the fourth and fifth ones, and the eye-spots upon these then glare on the puffed-up fore-part of the animal. Weismann has proved this experimentally, by throwing such caterpillars as food before birds, and then watching the expression of fear on the part of the latter. There are other caterpillars the markings upon which cannot possibly be looked upon as means of frightening their enemies, as their repulsive odour or taste alone suffice to ward off the insectivora. Wallace has shown that such insects bear their many coloured marking like a stamp of their unfitness for food, and already by this frighten off insectivorous animals Weismann has proved by some experiments that lizards not only refuse certain caterpillars (*Smerinthus*, *Sphinx*, &c.) at all times, but are even diffident towards others which are marked in a similar manner although quite edible, and only eat them after minute examination.

It is certain that many of these useful markings were acquired by natural adaptation (Naturzuechtung), and it is quite beyond doubt that others have resulted from the internal laws guiding the formation or growth of caterpillars, *i.e.*, through correlation of the different parts of the insect, independently of all usefulness. This is proved by the retrograde movement of the markings acquired in later stages towards the earlier ones, where the markings can but be perfectly useless. The eye-spots, in the same way, first appear through natural adaptation near the head or the tail of the animal, and are then of use; but later on they spread over the other segments also, and here they only reappear, because in articulata the *general* tendency exists to develop all segments in an equal manner. On the other hand the gradual disappearance of certain markings must be ascribed to natural adaptation, because under different conditions of life, more useful markings supplanted the existing ones, which had become useless. If the second phylogenetic form of the *Sphingidæ* caterpillars with single longitudinal streak, seems to indicate that the animals then lived on grass, these streaks became useless and even obnoxious when the caterpillars selected shrubs and trees for their food, and were then replaced by the more appropriate and useful eye-spots.

In short, as far as Weismann investigated the markings of caterpillars, particularly those of *Sphingidæ*, he could prove their development to be caused by external influences (natural adaptation and subsequent correlation), and could consequently reject the assumption of a special creative or form-shaping power.

D'ALBERTIS'S EXPEDITION UP THE FLY RIVER, NEW GUINEA

THE *Sydney Mail* of Saturday, January 20, contains a long account of the expedition of the Italian naturalist, D'Albertis, up the Fly River, New Guinea, translated from his diaries, and communicated by Dr. George Bennett. Signor D'Albertis left Sydney, April 20, 1876, in the mail-steamer, *Brisbane*, and reached Somerset, Cape York, on May 1, where the steam-launch *Neva*, which had been provided for the purpose of the expedition by the liberality of the good citizens of Sydney, was disembarked and equipped. On May 19, after various small casualties, a start from Port Somerset was effected, and Katow, on the coast of New Guinea, reached on the second day. Hence the mouth of the Fly River, already well known to D'Albertis from his previous expedition in the *Ellangowan*, in 1876, was soon entered, and more or less progress was made every day. The land traversed appears to have been mostly low and swampy. On June 20, being on shore, Mr. D'Albertis ascended a hill 250 feet high, and from the summit saw some "very high mountains" in the north-east, fifty or sixty miles distant—probably part of the "great Charles Louis range." On June 28, after having been for some time aground, and only got off by an unusually heavy flood, it was determined to return and to try the western branch of the Fly River. The strong current and other adverse circumstances rendered it necessary to abandon this attempt likewise, after about a week's struggle, and the *Neva* returned to the coast, when the expedition passed several months amongst the islands, and finally returned to Somerset in November. The following is Mr. D'Albertis's summary of his discoveries:—

"After my long narrative I shall conclude with a few words expressing my regret at not having been able to do more. But it is often not the pioneer who shows the way that attains the most glory, but those who follow him; it is easy to hear of a road, but very difficult to find one out. I wish every success to any explorer of this part of New Guinea (should I not be able to return and complete my work), and I hope that the little I have done will be some guide and enable him to find his way more readily than I did mine, and to correct any errors I may have made. By this exploration we are now acquainted with a road into the interior of New Guinea, which is of the more importance, as it is so near to Somerset, where a line of large steamers calls twice every month. We have also found a passage from Moatta to the Fly River, shorter and safer than the one previously known, and a passage which, when properly surveyed, may be found navigable for larger ships. The richness of the land we visited, its vegetable, and probably mineral, products, the soil suitable for many of the most valuable plants, as coffee, sugar, cotton, india-rubber, sago, tobacco, nutmeg, &c., ought to attract the capital of the colony to open up the country. The Dutch from their part of New Guinea, although on a small scale, derive some trade. The part of New Guinea into which we penetrated, was in latitude 5° 30′ S., and ran about 500 miles on the winding river, the course of which may be seen on the chart appended, and it almost forms a line of demarcation between that part of New Guinea claimed by the Dutch, and that remaining as yet unclaimed by any nation.

"About the Fly River, as far as I could judge, the natives appear less numerous than I have seen in other parts of New Guinea, and the land is cultivated in a smaller quantity, so that in this part of New Guinea the settler will not find the same difficulties which I have pointed out on former occasions when speaking of the south-eastern peninsula, where the natives are more numerous, and possess and cultivate all the best land. I have appended Baron von Mueller's report on my collection of dried plants;[1] and on the return of Prof. Liversidge to Sydney he will report on the small collection of minerals, &c., I submitted to him for examination. On a day not far distant I hope to give the ethnological report on the natives, their weapons, &c., also on the mammals and the birds collected, the latter consisting of about fifty species, many of which are new, or only recently described from specimens obtained during my first visit to the Fly River. I have also a rich collection of reptiles, fishes, both of salt and fresh water, some beetles, and some fresh water and land shells. I confidently expect that the voyage of the *Neva* will be remembered by those who take an interest in New Guinea, and by the scientific world."

[1] Some extracts from this were given in our last issue, p. 438.

THE PHYSIOLOGICAL ACTION OF LIGHT[1]

II.

DETERMINATION of Electro-motive Force.—Soon after the first experiments were announced, certain physiologists said that although the results of the action of light which I have just described may be observed, to say there was a change in the electro-motive force, as stated in the earlier communications, was not correct. That the effect was due to an alteration in the electro-motive force had been proved, but experimental details were reserved for the second part of the investigations. At first Sir William Thomson's electrometer was used, but the amount of electric potential to be measured was too small to get good results. Another plan of determining the electromotive force was adopted. This was the method introduced by Mr. Latimer Clarke, the eminent electrician, and described in his work on "Electrical Measurements." The instrument devised for this purpose is called by him a Potentiometer, and measures electromotive forces by a comparison of resistances. Practically we found the Daniell's cell far too strong a battery to use as a standard of comparison. A thermo-electric junction of bismuth and copper was substituted for it. One end of the junction was constantly heated by a current of steam passing over it, the other being immersed in melting ice. The electro-motive force of this thermo-electric junction, as estimated many years ago by Regnault, is extremely constant, and is about the $\frac{1}{175}$th part of a Daniell's cell. By means of this arrangement the following results were obtained:—The electromotive force of the nerve-current dealt with in experiments on the eye and the brain of a frog varies from the $\frac{1}{300}$th to the $\frac{1}{400}$th of a Daniell's cell. Light produced an alteration in the electro-motive force. This change was, in many instances, not more than the $\frac{1}{10000}$th of a Daniell's cell. But though small it was quite distinct, and proved that light produced a variation in the amount of the electromotive force. By the same arrangement the gastrocnemius muscle of a well-fed frog gave $\frac{1}{35}$th of a Daniell; the same muscle from a lean frog which had been long kept, gave $\frac{1}{240}$th of a Daniell; and the sciatic nerve of the well-fed frog $\frac{1}{480}$th of a Daniell. Dr. Charles Bland Radcliffe states, in his "Dynamics of Nerve and Muscle," p. 16, that he obtained by means of Sir William Thomson's quadrant electrometer, from a muscle a positive charge equal to about the tenth of a Daniell's cell, a much greater amount than ascertained by the method I have just described.

The electro-motive force existing between cornea and posterior portion of the sclerotic in a frog amounts to $\frac{1}{120}$th part of a Daniell, and between the cornea and cross section of the brain is about four-fifths of the above.

Effect of Temperature on the Eye of the Frog.—From numerous experiments on the irritability of muscle induced by the excitation of nerve, it has been satisfactorily proved that a temperature of about 40° C. destroys the action of motor nerves in cold-blooded animals. Up to the present time we are acquainted with no observations as to the temperature at which a terminal sense organ becomes incapable of performing its functions. Having satisfactorily proved that *the retina is the structure in the eye producing the electrical variation* observed, it becomes evident that as long as this phenomenon can be detected the retina is still capable of discharging its normal functions. In order to investigate thoroughly the effect of an increasing temperature on the sensibility of the retina, a method of procedure was adopted of which the following may be taken as a general account:—A frog was killed, the two eyes removed rapidly from the body; the one eye was placed on electrodes and maintained at the ordinary temperature of 16° C., while the other was placed on similar electrodes contained in the interior of a water bath having a glass front, the sides of the air chamber being lined with black cotton wool saturated with water. Into this chamber a delicate thermometer was inserted, and the currents coming from the two eyes were alternately transmitted to the galvanometer every five minutes by means of a commutator, the temperature and the electrical variation produced by the same amount of light being noted in each case. The general results are shown in the following table:—

Table showing Comparative Effect of Temperature on Sensibility of Frog's Eye.

Eye kept continuously at 16° C.		Eye at different Temperatures.		
Initial Effect.	Final Effect.	Temperature.	Initial Effect	Final Effect.
55	28	16° C.	58	21
61	28	19° C.	55	16
53	27	24° C.	65	14
53	39	29° C.	97	5
53	45	29° C.	103	—4
60	45	37° C.	65	—3
60	50	38° C.	65	—4
53	41	43° C.	12	—5
60	40	43° C.	no effect.	no effect.

The initial amount of current was, however, increased on the whole by the action of the higher temperature, thus showing that the sensibility to light does not depend on the amount of current circulating through the galvanometer. It will be observed, on inspecting this table, that the eye maintained at the temperature of 16° C. remains tolerably constant in its initial action, although it gradually gets more sluggish, whereas the final effect steadily rises. On the other hand, in the case of the eye subjected to a higher temperature, the initial effect seems to have a maximum about 29° C., then gradually diminishes, and vanishes about 43° C., the final effect continuously falling and being actually reversed. To succeed in this experiment it is necessary to heat the electrodes which are to be used in the water bath up to 40° C., in order to be certain that no changes are induced in the electrodes themselves that might be mistaken for those above mentioned. An eye that had been placed in dilute salt solution along with lumps of ice was found to have the usual sensibility to light.

Effect of Temperature on the Eye of Pigeon.—Having succeeded in experimenting with a water-bath in the manner above described, it appeared interesting to ascertain if the eye of a warm-blooded animal would be benefited by being maintained at the normal temperature of the body. The head of a pigeon was placed in the water bath at a temperature of 40° C., the eyes were found sensitive to light, the action, however, being always a negative variation; but instead of vanishing quickly, as it does at the ordinary temperature, kept up its activity for at least an hour. For example, in one experiment, the electrodes being placed on the corneas so that the currents were balanced, sensibility was active for an hour and a quarter, but half an hour later it had almost disappeared. In this experiment the sensibility of the eye is shown by the large deflection produced by a single candle at different distances, thus:—

Distance of Candle from Eye.	Divisions of Galvanometer Scale.
9 feet	100
6 feet	180
3 feet	230
1 foot	420

Sensibility of the Optic Nerve.—When the retina is entirely removed from the eye-ball, and the optic nerve is

[1] Friday evening Lecture by Prof. James Dewar, M.A., at the Royal Institution, March 31, 1876. See NATURE, vol. viii. p. 204. Continued from p. 435.

still adherent to the sclerotic, no effect of light can be detected. It now appeared possible to examine this question by repeating Donders's experiment of focussing an image on the optic disc in the uninjured eye, when no electrical disturbance ought to occur. This was done in the eye of the pigeon, but an image free from irradiation on the optic disc could not be produced, and consequently there was always an electrical effect observed.

Exhaustion and Stimulation of the Retina.—When the same light from a fixed position is allowed to act on the eye for successive intervals of time, say two minutes of light and two minutes of darkness, it gradually falls off in electrical sensibility. Thus, a candle at nine inches gives the following results when successively used as a stimulus :—

	Initial Effect.	Final Effect.
1st experiment	259	254
2nd ,, ...	171	276
3rd ,, ...	140	282
4th ,, ...	122	274

These figures show a rapid fall of the initial effect. In these circumstances, it is evident that the image being

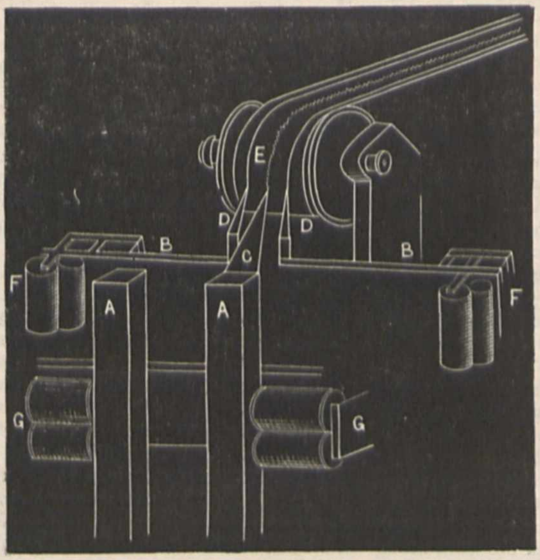

Diagram showing the recording portion of Regnault's Chronograph. A A, limbs of recording fork, worked by electro-magnets, G G. C, stilette on limb of recording tuning-fork. B B, levers in connection with armatures of electro-magnets, F F, and bearing markers, DD, which, along with C, record on E, a strip of blackened paper passing over pulley.

always localised on the same minute portion of the retina, only a few of the rods and cones of that structure are really exhausted. If the eye be allowed repose in the dark for a period of from half an hour to an hour, it will regain as much as triple the exhausted sensibility. But another mode of proving that only a minute portion of the retina was affected was to show that an alteration of position of the image by a slight movement of the luminous body was followed by a new electric variation. In order to vary and extend the action of a retinal image it is necessary to suspend a steady lamp by means of an indiarubber cord or spiral spring, so as to be able, by inducing vibrations in any direction, to stimulate in rapid succession different retinal areas. On oscillating a pendulum of this kind, an electrical variation is observed whenever the amplitude of the vibrations is increased, and by inducing a combination of vibrations, the electrical variation observed corresponds to what would be found if the luminous intensity were sixteen times as great as that of the stationary light. Similar experiments may be made by throwing an image from a small silver mirror connected with a metronome. The rapid exhaustion of the eye may be most readily demonstrated by cutting off the anterior half of the eye, leaving the vitreous humour in contact with the retina, observing the effect of a candle, and then subjecting it to the action of a magnesium lamp. The sensibility will now be enormously diminished. The electrical variations resulting from the respective actions of a candle and a magnesium lamp placed at the same distance from the eye were as follows :—

	Initial Effect.	Final Effect.
Candle	38	78
Magnesium lamp ...	120	135

This experiment proves that an increase of 200 per cent. in the illuminating power of a source of light only triples the electrical effect. Thus the eye becomes less sensitive as the illumination increases.

Chronometrical Observations.—A series of experiments have been begun with the object of measuring the time required from the initial impact of light before electrical variation is produced. As the electrical variation has been shown to agree with our consciousness of luminous effects, it became an interesting point to ascertain whether the time occupied by the action of light upon the eye of the frog is similar to the time occupied in its action upon the eye of man. A good many years ago, Prof. Donders and his pupil, Schelske, performed a number of experiments by which they determined that the time required by the human being to observe light and to signal back the impression occupied about $\frac{1}{10}$th of a second. That is to say, $\frac{1}{10}$th of a second is occupied by the action of light on the eye, the transmission of nerve-current to the brain, the change induced in the brain during perception and volition, the time for the transmission of the nerve-current to the muscles, on signalling the result, and the time occupied by muscular contraction. The true period of latent stimulation in the case of man must therefore be a very small fraction of a second. In order to attempt a solution of this problem a chronograph made by Dr. König, of Paris, was employed. A diagram of the recording portion of the instrument is given above. The experimental arrangements were as follows : the galvanometer, the eye apparatus, and the chronograph being in separate rooms, one observer was stationed at the galvanometer for the purpose of signalling the moment the needle worked, which was recorded by one of the markers D in the diagram, the other marker being used to register the time of initial action.

The *first experiment* was to transmit at a known moment, through the eye circuit in the dark room, a quantity of current equal in amount to the electrical variation produced, when the eye was stimulated by a flash of light from a vacuum tube, and to record the difference of time between the origin of the current and the observer's signal from the galvanometer.

The *second experiment* was to flash a vacuum tube at a known moment in a room where the eye was placed, and to record as before the instant the galvanometer was effected. From the first observation we ascertain the minimum amount of time necessary to overcome the inertia of the instrument, the observer's personal equation, and the signalling under the conditions of the experiment. If this result is subtracted from the record of the second observation, the difference will represent the latent period of light stimulation. From a large number of experiments made on the eye of the frog we have found the latent period amounts to less than $\frac{1}{10}$th of a second, but its absolute value must be ascertained by some method not liable to the variations that are inevitable to the process described. Altogether the problem is one of great difficulty, but further investigations are in progress. J. D.

ASTRONOMICAL BIBLIOGRAPHIES

PROF. HOLDEN, of the Observatory of Washington, U.S., lately read to the Washington Philosophical Society a paper on "Special Astronomical Bibliographies," in which a careful comparison of one section of the "Reference Catalogue of Scientific Papers," by Mr. E. B. Knobel, F.R.A.S. (lately mentioned in these columns), was made with a MSS. catalogue on the same subject (nebulæ and clusters) which Prof. Holden had compiled for his own use. Only one misprint was found (*op. cit.*, p. 377, for *Comptes Rendus*, vol. 28, p. 537, read p. 573) and only one omission of a highly important paper, the "Siderum Nebulosorum Observationes, Havniensis" of D'Arrest. The list of works given by Mr. Knobel might be still further extended, but in its present state it is very accurate and extremely useful.

A list of bibliographical works available to the astronomer for consultation is given, which we extract:—

1. WEIDLER, "Bibliographia Astronomica," &c., 1755, 8vo.
2. SCHEIBEL, "Astronomische Bibliographie," &c., 1789-98, 8vo.
3. LALANDE, "Bibliographie Astronomique," &c., 1803, 4to.
4. REUSS, "Repertorium Commentationum," &c., vol. v. 1804, 4to.
5. YOUNG, "Natural Philosophy," vol. ii. pp. 87-520, 2 vols., 1807, 4to.
6. SOHNKE, "Bibliotheca Mathematica," &c., 1854, 8vo.
7. [SCHUMACHER], "Catalogue des Livres Composant la Bibliothèque de Feu," Prof. Schumacher, Part 1, 1855, 8vo.
8. STRUVE, "Catalogus Librorum in Bibl. Spec. Pullovensis," 1858, 8vo.
9. ROYAL SOCIETY, "Catalogue of Scientific Papers," 6 vols., 1867-72, 4to.
10. DARBOUX et HOUEL, *Bulletin des Sciences*, &c., 8vo. (*serial*).
11. POGGENDORF, "Biog.-Liter. Handwörterbuch," 2 vols., 1863, 8vo.
12. R. WOLF, *Sonnenflecken-Literatur*, *Astr. Mit.*, 8vo. (*serial*).
13. R. WOLF, "Handbuch d. Mathematik," &c., 2 vols., 1872, 8vo.
14. CARL, "Die Principien d. astr. Instrumentenkunde," 1863, 8vo., p. 161 [Literature of Micrometers].
15. BELGIUM ACADEMY OF SCIENCES, "Bibliographie Académique," 1875, 8vo.
16. ST. PETERSBURG ACADEMY OF SCIENCES, "Tableau Général," &c., Part 1, 1874, 8vo.
17. ENGELMANN, "Literatur d. astronomische Nachrichten," &c., Bessel's Abhandlungen, 1876, 3 vols., 4to.
18. KNOBEL, "Reference Catalogue of Astronomical Papers and Researches." *Mon. Not.* R.A.S. 1876, Nov.
19. HOLDEN, "Index Catalogue of Works on Nebulæ and Clusters of Stars, 1876," MS.

Almost any desired paper may be at once found by means of these works, and Nos. 3, 4, 8, 9, 11, 16, 17, and 18 are practically indispensable.

MENDELEEF'S RESEARCHES ON MARIOTTE'S LAW

IN compliance with your request I hasten to make known, through your esteemed journal, to the scientific public of England, the results of my researches on the Boyle-Mariotte Law.

Special reasons which I have explained in the Russian *Journal of Artillery* (August, 1872), urged me to undertake new researches on this law. They are briefly as follows:—

1. It is impossible to admit in theory that under pressures infinitely great gases can be condensed into a volume infinitely small; or, in other words, that it is possible to introduce into a given volume an infinitely great mass of condensed gas. Although infinitely great pressures be practically unrealisable they are perfectly perceptible to the mind; as to an infinite condensation of matter it is quite inconceivable, otherwise we must admit the existence of an atomic substance without volume. The experiment of Cagniard-Latour developed and verified by MM. Wolf Drion, Andrews, and myself lead us to an inevitable conclusion, viz., that at a certain known temperature all gases and vapours can no longer be transformed under any pressure into liquid, but remain in the gaseous state, endowed with elasticity, but deprived of cohesion. At a lower temperature the gas may be transformed into liquid; but at a higher temperature it remains gas, whatever be the pressure. I gave to this temperature (*Lieb. Ann.*, t. 119, p. 11) in 1860 the name of "temperature of absolute ebullition;" in 1872 Dr. Andrews gave it the name of "critical point."

Imagine a mass of gas at a temperature higher than this, and suppose that this mass is subjected to pressures always increasing; if the Boyle-Mariotte Law is accurate the volume ought to diminish in inverse proportion to the pressure. If we represent this same mass of gas at a temperature a little less than that of absolute ebullition, the gas transformed into liquid will cease to be compressed as before, as the Boyle-Mariotte Law requires. Consequently, there is no doubt that we ought to come to the paradoxical conclusion:—A gas can be more compressed than a liquid or solid. As there is reason to believe that oxygen at the ordinary temperature is hotter than at its temperature of absolute ebullition, the Boyle-Mariotte Law being admitted, under a pressure of 2,000 atmospheres, we ought to find a specific weight of oxygen greater than that of sulphuric acid, and the pressure being 10,000 atmospheres, its density would reach that of mercury. But it is impossible to admit this, judging from what we know of the relation which subsists between the atomic weight of the elements and their density in the free state, as well as in their combinations with other elements. It is sufficient to indicate that a very great density is the peculiar property of combinations, the elements of which are endowed with a considerable atomic weight. Consequently, it cannot be admitted that elements having so small an atomic weight as oxygen can be condensed to any very considerable degree, no matter in what state. We must then conclude, *à priori*, that under high pressures the Boyle-Mariotte Law is inapplicable.

2. The researches of Rumford, which date from last century, relating to the density of the combustion gases of powder; also the researches of M. Natterer, on the compressibility of gases like oxygen, the oxide of carbon, hydrogen, and air, made in the years 1850 *et seq.*, are completely in accordance with the conclusions stated above, and show that under high pressure gases are endowed with a positive compressibility analogous to that of solid or liquid bodies. I mean by positive compressibility that property by which bodies, in proportion to the increase of pressure, diminish in volume less rapidly than the pressures increases. In cases where the Boyle-Mariotte Law is found exact, the product pv of the pressure p into the volume v remains constant; and $\frac{d(pv)}{dp} = 0$. When the compressibility is positive this product pv increases together with the increase of the pressure, and consequently $\frac{d(pv)}{dp} > 0$.

3. At the same time MM. Despretz, Regnault, and many others, have conclusively demonstrated that gases like carbonic and sulphuric acid, which are transformed into liquids under considerable pressure, possess a negative compressibility, so that for them $\frac{d(pv)}{dp} < 0$. M. Regnault has found the same negative compressibility for air, nitrogen, and the oxide of carbon under pressures higher than that of the atmosphere as far as thirty. It necessarily follows that if the data of MM. Regnault and Natterer are correct, the negative compressibility of air becomes under a certain pressure above thirty atmospheres, equal to zero, and therefore positive. When $\frac{d(pv)}{dp} = 0$, Mariotte's Law is verified. Consequently there is a certain pressure above thirty atmospheres under which Mariotte's Law is applicable; under pressures below that point the compressibility is negative; under pressures above the same, it becomes positive and remains so to the end.

4. Although there is no doubt that the Boyle-Mariotte Law is not rigorously applicable even under moderate pressures, yet the prevailing doctrine, so rich in instruction on the nature of gases, which deduces all their properties from the *vis viva* which animates their molecules, admits the supposition that in the rarefaction of gases the distances between these molecules increase to

such an extent that their mutual attraction is destroyed; and in this case gases comply exactly with the Boyle-Mariotte Law. On this hypothesis the law becomes a limit towards which every gas tends in proportion as the distance between its molecules increases, and in proportion as their *vis viva* and the rapidity of their motion increase. That idea finds no support in facts. If it were accurate, then, at a certain high temperature, gases, especially those whose density is not great and whose particles are endowed with a very great rapidity of movement, ought to conform rigidly to the Boyle-Mariotte Law; but this is inconceivable, leading, in fact, to the paradox examined above. Moreover, the researches of M. Regnault on hydrogen, the lightest of gases, have shown the very opposite, hydrogen being positively compressible under pressures only a little higher than that of the atmosphere. It indubitably follows that in reference to the diminution of a mass of a gas filling a certain volume and the increase of the rate of movement of the molecules, we cannot expect a rigorous compliance with the Boyle-Mariotte Law. But we must make certain of finding positive errors, and the following are the grounds which urged me to have recourse to new experiments in reference to the application to gases of the law with which we are dealing.

These grounds were explained by me in 1872; since then similar ideas have been published by many others. Nevertheless, so far as I know, these ideas are far from having been generally adopted into science. It is evident that to combat an established opinion, a single *à priori* conclusion is insufficient; new researches are necessary, all the more that doubt may arise in the mind on the accuracy of the data of experiments, not only such as those of Rumford and M. Natterer, but even those which the celebrated experiments of M. Regnault have established. On examining critically the processes of the last-named eminent experimenter, we may not be able to discover any cause to explain the positive deviations which he has obtained for hydrogen, although it is possible to admit that the negative deviations depend on some defects in experimenting. New experiments and researches thus became indispensable; above all, for the purpose of verifying the data of MM. Regnault and Natterer, following methods not less precise than those which these observers employed. Thus it was specially necessary to experiment on the compressibility of gases under pressures less than that of the atmosphere, seeing that until 1872 there had been no accurate researches on this point.

I distributed as follows the work undertaken by me in 1872:— I commenced with pressures less than the pressure of the atmosphere, and I passed from that to pressures which exceeded those employed by M. Regnault. For the latter purpose I devised in 1872, and have now constructed, a compound manometer, containing alternate columns of mercury and of water, and permitting the measurement of exceedingly great pressures by means of a large number of very low columns of mercury. However, at present I shall not dwell upon this side of the researches, seeing that the experiments are still being carried on; I shall only endeavour to explain the main points of the practical processes, and the results obtained under small pressures. The first experimental researches made by me on the compressibility of air under pressures less than that of the atmosphere were made by means of very simple apparatus. Imagine a vessel A terminated above and below by tubes. The upper tube is always in communication with a syphon barometer, or, as I call it, a baro-manometer. In this apparatus the height of the column of mercury measures the elasticity of the gas in the vessel. It is easy to make the volume of gas in the baro-manometer remain the same all the time, notwithstanding the variety of pressures. To accomplish this it is only necessary to arrange so that we may at pleasure increase or diminish the quantity of mercury in the baro-manometer. The lower tube of A serves to introduce and to withdraw the requisite quantities of mercury. It is not necessary to see the height of the mercury in the vessel; the mercury here serves only to measure the volumes, and consequently, if we close the feeding-tube by means of a cock, and if by means of an emptying tube we allow all the mercury in the vessel to escape, we may ascertain the capacity of the whole reservoir; by emptying only a part of it we may ascertain the volume of the gas at each moment. Thus the weight of the mercury, directly observed by means of a balance, gives immediately, in all my researches, the volume occupied by the gas in each particular case. The first experiments made in 1873 on air, showed me that air under pressures lower than that of the atmosphere possesses a positive compressibility, and that the smaller the pressure the more are the divergences presented by air from Boyle's Law increased. The first apparatus was constructed in a very simple manner; some errors might be suspected, and this is why I am not confident of the results obtained. I therefore constructed a second and a third apparatus, modifying successively not only the dimensions, but also the very construction of the details of the apparatus. I then arranged a fourth apparatus, by means of which, with Michel Kirpitcheff, whose death is a sensible loss to Russian science, I made numerous observations. The report of these experiments was given by me in 1874 to the Russian Chemical Society, and printed in the *Bulletin* of the St. Petersburg Academy of Sciences. The experiments themselves are described in considerable detail in vol. i. chap. 9, of my work "On the Elasticity of Gases." It is impossible to describe in an article the great number of particulars which belong to these researches; we are limited to the more important details which I have introduced into this inquiry, as also into my subsequent investigations.

The normal metre and normal kilogramme which I employed were compared with the Paris standards at the Conservatoire des Arts et Métiers in concert with M. Tresca; their sub-divisions were then carefully verified.

I had to work a long time at the construction of the barometer, and I found out a new process for constructing this apparatus, which consists specially in terminating the end of the barometric chamber by a capillary tube bent downwards. By means of this tube it is possible to expel the last traces of gas which remain in the vacuum, and thus to show the experiment of obtaining an absolute vacuum, *i.e.*, to construct a barometer such that with the diminution of the volume of the chamber the indications do not vary. The construction of two barometers with a common chamber and a single descending capillary tube affords an easy means of obtaining with the greatest precision the determination of the feeblest tensions in the barometric chamber. It is only necessary to direct the telescope of the cathetometer to the top of the column of mercury in one of the barometers when the mercury in the other is at its maximum height, and when the volume of the vacuum is very small; then pouring out the mercury contained in the other barometer, and thus diminishing the pressure which acts upon the vacuum, we may increase its capacity. Then the least quantity of gas contained in the vacuum will give an increase of height in the barometer observed. By constructing the barometer with the greatest care, and filling it with mercury distilled according to Weinhold's process, it is possible, as our numerous researches with M. Hemilian have proved, to obtain a perfect barometer requiring no correction for the tension of air which may remain in the vacuum. This result is obtained solely by means of the capillary tube referred to above. The process shows, moreover, the possibility of constructing barometers without boiling the mercury and without removing them from the position which they are ultimately to occupy. Here then is an undoubted improvement in the construction of an apparatus so important as the barometer, in a great number of physical researches.

Next a very long time and a great number of trials were necessary in order to attain the desired accuracy in measuring heights. I always employ the comparative method, consisting in placing beside the height to be measured a standard metre, well tried beforehand in all connections. My normal measures are generally in the form of tubes, in the inside of which is introduced water which enables me to appreciate at each moment the temperature of the measure, and if necessary even to change it. The telescopes of all my cathetometers are fitted with micrometer eye-pieces, carefully constructed by our engineer, M. Brauer, justly noted for his long residence at Pulkova, and for the construction of a great number of astronomical and magnetic apparatus. Besides the central cross-wire the micrometer eye-piece is fitted with one movable wire, or still better, with two movable wires. The fixed wires passing by the optical and geometrical centre of the telescope fitted with a level sensible to about 2–3″ are directed towards a point of the object whose height is to be determined. Then both telescopes fastened to the same cathetometer, or better to two small separate cathetometers, are directed to the normal measure arranged at the side, and the double movable wire serves to determine the distance of the fixed wire from the nearest lines of the normal measure. This last is placed at such a distance from the column whose height is being determined, that the measure and the object may be distinctly visible without changing the position of the eye-piece. Every variation in the position of the eye-piece may derange the position of the optical centre; this is why latterly I use exclusively cathetometric telescopes, in which the distance of the eye-piece from the objective cannot undergo any change. On the other hand, it is possible to move the lunette if the cathetometer itself is in the rest where it is fixed; which is not seldom necessary in practice. By using a considerable magnifier and an illuminator of the columns of mercury very carefully combined, it is possible to observe the columns with a precision carried to thousandths of a millimetre; so that the error in appreciating the height does not exceed 0·01 mm. Everyone who has worked with the ordinary cathetometers and who has used their scales for measuring heights, knows that the accuracy of the measurement by means of such apparatus never exceeds $\frac{1}{25}$ mm., and that often he makes errors which reach tenths of a millimetre. It is sufficient to refer to the variations of temperature infallibly due to the presence of the observer. In the construction applied by me, these reasons, as well as many other causes of error, do not exist at all.

Although for the barometers and the baromanometers I always use tubes of large diameter, exceeding 17 and very often even 20 millimetres, nevertheless I have thought it proper to verify the capillary depression of the mercury depending on various diameters of the tube and various heights of the meniscus. A very extensive research has been made in my laboratory by Mlle. Goutkovsky, and the results which she has obtained have obliged me to change the data which we possess on the depression of mercury. I cite one example from many which are in my work on "The Barometric Levelling and on the Application of the *Syssotomer* to that purpose." The diameter of the tube being 8·606, and the height of the meniscus—

0·6 0·8 1·0 1·2 1·4 millimetre,

the depressions are—

0·162 0·235 0·312 0·380 0·458 „

numbers differing from those generally adopted, according to which for a height of the meniscus 1·0, there ought to be a depression of 0·460 for the diameter 8·606.

DE MENDELEEFF

(*To be continued.*)

OUR ASTRONOMICAL COLUMN

THE SUSPECTED INTRA-MERCURIAL PLANET.—M. Leverrier has issued an ephemeris of positions of the hypothetical planet, interior to Mercury, derived apparently from the two orbits to which reference was made last week as representing the observations upon which the general formula was founded, with equal precision, and if the planet should not be met with in transit across the sun's disc between March 21 and 23, use may be made of M. Leverrier's ephemeris to examine with large telescopes the positions of the greatest elongation westward in the two orbits. The differences of right ascension and declination from the sun about these times are thus given:—

	ORBIT I.		ORBIT II.	
	Diff R.A.	Diff. decl.	Diff. R.A.	Diff. decl.
	m.	°	m.	°
March 28	− 38·4	− 2·6	− 38·8	− 2·6
29	− 40·4	− 2·6	− 42·0	− 2·8
30	− 40·4	− 2·8	− 44·8	− 2·9
31	− 39·2	− 2·6	− 45·2	− 3·0

The observation of Decuppis at Rome in 1839, one of the five utilised by M. Leverrier, was communicated to the Paris Academy of Sciences on December 16 in the same year. It is thus noticed in the *Comptes Rendus* of that sitting: "M. Decuppis announced that on October 2, while continuing the observations which he has made upon the spots of the sun, saw a black spot, perfectly round, and with well-defined contour, which advanced upon the disc with rapid motion, so that it would have traversed the diameter in about six hours. M. Decuppis thinks that the appearances which he has observed can only be explained by admitting the existence of a new planet." The observation is reproduced here, as it appears to have escaped the notice of several writers who have recently entered upon this subject. Haase mentions it, but does not give particulars.

The observation by Mr. Joseph Sidebotham at Manchester, on March 12, 1849, was communicated to the Literary and Philosophical Society of that city, April 1, 1873, and will be found in the *Proceedings*, vol. xii. p. 105. "A small circular black spot" was "watched in its progress across the disc for nearly half an hour," by Mr. Sidebotham and Mr. G. C. Lowe, also a member of the same society.

D'ARREST'S COMET.—If this comet is not detected before moonlight interferes in the mornings, it may probably be observed in the middle of the ensuing month, where the sky is very transparent down to the eastern horizon; it will then rise rather more than two hours before the sun, and the intensity of light will be greater than when it was last seen by Prof. Schmidt at Athens in December, 1870; still its distance from the earth will be considerable (1·7). When theoretically brightest, in May, observations may be made at the observatories of the southern hemisphere. At the Cape, Melbourne, and Sydney, the comet will rise more than four hours before the sun; the perihelion passage takes place on May 10. The following positions will sufficiently indicate its course about that time:—

At Greenwich Noon.

	R.A.	N.P.D.	Distance from Earth.
	h. m. s.	° ′	
May 2	23 18 16	91 17·0	1·670
6	23 32 13	90 31·9	1·659
10	23 46 5	89 47·8	1·650
14	23 59 51	89 4·8	1·642
18	0 13 28	88 23·3	1·635
22	0 26 56	87 43·6	1·629
26	0 40 14	87 5·8	1·624

The intensity of light remains sensibly the same during this period. In August and September next observations may be practicable with very powerful instruments, as the comet moves from Taurus into Orion.

According to the elements of M. Leveau, who has continued the investigations on the motion of D'Arrest's comet, commenced on its first discovery in the summer of 1851 by M. Villarceau, the dimensions of the orbit in 1877 are as follow:—

Semi-axis major	3·5414
Semi-axis minor	2·7565
Semi-parameter	2·1456
Perihelion distance	1·3181
Aphelion distance	5·7647

The period of revolution in the ellipse of 1877 is 2434·2 days, or 6·664 years; it has been lengthened 104 days since 1851, by the effect of perturbation from the action of Jupiter, the principal disturbance of its motion having taken place in the spring of 1861, when the comet approached the planet within 0·36 of the earth's mean distance from the sun.

TOTAL SOLAR ECLIPSES.—It might be worth while to collect together and discuss the various notices of the total solar eclipses of 1386, January 1, and 1415, June 7, in the same manner that Prof. Schiaparelli and M. Celoria have done with the eclipses of 1239 and 1241. The eclipse of 1415 in particular was a very notable one from the large excess of the moon's augmented diameter over the diameter of the sun; as Baron de Zach states, "plusieurs historiens et presque tous les astronomes en ont parlé." Both eclipses were total at Montpellier, not a common occurrence at a particular place in an interval of only twenty-nine years.

METEOROLOGICAL NOTES

MEAN ATMOSPHERIC PRESSURE IN RUSSIA IN EUROPE.—A paper on this subject, by M. Rikatcheff, appeared some time ago in the *Repertorium für Meteorologie*. The work is based on monthly averages for various terms of years for thirty places in Russia, to which are added the averages for thirty-three places situated in other parts of Europe. A valuable part of the paper is that which gives the details of the observations at each place, as regards the errors of the instruments employed and the heights above the sea, so far as known. The heights of places not yet determined trigonometrically are approximated to barometrically by a comparison with other stations whose heights are known. From these data the monthly and annual isobars for each millimetre (0·039 inch) are drawn on thirteen maps. It is to be regretted that so much work, characterised not only by general accuracy, but also by an attention to minute accuracy of detail in certain directions, can only be regarded as to a great extent thrown away, at least in so far as regards the inquiry in hand, viz., the representation of the facts of atmospheric pressure in Russia, as that pressure varies by latitude and season, in their relation to configuration of surface and the relative distribution of land and water. The author has failed to see that, in order to give a satisfactory solution of this problem, one of the first requisites is that the observations at the different stations be for the same terms of years, or be reduced to the same terms of years, by the process of differentiation. As regards the thirty Russian stations, the averages are for periods varying from seven to fifty years, and excepting Lugan and Catherinenburg, no two places are for the same terms of years. As regards the months the result of this method of discussion is great unsatisfactoriness. Thus at several places where the averages are only for a few years, they not unfrequently are very different from the isobars which have been drawn for the districts where they are situated. Still further, the anomalous directions of several of the isobars, such as the isobar of 759 millimetres for March, cannot be accounted for by the physical peculiarities of the region traversed by the anomalous portion of the curve; but an examination of the facts suggests that the anomaly is probably due to the simple circumstance that exceptionally high or low monthly means of particular years are included in the averages of some stations, whilst at other neighbouring stations observations were not made during these exceptional months. The annual isobars are necessarily more satisfactory. It may, however, be noted that if allowance be made for the correction for gravity, according to latitude, which has been employed, a correction which for several reasons is objectionable, the annual isobars for Russia are substantially the same as those published by Mr. Buchan, even though these were confessedly a first approximation, giving only the broad features of the distribution of atmospheric pressure over the globe. Much more is now required than this, seeing that the data since acquired would enable us to draw the isobars with a precision sufficient to show not merely their general change of position with season and latitude, but also the exact forms impressed on the curves by their position with reference to large masses of land and water. In solving this problem, what is required from Russia are tables of the monthly means of each year during which observations have been made at each station, corrected for instrumental errors now ascertained—tables, in short, similar to those published by Dr. Buys Ballot for many places in Europe, in the *Annals* of the Dutch Meteorological Institute for 1870.

METEOROLOGY OF MAURITIUS—The *Mauritius Meteorological Results* and *Meteorological Reports* for 1874 and 1875, have been received, which are deserving of special notice from the increased vigour and efficiency with which they show meteorological research to be prosecuted in that part of the globe. In addition to the usual elaborate summaries, the *Results* for 1875 contain a noteworthy addition in the form of two Tables, one giving the hourly means of the atmospheric pressure of the months during 1875 deduced from the barograph curves, and the other the same means from the term-day observations made at the observatory from 1853 to 1871. Tables showing the hourly readings for each day were prepared but are not printed in the *Results*. If this be due to want of funds to meet the expense of publication it is to be hoped that the difficulty will be got over in next publication, on account of the great value of such hourly readings in many meteorological inquiries, but more particularly in connection with the gales and hurricanes of the Indian Ocean, which are so carefully detailed by Dr. Meldrum in the *Results*. The examination of these readings and the hourly observations of the wind could not fail to suggest conclusions of the utmost value in their bearings on systems of storm warnings for tropical countries such as we recently sketched in NATURE (vol. xv. p. 261) for the Bay of Bengal. In the *Annual Report* for 1875, it is stated in the course of a discussion on sunspots and rainfall, that since the photoheliograph has been in use at the Observatory the sunspots have been compared with the daily weather, and that, so far as the observations have gone, the results are in conformity with those for longer periods, both the rainfall and the velocity of the wind having been greater when the spots were most numerous. This increase of the velocity of the wind with an increase of sunspots is a point of first importance when viewed in connection with Mr. Lockyer's suggestion that increased sunspot area implies increased solar radiation, with Mr. Blanford's confirmation of this idea from an examination of the results of the solar radiation thermometers in India, and with the result arrived at by Mr. Clement Ley, showing that with like conditions of pressure the wind's velocity is greatest during those months of the year when temperature is highest.

EXPLORING BALLOONS FOR METEOROLOGICAL PURPOSES.—Since the beginning of February, M. Secretan, the optician of the Pont-neuf, in Paris, has been sending up regularly every day at noon small exploring balloons for the purpose of ascertaining the direction of the several streams of air and the height of clouds. The results are daily published in the *Petit Moniteur*. The balloons are given gratuitously by the *Grand Magazin du Louvre*, and are of india-rubber filled with pure hydrogen. The diameter is ninety centimetres. M. de Fonvielle finds by calculation and by several experiments, that the mean velocity of elevation is about four metres per second. Hence to obtain the altitude of the clouds it is sufficient to observe the balloon with an opera-glass, to count the number of seconds necessary to lose sight of it owing to the opacity of the clouds, and to multiply the number of seconds by four. It was found that the altitude of clouds varies from 400 to 800 metres, and prospects of fair weather are increased in proportion to the elevation of clouds. The clouds follow the direction of an aërial stream in which they are wholly immersed, and are not placed, as has been repeatedly stated, at the surface of separation. The direction of the air for the first 100 metres is almost always very uncertain and varies according to unknown causes. This shows that anemometers give a very poor idea not only of the velocity but also of the direction of prevailing winds, and that no real progress is to be expected in the knowledge of atmospheric calculation as long as meteorologists confine themselves to taking into account anemometrical observations. Very often two different streams of air are observed, the lower one extending from 100 to 200 or 300 metres; under these circumstances the weather seems to be particularly uncertain and unsettled. Meteorologists, we think, might make use of this method of observation with great advantage.

BIOLOGICAL NOTES

A CHYTRIDIUM WITH TRUE REPRODUCTION.—Botanists are indebted to Dr. L. Nowakowski for a memoir on *Polyphagus euglenæ*, in which they will find recorded for the first time the whole life-history of one of the most interesting of the group of vegetable parasites known as Chytridia. First described in 1855 by Bail, who was a pupil at Breslau of the illustrious F. Cohn, this species has now had all the mysteries of its life cleared up by the researches of Nowakowski, studying at the same university and under the same master. The Euglenæ on which it is parasitic will be well known to microscopists as a group of flagellate Infusoria, at one time found as freely-swimming forms, and at another passing into a resting stage. It is at this period of their existence that the Polyphagus attacks them. The minute spores are furnished with four or more delicate filaments, which project from the body of the spores like rays. One or more of these soon comes into contact with a Euglena, bores through its integument, and penetrates into its protoplasmic contents; it now becomes a haustorium, increases in size, often sends off other filaments, which go on the search for other specimens of Euglenæ; in the meanwhile the body of the spore grows apace, and, if its haustoria be only fairly successful in catching Euglenæ, soon increases to considerable (microscopical) dimensions, and in course develops into a pro-sporangium. Next a little bladder-like projection is seen slowly forcing its way out from this latter, and at last becomes developed into a zoosporangium, from which in time issue the cloud of zoospores, and so after a well-known fashion the vegetative development of this parasite is carried on. The presence of a true reproduction is, however, the great fact in the memoir. Among the individuals of Polyphagus developing in the interspaces of the dead Euglenæ will be found *two* forms; one larger than the other, and generally spherical in shape, is the female plant; the other, small and more or less club-shaped, is the male plant. From the former there is a tube-like prolongation which passes into a haustorium; from the latter there are several haustoria; these remain thread-like if they encounter no Euglenæ, or enlarge when they do. These two unicellular plants then conjugate, but after a somewhat strange and novel manner. The protoplasmic contents of the female plant project through an opening in the cell wall, forming slowly an oval mass (gonosphere), with the which a haustorium from a neighbouring male plant, coming into contact, there is a commingling of the contents of the two plants, and thereby a zygospore is produced; sometimes these have a quite smooth covering, at other times they are rough, with minute prickles. After a little rest the zygospore develops a zoosporangium, from which issue swarm-spores, and the cycle is complete. As the result of these investigations, the author would place the Chytridia forms in the group of the Siphomycetes. It will be observed that though the whole contents of two cells go to form the zygospore, yet that the difference in the size of these cells is very marked, and that the behaviour of the gynœciel cell reminds one of what takes place in an oospore. (Cohn's *Beiträge zur Biol. der Pflanzen*, Bd. ii. Heft 2, 1876.)

CRYPTOGAMIC FLORA OF RUSSIA.—We notice the appearance of the first fasciculus of an important Russian work, by M. Sredinsky, being a Catalogue of Russian Cryptogams. The work will be divided into five parts: Vascular Cryptogams, Musci, Lichens, Fungi, Characeæ, and Algæ, each part to appear in several separate fascicules. The first fasciculus is a description of the Vascular Cryptogams of Southern Russia, Transcaucasia, and the neighbourhood of St. Petersburg. Much valuable material, collected by Russian botanists, is already in the hands of the author, and many members of the St. Petersburg Society of Naturalists have promised to supply him with much additional material for his valuable work.

ALGÆ OF THE GULF OF FINLAND.—At the last meeting, February 28, of the St. Petersburg Society of Naturalists, M. Gobi made an interesting communication on the Algæ of the Gulf of Finland. They are not numerous and have migrated from the Atlantic Ocean. Towards the east the red Algæ become rare, and all diminish in number and size. It must be observed also that the red Algæ of the Gulf of Finland have almost nothing in common with those of the White Sea, which circumstance is an argument against the existence of a former communication between the Baltic and the White Sea advocated by some geologists, but more and more discountenanced by the latest explorations. Describing in detail the most important forms of red Algæ of the Gulf of Finland, M. Gobi exhibited a complete collection of them, together with a series of drawings and of microscopical plates from the same.

BOTANICAL GEOGRAPHY OF RUSSIA.—The seventh volume of the *Memoirs* of the St. Petersburg Society of Naturalists contains a most valuable contribution to the botanical geography of Russia, by M. Gobi, "On the Influence of the Valdai-plateau on the Geographical Distribution of Plants, with a Sketch of the Flora of the Western Part of Novgorod Government." The author begins with a detailed description of the orography of the region, of its geological structure, its soil and subsoil, its marshes, lakes, &c., and deals at length with the climate of the country. Further, after a review of former botanical works dealing with the same region, he gives a list of plants growing on the plateau (615 Phanerogams). The fourth chapter is devoted to a delineation of the main topographico-botanical subdivisions of the flora; and the fifth is a detailed discussion of the relations existing between the flora of the plateau and those of neighbouring tracts. After some general remarks the author traces here the boundaries of the regions occupied by about fifty plants, which boundary-lines run either across the plateau or along its slopes. The intrusion of these plants from the north, north-east, and south is graphically shown on three maps accompanying the paper.

NOTES

WE understand that the Council of the new University College, Bristol, intend shortly to appoint a Principal of the College. We presume that the claims of science will be well considered in the appointment, as the movement to which the college owes its origin took its rise in the desire to found a school of science for the West of England and South Wales. In the interests of the higher scientific and literary education, we hope that the Council may be successful in securing the services of an eminent man for so important a post.

AT the monthly meeting of the Council of the University College of Wales, one of the governors present expressed his intention to give 200*l*. a year for three years, to be applied in such form as the Council may deem best in connection with the college for the encouragement of scientific agriculture.

THE President and Fellows of the Chemical Society dined together at Willis's Rooms on Tuesday evening, the company numbering about 200, and including some of the most distinguished names in science. Prof. Huxley, in responding to the toast of the Learned Societies, pointed out that most of the younger London scientific societies are offshoots, or "buds," of the Royal Society; the latter, he maintained, now more than at any other time, needed sympathy and support. Prof. Huxley alluded with some humour to the extraordinary claims put in by some applicants for a share of the government grant, one gentleman alone having asked for 3,000*l*. out of the 4,000*l*. Some of the applicants reminded him of the Irishman who requested government to give him an appointment in any capacity in Church, Army, Navy, or Civil Service, his sole qualifications, the applicant confessed, being an inexhaustible fund of animal spirits and a keen sense of humour.

AN influential meeting was held at the Mansion House on Tuesday in support of the erection of an Imperial Museum for India and the Colonies, to which scheme we have already referred in detail. The proposal met with the warm approval of the meeting, and it was resolved that steps should be taken to move in the matter, and have a building erected on the Thames Embankment, on the site of the now demolished Fife House.

THE *Morning Post* of March 15 contains an article on the present state of the Loan Collection of Scientific Apparatus, and enumerates a number of collections that after the removal of foreign loans still remain to form the nucleus of a permanent museum. It points out that though the galleries have had to be closed in consequence of packing, the lectures have kept up the continuity of the scheme, and the apparatus forming the subject of the lectures have been brought into the lecture theatre as wanted. It adds, "There seems a fair probability that the nucleus of the permanent collection can be thrown open early in May."

THE obstacles hitherto presented to the medical education of women in England appear suddenly to have collapsed. The enabling Act of last session, introduced by the Right Hon. Russell Gurney, which permitted any licensing body to examine women for its diplomas, was first of all accepted by the Queen's University for Ireland and the Royal College of Physicians for Dublin. The example of these bodies has been speedily followed by the University of London. At a recent meeting the Senate reversed its decision of two years ago, and decided, by a majority of fourteen to eight, to admit women to its medical degrees. Among the majority are found the names of two of the most eminent medical men in London, who supported the motion on the ground that it was the duty of the University to give effect to the resolution arrived at by the Medical Council and by Parliament, that women should not be debarred from entering the profession. Since the matriculation examination is the sole avenue to all degrees in the University, this examination is now thrown open to women who present themselves with the intention of following it up by a course of medical studies. All these concessions to the friends of the medical education of women were, however, but barren victories as long as the hospitals closed their doors against the admission of female students to clinical instruction. Every hospital in London to which a medical school is already attached has refused this permission; and one chance only remained. The Royal Free Hospital in Gray's Inn Road is a general hospital containing the maximum number of beds required by any licensing body, and free from the difficulty of having already attached to it a school of male students. At the instance of the London School of Medicine for Women, the subject was last week brought before the Governing Body of this hospital, and a resolution was unanimously passed that, since they were the only body in London in a position to grant this privilege, it was their duty to throw open their hospital to female students. This decision, due mainly to the untiring exertions of the Treasurer to the London School of Medicine for Women in Henrietta Street, Brunswick Square, the Right Hon. J. Stansfeld, M.P., has only come just in time to prevent the breaking up of that institution. The executive committee of that school, at which regular courses of lectures in the whole curriculum of medical study have now been given for three years, had determined that, unless they could, before the close of this winter session, announced to the students that there was a prospect of solving the hospital difficulty in London, they must close the school in the summer, and recommend the students to go abroad for their clinical studies. The winter session closes next week, and it was only last Saturday that the announcement was made, in consequence of the decision of the Governing Body of the Royal Free Hospital, arrived at the preceding Wednesday. For the purpose of making the necessary arrangements, the Medical School for Women has entered into heavy engagements of a pecuniary nature, to enable them to fulfil which they will require the liberal support of the friends of the movement. With regard to the University of London, it is felt that the present position of admitting women to its medical degrees only, and to no others, is not one that can be permanently sustained; but any further extension of its privileges can only be effected by a new charter, or by an enabling Act similar to that of last session, applicable to all degrees.

PROF. GARROD completed on Tuesday his course of lectures at the Royal Institution on "The Human Form, its Structure in Relation to its Contour." Although some of the lectures have consisted of anatomical details, illustrated with diagrams prepared for a medical school, the attendances have been large in comparison with those of other Royal Institution courses, and ladies have formed more than a half of the audiences. Prof. Garrod's object was to describe the parts of the structure of the body which affect the contour in such natural attitudes as are commonly portrayed in works of art. Several ingenious working models to illustrate the action of different parts of the body were devised especially for these lectures, and a colossal wooden model of a disarticulated human skeleton was also specially prepared.

M. WADDINGTON has appointed M. Maindron a Chevalier of the Legion of Honour for services rendered to science in the capacity of secretary of the French Transit of Venus Commission. A new volume will be issued very shortly by the French Academy.

TUNGSTATE of soda has been much talked about lately as valuable, when mixed with ordinary starch, for rendering muslin dresses uninflammable. Prof. Gladstone and Dr. Alder Wright have both brought it before audiences at the Royal Institution, Dr. Wright showing its efficacy by having a muslin dress so prepared for one of his assistants to wear, in which he walked about over flames. In repeating the demonstration in the course of a lecture at South Kensington, on Saturday evening, it was fortunate that Dr. Wright had the dress placed on a dummy instead of being worn by an assistant, for no sooner was a light applied to it than it blazed up and was consumed. Why this happened could not be explained, as it is believed no mistake had been made in the preparation. No doubt the exact conditions under which the tungstate is reliable will be a subject for further investigation.

M. REDIER, barometer maker to the French Association for the Advancement of Science, has devised a barometer for warning miners when the atmospheric pressure is undergoing a sudden depression so that they may be on their guard against fire-damp explosion.

RURAL meteorology is progressing rapidly in France. No fewer than 500 parishes receive by telegraph daily warnings from the observatory. The telegrams summarising the readings taken at seven or eight o'clock in the morning (local time) from Constantinople to Valentia, arrive daily at two o'clock in each parish in connection with the observatory. The number of parishes is being daily increased.

EVERYONE knows that the aneroid barometer is composed of a metallic box exhausted of air and kept in a state of tension by an interior spring. A French optician has conceived the idea of substituting for the spring a weight attached to the exterior by a hook underneath.

IN a forcible article in the *Cape Argus*, for January 23, it is shown how much service could be done to farmers and others by giving them timely warning of approaching unfavourable weather. Such warning can only be based on extensive and carefully collected data, involving work which cannot be done for nothing. The *Argus*, therefore, reasonably urges that it is the duty of the Cape Parliament to provide the means of carrying on work that would undoubtedly benefit the whole colony.

SEVEN warnings have been sent to Europe by the Meteorological Office established by the *New York Herald*, since the end of February. Six of the predicted storms were felt in Paris, having crossed the Atlantic with a velocity somewhat less than had been anticipated.

A BRIGHT violet meteor was observed at St. Etienne on March 11 at two o'clock in the morning, in the southern part of the horizon. It was travelling with great velocity from west to east. No detonation was heard.

It is stated that on the 5th instant a rather severe earthquake was felt in the districts of Hällsback, Lerbäck, Bodarne, and Sköllersha, in the county of Nerike, Sweden, a distance of about ten English miles. The shocks were strong enough to shake the houses and make china and even heavier objects tumble down. The earthquake was also felt in the Province of Ostergötland. It is stated also that the island of Mull has been visited by an earthquake, which, although it lasted only a minute, caused much commotion in the island.

The Sedgwick Prize Essays must be sent in to the Registrary on or before October 1, *1879*, not as stated in last week's Nature (p. 439) on or before October 1, *1880*. The award is to be made in the Lent Term, 1880.

At the meeting of the Swedish Academy on February 14, Prof. Nordenskjöld read a paper by Dr. Kjelman, on "The Algæ of the Kara Sea," from which it appears that the sea, contrary to current opinion, is full of algæ, which sometimes attain gigantic sizes. The professor exhibited also photographic views of glaciers of the interior of Greenland, taken in 1870, by M. Berggion. They are true representations of views of the glacial period now prevailing in Greenland.

The *Stockholm Dagbladet* states that recently Prof. Nordenskjöld and the companions of his last travel, Messrs. Kjelman Lundström, Tribom, Stucksberg, and Théel, as well as Mr Oscar Dickson, were entertained by the King of Sweden at dinner, where the question as to Prof. Nordenskjöld's expedition in 1878 was discussed. The king promised to place at Prof. Nordenskjöld's disposal the steamer *Sophia* on the same terms as in 1868, and the pecuniary means for the expedition are promised by Mr. Dickson. The expedition proposes to explore the Arctic Ocean east of the Yenissei as far as Behring's Strait. Some Russian naturalists have applied to Prof. Nordenskjöld to be permitted to take part in the explorations.

Prof. Ahlquist and students Böhm and Bergroth started on February 24 from Helsingfors on their ethnographical journey to the mouth of the Obi (Nature, vol. xv., p. 207).

At the last meeting of the St. Petersburg Society of Naturalists the programme of a botanical excursion to the Fergana province and the Pamir plateau, to be undertaken this year by M. Smirnof, was discussed and agreed to.

We regret to notice the death of Admiral Sir Edward Belcher' whose name is so well known in connection with the Arctic exploration of about a generation ago. Sir Edward was in his seventy-ninth year, and began his naval career sixty-six years ago. He did a great amount of surveying work in various parts of the world. He had almost retired from active work when, in 1852, he was sent out in command of one of the expeditions in search of Sir John Franklin. Although the search was unsuccessful, and the vessel had to be abandoned in 1854, the work then done by Sir Edward Belcher was sufficient to win for him a worthy place among Arctic heroes.

We notice in the seventh volume of the *Memoirs* of the St. Petersburg Society of Naturalists a paper, by M. Alénitzin, on the existence, in the Aralo-Caspian region, of a rise of land in a direction from south-east to north-west, and on the causes of the change of bed of the Amu-darya. Combining some observations relative to the structure of the shores of lakes Aral and Balkhash, the author proves that the south-eastern shores of both lakes have, during the recent geological period, been rising; and he explains by this circumstance the rapid undermining by the Amu of its right bank, at the point where the river turned in former times sharply to the west. This undermining, assisted by a relative rising of the upper parts of the river, resulted in an excavation of a bed directed to the north. Whatever may be thought of his theories the reader will find in M. Alénitzin's paper interesting information on the structure of the shores of both Central Asian interior seas.

The same volume contains an interesting note, by Prof. Féofilaktoff, on the diluvial deposits in Kieff and Poltava governments, containing the general results arrived at by the author during his many years' explorations, the details of which will be found in the *Memoirs* of the Kharkof Society of Naturalists for 1874.

The latest news received by the St. Petersburg Geographical Society from M. Potanin, announces that the expedition was stopped at Khobdo by the arrival of winter. The proposed further route of the expedition is across the great ridge which runs between the Altai and the Khangai, but the masses of snow which usually accumulated in the mountain-passes south of the Djabgan River made any further advance during winter impossible. Staying at Khobdo the expedition will make many important ethnographical observations, and collect information as to the trade of this place. As to Col. Prjevalsky, no news has been received from him, and probably will not for a long time. In November he was at Korleh, on his way to Lob-nor, entering thus on a country which has no communication with Russia. News may be expected only when he returns to Kuldsha, before undertaking his journey to Thibet.

The secretary of the St. Petersburg Geographical Society announces the return to the capital of M. Wojeikoff from his meteorological journey round the world. The countries he visited last were India, Java, and Japan. The visit to Japan was especially interesting, as M. Wojeikoff made an excursion into a part of the interior never visited before by Europeans, and collected very valuable information as to the Ainos tribe. The observations made during the journey will be the subject of communications to the Geographical Society.

At a sectional meeting of the Chester Society of Natural Science held last month Mr. J. D. Siddall read a paper on Foraminifera and other Microzoa in neighbouring limestone rocks, during the course of which he announced his discovery of Radiolarians, first, in the Halkin, and afterwards in the Minera limestones. Mr. Siddall had prepared several polished blocks to illustrate his lecture. Two of these showed specimens of beautifully preserved Radiolarians. Other members of this flourishing society have since obtained similar results by following his method of preparing these interesting microscopic objects. In some of the best pieces most of the types to be seen in thin slices of Barbadoes earth are represented, and in as great abundance. This discovery furnishes a capital example of the rewards which sooner or later follow patient scientific investigations. By this discovery Mr. Siddall has thrown back our knowledge of the distribution of Radiolarians in time from Mesozoic, if no from Tertiary, to Palæozoic formations.

The additions to the Zoological Society's Gardens during the past week include two Orang-outangs (*Simia satyrus*) from Borneo, presented by Dr. R. Sim, F.Z.S.; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. J. Mason Allen; a Grivet Monkey (*Cercopithecus griseo-viridis*) from North-east Africa, presented by Mr. J. Walter Richardson; a Cape Hyrax (*Hyrax capensis*) from South Africa, a Chinese Blue Magpie (*Urocissa sinensis*) from China, a Red-capped Parrot (*Pionopsitta pileata*) from Brazil, three Red-eared Conures (*Conurus cruentatus*) from South America, a Sarus Crane (*Grus antigone*) from North India, an Arabian Baboon (*Cynocephalus hamadryas*) from Arabia, purchased; an American Jabiru (*Mycteria americana*) from South America, deposited; a Common Badger (*Meles taxus*), born in the Gardens.

IRON AND STEEL INSTITUTE

ADDRESS OF THE PRESIDENT, C. WILLIAM SIEMENS, D.C.L., F.R.S.

THE Iron and Steel Institute opened its London Session on Tuesday, and yesterday Dr. C. W. Siemens gave his presidential address.

Dr. Siemens, after referring to the origin and progress of the Institute, touched upon several topics of interest to those connected with the iron and steel industries.

Speaking of Education, Dr. Siemens said :—Intimately connected with the interests of this institution, and with the prosperity of the iron trade, is the subject of technical education. It is not many years since practical knowledge was regarded as the one thing requisite in an iron smelter, whilst theoretical knowledge of the chemical and mechanical principles involved in the operations was viewed with considerable suspicion. The aversion to scientific reasoning upon metallurgical processes extended even to the authors who professed to enlighten us upon these subjects; and we find, in technological works of the early part of the present century, little more than eye-witness accounts of the processes pursued by the operating smelter, and no attempt to reconcile those operations with scientific facts. A great step in advance was made in this country by Dr. Percy, when, in 1864, he published his remarkable "Metallurgy of Iron and Steel." Here we find the gradual processes of iron smelting passed in review, and supported by chemical analyses of the fuel, ores, and fluxing materials employed, and of the metal, slags, and cinder produced in the operation. On the continent of Europe the researches of Ebelmann, and the technological writings of Karsten, Tunner, Gruner, Karl, Akermann, and others, have also contributed largely towards a more rational conception of the processes employed in iron smelting.

It must be conceded to the nations of the Continent of Europe that they were the first to recognise the necessity of technical education, and it has been chiefly in consequence of their increasing competition with the producers of this country, that the attention of the latter has been forcibly drawn to this subject. The only special educational establishment for the metallurgist of Great Britain is the School of Mines. This institution has unquestionably already produced most excellent results in furnishing us with young metallurgists, qualified to make good careers for themselves, and to advance the practical processes of iron making. But it is equally evident that that institution is still susceptible of great improvement, by adding to the branches of knowledge now taught at Jermyn Street, and I cannot help thinking that a step in the wrong direction has recently been made in separating geographically and administratively the instruction in pure chemistry from that in applied chemistry, geology, and mineralogy. If properly supported, the School of Mines might become one of the best and largest institutions of its kind, but it would be an error to suppose that, however successful it might be, it could be made to suffice for the requirements of the whole country. Other similar institutions will have to be opened in provincial centres, and we have an excellent example set us by the town of Manchester, which, in creating its Owens College, has laid the foundation for a technical university, capable of imparting useful knowledge to the technologist of the future.

Technical education is here spoken of in contradistinction to the purely classic and scientific education of the Universities, but it must not be supposed that I would advocate any attempt at comprising in its curriculum a practical working of the processes which the student would have to direct in after-life. This has been attempted at many of the polytechnic schools of the Continent with results decidedly unfavourable to the useful career of the student. The practice taught in such establishments is devoid of the commercial element, and must of necessity be an objectionable practice, engendering conceit in the mind of the student, which will stand in the way of the unbiassed application of his mind to real work. Let technical schools confine themselves to the teaching of those natural sciences which bear upon practice, but let practice itself be taught in the workshop and in the metallurgical works.

After referring to the question of Labour, Dr. Siemens spoke in some detail on that of Fuel. Fuel, in the widest acceptation of the word, may be said to comprise all potential force which we may call into requisition for effecting our purposes of heating and working the materials with which we have to deal, although in a more restricted sense it comprises only those carbonaceous matters which, in their combustion, yield the heat necessary for working our furnaces, and for raising steam in our boilers.

The form of fuel which possesses the greatest interest for us, the iron smelters of Great Britain of the nineteenth century, is without doubt the accumulation of the solar energy of former ages which is embodied in the form of coal, and it behoves us to inquire what are the stores of this most convenient form of fuel.

Recent inquiry into the distribution of coal in this and other countries has proved that the stores of these invaluable deposits are greater than had at one time been supposed.

I have compiled a table of the coal areas and production of the globe, the figures in which are collected from various sources. It is far from being complete, but will serve us for purposes of comparison.

The Coal Areas and Annual Coal Production of the Globe.

	Area in Square Miles.	Production in 1874 Tons.
Great Britain	11,900	125,070,000
Germany	1,800	46,658,000
United States	192,000	50,000,000
France	1,800	17,060,000
Belgium	900	14,670,000
Austria	1,800	12,280,000
Russia	11,000	1,392,000
Nova Scotia	18,000	1,052,000
Spain	3,000	580,000
Other Countries	28,000	5,000,000
	270,200	274,262,000

This table shows that, roughly, the total area of the discovered coal fields of the world amounts to 270,000 square miles.

It also appears that the total coal deposits of Great Britian compare favourably with those of other European countries; but that both in the United States and in British North America, there exist deposits of extraordinary magnitude, which seem to promise a great future for the New World.

According to the report of the Coal Commissioners, published in 1871, there were then 90,207 million tons of coal available in Great Britain, at depths not greater than 4,000 feet, and in seams not less than 1 foot thick, besides a quantity of concealed coal estimated at 56,273 millions of tons, making a total of 146,480 millions. Since that period, there have been raised 600 millions of tons up to the close of 1875, leaving 145,880 millions of tons, which at the present rate of consumption of nearly 132 millions of tons annually, would last 1,100 years. Statistics show that during the last 20 years there has been a mean annual increase in output of about 3½ millions of tons, and a calculation made at this rate of increase would give 250 years as the life of our coalfields.

In comparing, however, the above rate of increase with that of population and manufactures, it will be found that the additional coal consumption has not nearly kept pace with the increased demand for the effects of heat, the difference being ascribable to the introduction of economical processes in the application of fuel. In the case of the production of power, the economy effected within the last 20 years exceeds 50 per cent., and a still greater saving has probably been realised in the production of iron and steel within the same period, as may be gathered from the fact that a ton of steel rails can now be produced from the ore with an expenditure not exceeding 50 cwt. of raw coal, whereas a ton of iron rails, 20 years ago, involved an expenditure exceeding 100 cwt. According to Dr. Percy, one large works consumed, in 1859, from 5 to 6 tons of coal per ton of rails. Statistics are unfortunately wanting to guide us respecting these important questions.

Considering the large margin for further improvement regarding almost every application of fuel which can be shown upon theoretical grounds to exist, it seems not unreasonable to conclude that the ratio of increase of population and of output of manufactured goods will be nearly balanced, for many years to come, by the further introduction of economical processes, and that our annual production of coal will remain substantially the same within that period, which under those circumstances will probably be a period of comparatively cheap coal.

The above-mentioned speculation leads to the further conclusion that our coal supply at a workable depth will last for a period far exceeding the shorter estimated period of 250 years, especially if we take into account the probability of fresh discoveries, of which we have had recent instances, particularly in

North Staffordshire, where a large area of coal and blackband ironstone is being opened up, under the auspices of his Grace the Duke of Sutherland, by our member, Mr. Homer.

Dr. Siemens then spoke of Anthracite and the large extent to which it was used in America, of Lignite and Peat, which may be looked on as coal in formation. After referring to natural gaseous fuel he went on to say :—

Although the use of natural gas is not likely to assume very large proportions owing to its rare occurrence, its application at Pittsburg has forcibly reminded me of a project I had occasion to put forward a good many years ago, namely to erect gas producers at the bottom of coal mines, and by the conversion of solid into gaseous fuel, to save entirely the labour of raising and carrying the latter to its destination. The gaseous fuel, in ascending from the bottom of the mine to the bank, would acquire in its ascent (owing to its temperature and low specific gravity), an onward pressure sufficient to propel it through pipes or culverts to a considerable distance, and it would be possible in this way to supply townships with heating gas, not only for use in factories, but, to a great extent, for domestic purposes also. In 1869, a company, in which I took a leading interest, was formed at Birmingham, under the sanction of the Town Council, to supply the town of Birmingham with heating gas at the rate of 6*d.* per 1,000 cubic feet, but their object was defeated by the existing gas companies, who opposed their bill in Parliament upon the ground that it would interfere with vested interests. I am still satisfied, however, that such a plan could be carried out with great advantage to the public ; and although I am no longer specifically interested in the matter, I would gladly lend my aid to those who might be willing to realise the same.

With reference to water power, Dr. Siemens said :—The advantage of utilising water power applies chiefly to continental countries, with large elevated plateaus, such as Sweden and the United States of North America, and it is interesting to contemplate the magnitude of power which is now for the most part lost, but which may be, sooner or later, called into requisition.

Take the Falls of Niagara as a familiar example. The amount of water passing over this fall has been estimated at 100 millions of tons per hour, and its perpendicular descent may be taken at 150 feet, without counting the rapids, which represent a further fall of 150 feet, making a total of 300 feet between lake and lake. But the force represented by the principal fall alone amounts to 16,800,000 horse-power, an amount which, if it had to be produced by steam, would necessitate an expenditure of not less than 266,000,000 tons of coal per annum, taking the consumption of coal at 4 lbs. per horse-power per hour. In other words, all the coal raised throughout the world would barely suffice to produce the amount of power that continually runs to waste at this one great fall. It would not be difficult, indeed, to realise a large proportion of the power so wasted, by means of turbines and water-wheels erected on the shores of the deep river below the falls, supplying them from canals cut along the edges. But it would be impossible to utilise the power on the spot, the district being devoid of mineral wealth, or other natural inducements for the establishment of factories. In order practically to render available the force of falling water at this and the thousands of other places under analogous conditions, we must devise a practicable means of carrying the power to a distance. Sir William Armstrong has taught us how to carry and utilise water power at a distance, if conveyed through high-pressure mains, and at Schaffhausen, in Switzerland, as well as at some other places on the Continent, it is conveyed by means of quick-working steel ropes passing over large pulleys. By these means, power may be carried to a distance of one or two miles without difficulty. Time will probably reveal to us effectual means of carrying power to great distances, but I cannot refrain from alluding to one which is, in my opinion, worthy of consideration, namely, the electrical conductor. Suppose water power to be employed to give motion to a dynamo-electrical machine—a very powerful electrical current is the result. This may be carried to a great distance, through a large metallic conductor, and there be made to impart motion to electro-magnetic engines to ignite the carbon points of electric lamps, or to effect the separation of metals from their combinations. A copper rod of 3 in. in diameter would be capable of transmitting 1,000 horse-power a distance of say 30 miles, an amount sufficient to supply one quarter of a million candle power which would suffice to illuminate a moderately sized town.

The use of electrical power has sometimes been suggested as a substitute for steam power, but it should be borne in mind that so long as the electric power depends upon a galvanic battery, it must be much more costly than steam power, inasmuch as the combustible consumed in the battery is zinc, a substance necessarily much more expensive than coal ; but this question assumes a totally different aspect if in the production of the electric current a natural force is used which could not otherwise be rendered available.

Dr. Siemens then went on to speak of the processes of manufacture, sketching briefly the history of the improvements in these processes, and concluded by referring to the various applications of steel. Speaking of the means of preserving iron and steel from rust, he referred to Prof. Barff's recently discovered process. This consists in exposing the metallic surfaces, while heated to redness, to the action of superheated steam, thus producing upon their surface the magnetic oxide of iron, which, unlike common rust, possesses the characteristic of permanency, and adheres closely to the metallic surface below. In this respect it is analogous to zinc oxide adhering to and protecting metallic zinc, with this further advantage in its favour, that the magnetic oxide is practically insoluble in sea water and other weak saline solutions.

Dr. Siemens concluded his valuable address by urging upon the Institute, now that it has attained to such importance, to obtain recognition in official quarters and to become possessed of a habitation in a central position, and in such a building as would serve the societies devoted to applied science in the same way that Burlington House does those devoted to pure science.

SOCIETIES AND ACADEMIES

London

Mathematical Society, March 8.—Mr. C. W. Merrifield, F.R.S., vice-president, in the chair.—The following communications were made :—On a new view of the Pascal hexagram, by Mr. T. Cotterill. In a system of co-planar points, the number of intersections of two chords is a multiple of 3. In the case of the hexagram the forty-five points thus derived are divided into four sets of triangles—(1) The three intersections of the chords joining four points form a triad self-conjugate to the conics through the four points. (2) Any three non-conterminous chords intersect in three points, forming a diagonal triangle. In each of these two cases, a derived point determines uniquely its corresponding triad, the number of triads being fifteen. (3) An inscribed triangle determines an opposite inscribed triangle ; the three intersections of the pairs of sides supposed to correspond form a triangle, the intersections of two inscribed triangles, the nine intersections of the two triangles forming an ennead. (4) The three intersections of the opposite sides of a hexagon of the system form a Pascal triangle. The number of triangles in each of the two last cases is sixty ; to each triangle of one set corresponding a triangle of the other, as well as a triad of the second set, the nine points forming three triads of the first set. Denoting, then, the primitive points by italics and fifteen of the derived points (no two of which are conjugate) by Greek letters, we obtain all the derived points by accenting once and twice the Greek letters to form self-conjugate triads. Tables are then formed in matrices of the nine chords joining the vertices of two opposite triangles and their eighteen intersections, found to consist of six triangles of each of the second and fourth sets. To these corresponds a matrix containing the nine intersections of the two triangles. In the case of a conic hexagram, the properties of the sixty points of intersection of chords with the tangents at the conic points are then examined.—On a class of integers expressible as the sum of two integral squares, by Mr. T. Muir. [The class of integers considered included those whose square root, when expressed as a continued fraction, has two middle terms in the cycle of partial denominators. A general expression was given for all such integers, and an equivalent expression in the form of the sum of two squares.]—Some properties of the double-theta functions, by Prof. Cayley, F.R.S. (founded on papers by Goepel and Rosenhain).—A property of an envelope, by Mr. J. J. Walker.

Chemical Society, March 15.—Prof. Abel, F.R.S., president, in the chair.—The secretary read a paper by Dr. W. A. Tilden and Mr. W. A. Shenstone, on isomeric nitroso terpenes, being a further contribution to Dr. Tilden's previous researches on these compounds. This was followed by a communication entitled "Preparation of Copper-zinc Couples," by Dr. J. H. Gladstone, and Mr. A. Tribe, which was experimentally illustrated; it gave the details of the experiments made to ascertain the conditions for the preparation of a couple of maximum activity. The other papers were on chromium pig-iron, by Mr. E. Riley; a note on gardenin, by Dr. J. Stenhouse and Mr. C. E. Groves; two papers by Mr. M. M. P. Muir entitled "Additional Note on a process for estimating Bismuth volumetrically," and "On certain Bismuth Compounds," Part IV.; and a note by Dr. M. Simpson and Mr. C. O. Keeffe, on the determination of urea by means of hypobromite.

Victoria (Philosophical) Institute, February 5.—Dr. C. Brooke, F.R.S., in the chair.—A paper was read by Prof. Birks, of Cambridge, on "The Bible and Modern Astronomy."

Geneva

Physical and Natural History Society, January 18.—M. Ernest Favre read a paper on the question of the origin of the gravels which are found in the portion of the Alps under the glacial soil, and which are known as the old alluvium. This is formed in the vicinity of the ancient glaciers, as is proved by the following facts: (1) The presence of glacial soil in two localities in the neighbourhood of Geneva, in the very interior of the alluvium, at several metres under the great glacial sheet; (2) the very different heights at which the alluvium is deposited in the interior of the same basin; (3) the fact that it is formed of the same elements, large pebbles and fossil sand at whatever distance it is observed from the foot of the Alps; the disappearance of these depôts begins the limit of the ancient glaciers.—M. Philippe Plantamour has undertaken observations on the variations of the level of the Lake of Geneva, similar to those of Prof. F. A. Forel at Morges. They confirm the theory of the perpetual oscillation to which the surface of the water is subject, as shown by Dr. Forel, and which lasts about an hour and a quarter in the longitudinal direction. The variations of level, or *seiches*, are much greater in the neigbourhood of Geneva, at the western extremity of the lake than at Morges, a little beyond the middle of its length towards the east, and they are in the opposite direction. A registering limnimetre, which is to be erected by M. Th. Plantamour, will permit of following with a new facility the phases of the phenomenon, and of comparing them with those which occur at Morges.

Paris

Academy of Sciences, March 12.—M. Peligot in the chair.—The following papers were read:—Theorems relative to series of isoperimetric triangles which have one side of constant size, and satisfy three other diverse conditions, by M. Chasles.—Influence of pressure on chemical phenomena, by M. Berthelot. He cites an experiment of Quincke's showing that the liberation of hydrogen from zinc and sulphuric acid is not stopped by pressure of the gas, but only retarded. It goes on so long as there is acid to saturate or zinc to dissolve.—On a metallic iron found at Santa Catarina (Brazil), by M. Damour. This is supposed of meteoric origin. The small quantities of carbon (0·0020) and silicium (0·0001) in it are like those of the best qualities of iron obtained in industry, while the proportion of nickel (0·3397) considerably exceeds that of meteoric irons hitherto known. To this latter is doubtless due its resistance to oxidation in moist air and to the action of dilute sulphuric and hydrochloric acids. M. Boussingault stated he had had cast in his laboratory 62 per cent. steel and 38 nickel. A polished face of the alloy did not rust in contact with air and water. Of the filings two or three grains took rust, merely showing the alloy was not entirely homogeneous. Alloys with 5, 10, or 15 per cent. nickel oxidised rapidly.—Observations on the native iron of Santa Catarina and on the pyrrhotine and magnetite associated with it, by M. Daubrée. The masses, when at a high temperature, seem to have been subjected to oxidising action of air or water, which action penetrated into the interior by very fine fissures.—On the maintenance of constant temperatures; second note by M. D'Arsonval. He heats the apparatus by means of a thermo-siphon, and the *rôle* of the regulator is to proportion the activity of the circulation to the causes of loss. Thus the fire may be of any strength; it gives its heat to a liquid which distributes it as the regulator allows.—On the annual aberration and annual parallax of stars, by M. Kericuff. He corrects some mistakes in the formulæ made for these.—Applications of a theorem comprising the two principles of the mechanical theory of heat, by M. Levy.—On the periodicity of solar spots, by M. Wolf. In a *brochure* he gives not only all the epochs of maxima and minima since the discovery of the spots, but, for a century and a quarter, by means of a relative number, the monthly energy of the phenomenon. He shows by curves the average course of the phenomena and anomalies; also the indices of a great period embracing sixteen small periods of eleven and a half years, or nearly 168 years.—Measurements of the calorific intensity of the solar radiations received at the surface of the ground, by M. Crova. He calculates that on January 4, 1876, the heat received on a square centimetre at right angles to the direction of the sun's rays from sunrise to sunset, would be 535·0 cal., that on the surface of the ground 161·2 cal.; for July 11, 1876, the corresponding numbers are 876·4 cal. and 574·1 cal. The heat received at right angles on January 4 is 0·610 of that on July 11; the heat received on the surface of the ground on January 4 is 0·281 of that on July 11.—Metals which accompany iron, by M. Terreil. Their proportions are small, they rarely amount to five thousandths; whereas, in native or meteoric iron, they may be ten per cent. They are chiefly manganese, nickel, cobalt, and chromium; while copper, vanadium, titanium and tungsten occur accidentally.—Chemical study of mistletoe, (*Viscum album*, L.), by MM. Grandeau and Bouton. *Inter alia*, the composition of the stem is very near that of the leaves, and the composition of the mistletoes of different species is widely different. As to nutritive value, the mistletoe of the oak takes rank with meadow grass of good quality or red clover, the leaves of the mistletoe of the cornelian and pear trees have equal value with good hay or aftermath; while their branches may be compared to the straw of leguminous plants, or the husks of cereals.—On the electrotonic state in the case of unipolar excitation of the nerves, by MM. Morat and Toussaint. When the positive pole is applied to the nerve, the current is divergent, from the middle of the nerve it goes towards the two extremities; it is thus in the two ends contrary to the proper current of the nerve; hence the negative phase of the electrotonic state. If the negative pole is applied, the battery current converges towards the middle, and is in the same direction with the proper current, which it increases (positive phase of the electrotonic state).—Acute poisoning by acetate of copper, by MM. Feltz and Ritter. It is more active than sulphate. The disorders are more intense and long in fasting animals. One could not swallow the substance in food or drink without perceiving the taste.—On the value of certain arguments of transformism, taken from the evolution of the dental follicles in ruminants, by M. Pietkiewicz. In these animals there is nothing at all like germs of canines and incisors, as Goodsir affirmed.—On the unity of the forces in geology, by M. Hermite.—On the crevasses of the cretaceous system, by M. Robert.

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