THURSDAY, AUGUST 19, 1897.

THE STORY OF AN INDIAN PROVINCE.

The North-Western Provinces of India. By W. Crooke, B.C.S. (retired). Pp. 361. (London: Methuen and Co., 1897.)

THIS book professes to be "an attempt to tell the story of one of the greatest of our Indian Provinces from the social point of view"; and no one who reads it will deny that the attempt has been completely successful. The author has had vast experience as an Indian official; he is well known as an authority on Indian religions and folk-lore (see the review in NATURE, April 22, 1897, of his work on "The Popular Religion and Folk-Lore of Northern India"), and, as Superintendent of the Ethnographical Survey of the N.W.P., he has had unique opportunities of studying the life and customs of the people. Lastly, he possesses the enviable faculty of being able to express the results of tedious and painstaking observations in a most readable form. In fact, we have here, in a nutshell, an amount of information which could only be obtained otherwise either by a similar experience or by a diligent study of many ponderous Blue-books. The style is easy and pleasant, and possesses no small merit from the literary point of view. The keen intelligent interest of the writer in his subject communicates itself to the reader, while his sympathy with the beauties of nature gives to the book something of the charm which characterises Bishop Heber's diary.

At the present time, especially, Mr. Crooke's work will be welcome as affording information on those serious questions which have of late excited an unusual degree of interest. Now that famine, plague, and, in some degree, disaffection, are taxing the resources of British administration, it is well to know what steps have been taken in times past to meet similar dangers and difficulties; and it is not too much to say that little doubt will be left in the impartial reader's mind that, whatever may have been the shortcomings of British rule, it has at least accumulated a vast store of knowledge and experience which enables it to cope, with a great degree of success, with calamities the effects of which would, in bygone ages, have been appalling beyond measure. And it must not be forgotten that, in India, there are many causes which combine to make progress not only slow and uncertain, but even sometimes dangerous. To take, for example, one of the difficulties which the ultra-conservative instinct of the people places in the way of the prevention of famine. Elsewhere the natural relief of a congested population is afforded by emigration, but—

"This people, again, definitely refuses to avail itself of that relief by emigration to less congested areas, which led the surplus population of Ireland to the American continent, and is now driving Italians to Brazil or Argentina, and the Chinaman to the Malay Peninsula and the islands of the Southern Sea. The State is thus here confronted with a problem which would tax the resources of the greatest governments. There is, perhaps, no more pathetic situation in the whole range of human history than to watch these dull, patient masses stumbling in their traditional way along a path which can lead only to suffering, most of them careless of the future, marrying and giving in marriage, fresh generations ever encroaching on the narrow margin which separates them from destitution."

Even greater obstacles to reform are those due to religious or social causes. Sanitary reform, for instance, must necessarily interfere with habits and customs all of which are sanctioned by a long tradition, and some of which are actually regarded as sacred.

"The progress made in sanitation during the last thirty years serves only to emphasise the fact that the task is of stupendous difficulty, that much of it is beyond the power of any Government to undertake unless it throws to the winds all considerations of finance and all regard for the prejudices of the people. . . . The constant crusade carried on to enforce some degree of cleanliness among the town population has undoubtedly been to some extent effective. . . . But as for a general crusade against filth in rural India the people will not endure it, and no Government in its senses would seriously propose to wage it."

Of course, if sanitation is not to be enforced, and if over-crowding is not to be prevented, how can plague be averted? And, as has been seen quite recently, the means of stamping out plague cannot be put into practice without grave risk of exciting the religious or caste susceptibilities of the people.

Some curious results come from the application of Western ideas to Eastern circumstances. We accept as fundamental axioms of law and justice the doctrines of the equality of all men in the eye of the law, and of the binding force of contracts legally made; and it is not imaginable that English government could prevail anywhere apart from these axioms. But it cannot be denied that rigid adherence to them has brought, and must necessarily bring, not only unpopularity to the Government, but also serious economic troubles to the people themselves. The first cause has, by completely upsetting all their ideas of the distinctions of caste, made British rule extremely distasteful to the nobility and higher classes generally.

"They disapprove of the cold impartiality of our law, which has abolished the traditional distinction between the gentleman and the menial, and makes it possible for the serf to drag the Rāja before one of our courts."

The second cause has results far more serious. We have had some instances recently in England of the power which the law, by enforcing the terms of a legal contract, gives to the usurer over his victim. But this abuse is in India carried to an extent of which we can have no conception.

"It is, in the opinion of the most competent authorities, not an exaggeration to say that three-fourths of the tenantry are indebted to the amount of a year's rent at least."

And the remedy for this terrible state of affairs, in which the most worthy portion of the community is surely being brought under the power of the least worthy? That is the problem which for many years has exercised, and is now exercising, many anxious minds, for on its solution the future prosperity of a great portion of India depends. Expedients innumerable have been suggested, and some of these cures have made the disease worse.

"We ourselves did try a form of usury law, but with the natural result. It was evaded wholesale by necessitous debtors, and the simple device was adopted of tacking on the prohibited interest to the original loan. . . . A native ruler of the old type would probably begin by putting half a score of usurers to death by slow torture; but this is not our way, nor would it in this case do much good except to check money-lending for a season. We are on the horns of a dilemma. We believe in the sanctity of contract, but we loathe Shylock, and writhe at seeing the peasant come shorn out of his clutches."

As an interesting and clear account of these and innumerable other questions affecting the welfare of the people, Mr. Crooke's book is greatly to be commended. The stay-at-home Englishman is, and always has been, inclined to lay down some system of theoretical perfection for the government of our great Eastern dependency. He will learn here that there are too many causes of friction—the opposing interests and prejudices of different religions, different races, and different castes—to admit of any perfectly smooth working of the great machine of Government. For the sake of peace and practical expediency it is necessary to yield a little here and a little there, and to moderate the rate of progress to suit the conditions of the case.

A really weak point in an otherwise altogether admirable work is that portion of Chapter ii. which deals with the history of the province from the earliest times down to the Muhammadan conquest. Mr. Crooke has not consulted the latest authorities on this subject; and in not doing so, he has made a mistake which is all the more serious because, as it happens, the study of inscriptions and coins has, within the last few years only, brought to light a very considerable amount of historical information. Very few of the dates for the earliest period given by Mr. Crooke are accepted by scholars nowadays. The initial year of the Gupta era is, for instance, no longer regarded by any one as lying between the years 160 and 170 A.D. For this statement Mr. Crooke quotes Mr. Vincent Smith: "the most recent authority on the gold coinage of the Guptas." Mr. Vincent Smith is still entitled to this designation; but, between the date of the work to which Mr. Crooke refers and the date of publication of Mr. Crooke's book, he has contributed a number of articles to learned periodicals, in which he has accepted the years 319-20 A.D. as the starting-point of the Gupta era Again, a great deal more is known now about the different Scythic invasions of Northern India and their dates than would be inferred from Mr. Crooke's account. In a word, this portion of the book—unimportant though it may be from the author's point of view—is altogether unsatisfactory, and should be entirely rewritten in a new edition.

It remains only to notice that the work is admirably printed, bound, and illustrated.

CHEMICAL EQUILIBRIUM.

The Phase Rule. By Wilder D. Bancroft. Pp. viii + 255. (*The Journal of Physical Chemistry*, Ithaca, New York, 1897.)

IN the preface to this book the author says:—

"My idea is that all qualitative experimental data (referring to chemical equilibrium) should be presented as particular applications of the Phase Rule and the Theorem of Le Chatelier, while the guiding principles for the classification of quantitative phenomena should be the Mass Law and the Theorem of van 't Hoff. In this book I have tried to present the subject of qualitative equilibrium from the point of view of the Phase Rule and of the Theorem of Le Chatelier, without the use of mathematics."

Notwithstanding the thoroughness with which this programme has, on the whole, been carried out, the writer cannot help feeling that the result is not completely satisfactory, and that this is largely due to the narrow treatment rendered necessary by the limitations which the author has imposed on himself. The phase rule itself merely gives the connection between the number of components of a system, the number of phases, and the number of external factors of equilibrium (pressure and temperature usually) on the one hand, and the number of independently variable factors of equilibrium on the other. It gives no further information about the system whatever. The theorem of Le Chatelier permits us to predict the direction in which the equilibrium will be displaced by a change in one of the factors of equilibrium. A study of chemical equilibrium from this limited point of view, almost of necessity resolves itself into an enumeration of the different possible cases (as, for example, a system containing one component and one phase, which may be liquid, solid, or gaseous; a system containing one component and two phases, and so on, considering in turn systems containing two, three, or more components), together with a description of the special cases which have been investigated, in so far as they illustrate the two guiding principles above mentioned. This is, on the whole, the plan adopted by the author, and his discussion of many possible cases of which no actual example has been investigated, will be of much service in indicating profitable lines for research.

The equilibria between solids and solutions, and between two solutions are very fully treated; whilst the dissociation of calcium carbonate and of ammonium chloride are the only ones of their kind mentioned, equilibria in homogeneous gaseous or liquid systems are omitted, and the metallic alloys receive somewhat scanty attention. Some of these omissions are doubtless due to the fact that the study of the cases in question is not materially assisted by the application of the phase rule.

The distinction which is made between solvent and dissolved substance (for which latter the author adopts the not very euphonious term "solute") appears to the writer to be purely conventional, and not, as Prof. Bancroft seems to think, a real difference. On pp. 36 and 37 we read:—

"In cases of limited miscibility there is no difficulty in telling which component is solvent and which solute; but when the two substances are consolute (miscible in all proportions), there is at present no sure way of deciding at what concentration the change takes place."

The introduction of a conception which is incapable of more accurate definition than the foregoing, can and does only obscure the subject. The distinction of solubility from fusion curves and the consequent differentiation of eutectic alloys from cryohydrates, seems to the writer to be unnecessary and undesirable. On p. 127 there is a curious discussion as to whether liquids which are miscible in all proportions are, or are not, mutually infinitely soluble. A clear definition of solubility, which is nowhere to be found in the book, would have made this impossible.

A little more care bestowed on the literary workmanship of the book would have made it much more attractive, and saved the reader both time and trouble. For example, he is left to guess what quantities are taken as ordinates and abscissæ of several of the curves; frequently, also, a curve is referred to without any indication of the diagram on which it is to be found; this is perplexing when, as on p. 49, the diagram is finally discovered eleven pages further back. The English, too, is not always above reproach. On p. 51 we read, for example: "If instead of diminishing the pressure upon a saturated solution reaching the divariant system, solid solute and vapour, we increase it," &c. Abrupt, not to say discourteous, criticism, such as the following (from p. 158), is also to be deprecated in a serious scientific work:—

"Étard has stated that the line O F terminates at the melting point of the more fusible salt. This is entirely wrong. The curve O F terminates at the temperature of the eutectic alloy formed from the two salts, a temperature which is necessarily lower than the melting point of potassium nitrate. Curiously enough, Étard has an inkling of the truth in one case, but it is not sufficient to make him modify his erroneous hypothesis."

The writer would not have dwelt on these comparatively trivial faults at such length were it not that the book contains much that is valuable. The numerous references to the newer work on the subject (and that is by far the greater part of it) will be of great service to any one desiring to make a more profound study of the problems of heterogeneous equilibrium. T. E.

EAST INDIAN BEES AND WASPS.

The Fauna of British India, including Ceylon and Burma. Edited by W. T. Blanford, F.R.S. (Published under the authority of the Secretary of State for India in Council.)

Hymenoptera. Vol. i. Wasps and Bees. By Lieut.-Colonel C. T. Bingham. 8vo. Pp. xxix + 579; 188 woodcuts, 4 coloured plates. (London: Taylor and Francis, 1897.)

THE first of the volumes before us is one of a series that is being produced under the authority of the Secretary of State for India, in order to diffuse knowledge already obtained and to facilitate the acquisition of further information. The book is the twelfth volume of the series, which was commenced in 1888 by the issue of a volume on the Mammals, written by Dr. W. T. Blanford, F.R.S., who is also the editor of the series. The entomology of India has been hitherto too much neglected, and the literature is very scattered and fragmentary; indeed, until quite recently it has not been possible to ascertain even the names of the locusts that from time to time devastate various parts of India. Our thanks are therefore due to those who are endeavouring to remedy this state of affairs. Four volumes on Lepidoptera have already appeared in the series. Though butterflies and moths are the special favourites of the Insect world with British entomologists, they are by no means the most important insects either from a scientific or an economic point of view, and the editor of the series has done well to break fresh ground with this volume on the bees and wasps, even though it cannot be expected that the work shall prove an exhaustive one. We are glad to find that the venture is a successful one, especially as very serious difficulties, arising from the incompleteness of collections of these insects and their inadequate nomenclature, have had to be encountered.

Colonel Bingham, the author of the second volume under notice, has an extensive acquaintance with the insects he describes, gained during many years of work as Conservator of Forests in India. The volume does not include the ants and the ruby-flies, but, with these exceptions, comprises the most important of the Hymenoptera, and includes just about 1000 species. It is chiefly devoted to descriptions and to tables by which the names of the species may be ascertained. As this work will necessarily be, for some time to come, the standard one on the aculeate Hymenoptera of India, it was necessary that nomenclature and classification should be thoroughly dealt with. These points are satisfactory, the descriptions being concise and clear, and the classification and nomenclature up to the level of the best and latest work. Now that the naturalists of India have this valuable volume to aid them in the preliminary work of finding out the names of the stinging Hymenoptera they meet with, we may hope that attention may be given to their habits and instincts. The fossorial Hymenoptera occupy considerably more than half the present volume; their habits are varied in details and of extreme interest, so that a large field is here opened to the observing naturalist. The fact that these insects are many of them able to sting the victims they carry off and store as food for their young, in a manner that would be creditable even if they had a perfect knowledge of anatomy, is now, thanks to the labours of Fabre and others, generally known.

It is not, however, known to more than a few that in one group of Fossors—the Ampulicides—the members have the still more inexplicable power of merely taking their prisoners captives, and making them march to the spot where they are to be immured and eaten. Colonel Bingham tells us that "these beautiful insects are predatory on cockroaches. In Burma I have frequently seen these wasps come into the house and search for their prey under boxes and furniture. In the forest once I watched a rather large specimen of *A. compressa*, Fabr., struggling with a huge cockroach; the latter was either paralysed by a sting, or dazed with fear, and was being half dragged by an antenna and foreleg, half hustled and pushed along by the active little wasp."

Although the number of species in this volume (about 1000, as we have previously stated) may appear to those unacquainted with entomology to be large, yet the volume in this respect is probably far from complete. Indeed, it is not improbable that the aculeate Hymenoptera of India will be found to number 2000 species; and now that this volume is available for the assistance of investigators, we may trust that the museums and naturalists of India will avail themselves of it in order to advance the subject by serious work, and thus render a second edition necessary. We may point out that in such case it would be well, in the bibliographical references, to distinguish those that relate to habits and biology from those that are systematic and nomenclatorial. D. S.

OUR BOOK SHELF.

A Contribution to the History of the Respiration of Man. By William Marcet, M.D., F.R.C.P., F.R.S. Pp. 116; charts and diagrams. (London: J. and A. Churchill, 1897.)

THE book before us comprises the subject-matter of the Croonian lectures delivered before the Royal College of Physicians in 1895, and an appendix, which latter contains a full description of the methods of investigation employed by the author. The first lecture contains a discussion of respiration from a general biological standpoint, and concludes by giving the effects of muscular exercise upon the production of CO_2, and the temperature of the body. The second lecture is devoted to human respiration; the different forms of breathing, and the absorption of oxygen in the body are considered in it. In the third lecture the effect of volition upon respiration is discussed, simple volition towards any kind of muscular exercise, *i.e.* volition without any response being attended by an increased production of CO_2, and an increased absorption of O. The question to what extent response to volition can be checked is, with regard to the author's deductions, of the greatest importance. In any case the volition exerted is not simple volition to increased breathing or muscular movement, but volition to the movement in question + volitional inhibition of the movement. The results of the author in this direction will almost certainly attract the attention of psychologists. In the fourth lecture the author discusses the changes in respiration produced by changes in the pressure of the atmosphere breathed, and concludes by showing the influence low atmospheric pressure exerts in checking volition. The appendix comprises a description of the methods used, and numerous experimental protocols.

The book must be regarded as a valuable contribution to the physiology of man. It is to be regretted that no general index, and no headings to the chapters are given. The summaries at p. 70 in the text could have been placed to much greater advantage at the head of their respective chapters. It is to be hoped that the sphere of usefulness of the book will not be curtailed by this omission. F. W. T.

Untersuchungen ueber den Bau der Cyanophycien und Bacterien. Von Prof. Dr. Alfred Fischer. Mit 3 lith. Tafeln. (Jena: verlag von G. Fischer, 1897.)

THE author gives a critical and literary account of the structure of the cells of bacteria and of the blue-green algæ, which possess many features in common. He comes to the conclusion, in opposition to many other investigators, that no real nucleus, or nucleus-like body is really present, but he considers that the colour of the algæ is to be regarded as localised in a chromatophore.

A great part of the book is devoted to an account of the methods in vogue in connection with researches into these minute histological details, and Dr. Fischer's criticisms will be read with interest by all who remember the fine work he has already done, especially in elucidating the structure of bacteria.

LETTERS TO THE EDITOR.

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

A Brilliant Perseid.

IN the hope of being able to record photographically the passage through our atmosphere of some of the August meteors, I exposed during the night of August 12 three photographic plates, one towards the pole star, and the other two towards the constellations of Perseus and Lyra. On the plate exposed to the latter constellation absolutely no trace of any meteor trail can be seen, but on the other two a very definite and distinct trail, peculiar for the different degrees of condensation in it, is easily visible. From the position and similarity of the trail on the two plates, there is no doubt that the same meteor was in question. These two plates were exposed in cameras, one a 5 × 4 with a Zeiss double combination lens of 224 millimetres focal length, and the other a 7½ × 5 with a Dallmeyer rapid rectilinear lens. The trail recorded by the latter instrument falls so near the edge of the plate that the image is somewhat out of focus; but on the other plate, although it is also near the edge, the image is very sharp and clear. From an examination of this plate, the following particulars have been obtained:—The path of the meteor extends nearly 9° in the constellation of Camelopardus, and a comparison with Cottam's star chart shows that the coordinates of the points of appearance and disappearance were, according to the photographic plate, R.A. 4h. 53m. Decl. + 65° 25′, and R.A. 5h. 32m. Decl. + 66° 15′. The actual visible length of trail must have been much longer than this, although the lens and plates used were both very rapid. It may be mentioned that the most dense part of the trail was equal in intensity to that given by the image of β Camelopardus (4th mag.) after an exposure of fifty-five minutes (11.20 p.m. to 12.15 a.m.).

The trail commences by being very faint, gradually increasing in density; it then fades off a little, and again becomes more dense for a short period of time. After another interval it becomes very dense, corresponding in this respect with the image of β Camelopardus. Again it becomes feeble for some distance on the plate with one minor condensation, and then suddenly there is an abrupt increase in density, after which it gradually fades away, and is no longer visible.

The path traversed lay nearly half-way between the stars 739 and 780, and passes a little to the south of α Camelopardus. The meteor was evidently a true Perseid, the trail, when produced backwards, lying slightly to the south of η Persei.

In conclusion, it may be added that the camera was set so that η Persei should fall in the centre of the plate, the instrument being mounted on the object-glass end of the Kensington 10-inch equatorial. The exposure lasted from 11h. 20m. p.m. to 12h. 15m. a.m. W. J. S. LOCKYER.

X-Ray Tubes.

THESE tubes when very highly exhausted become capricious; at times they will do good work, while at other times, and without any known cause, they refuse to illuminate.

In my endeavour to learn by experiment, I found that when playing the electricity upon and round the outer surface of the kathode end of the tube, it is quite possible to obtain sparks between the kathode wire and the inner surface of the glass. With each such spark the otherwise obstinate tube is momentarily illuminated; evidently the spark is owing to charges induced on the inner surfaces of the tube.

This led me to coat the kathode end of the tube with tinfoil, leaving about a ¼-inch gap between the tinfoil and the kathode terminal of the tube; the behaviour of the tube is now much affected—formerly it was uncertain when used with a 6-inch spark-length—and required frequently heating. With the tinfoil coating the tubes illuminate with certainty and with a much shorter spark-length. In fact I can now easily, and well, illuminate a highly-exhausted tube with an influence machine which has 17-inch plates. JAMES WIMSHURST.

THE APPROACHING TOTAL ECLIPSE OF THE SUN.[1]

IV.

THE programme of work to be attempted in the Indian eclipse of next year, referred to in the last article, carries me back very vividly to the eclipse of 1871, also observed in India. The shadow path of the eclipse of that year also cut the west coast of India, but at a much more southerly point than Viziadurg. The coast station was then Baikal, and from this point the shadow swept over the land in a south-east direction, as shown in the accompanying map (Fig. 13).

The retrospect is very encouraging, for one is reminded of the enormous advance in our knowledge of the sun since that time; and more than this, we have now the supreme advantage that eye observations have been almost entirely superseded by permanent photographic records. The accompanying view (Fig. 14) of my observatory at Baikal in 1871 will show that eye observations of the spectra alone were attempted.

It must also be remembered that none of the laboratory experiments, referred to in the last article, had then been made.

Now it was to try to understand such hard solar facts as those referred to in the last article, facts since observed carefully in all their detail, month after month and year after year, that much of my early experimental work was undertaken.

By 1872 the influence of quantity or density had been made out; when experiments were made at one temperature the spectrum got simpler as the quantity was reduced, so that the spectrum was finally reduced to its longest line.[1]

I am glad to see that Sir William Huggins, who appears to be ignorant of my quarter-of-a-century-old work, has quite recently arrived independently at the same conclusions.

Next came the influence of temperature. This was a much more difficult problem to tackle, for the reason that enormous changes in the spectrum of each chemical substance were brought about by changing the temperature conditions; but finally the association of certain lines with certain temperatures was accepted by everybody, though as to the *why* there were and are contending schools of opinion.

In connection with certain stellar problems awaiting solution, I have recently been compelled to return to this question, and I have used a more powerful current and larger jar-surface than that I formerly employed; and, further, the recent work carries the results into the photographic region. The result is important, since the old results have been confirmed and extended. To deal with the case of iron, seven additional lines in the spectrum have been found to have their brightness enhanced at the highest temperature.

These, as well as the two previously observed, are shown in the following table, which also indicates the behaviour

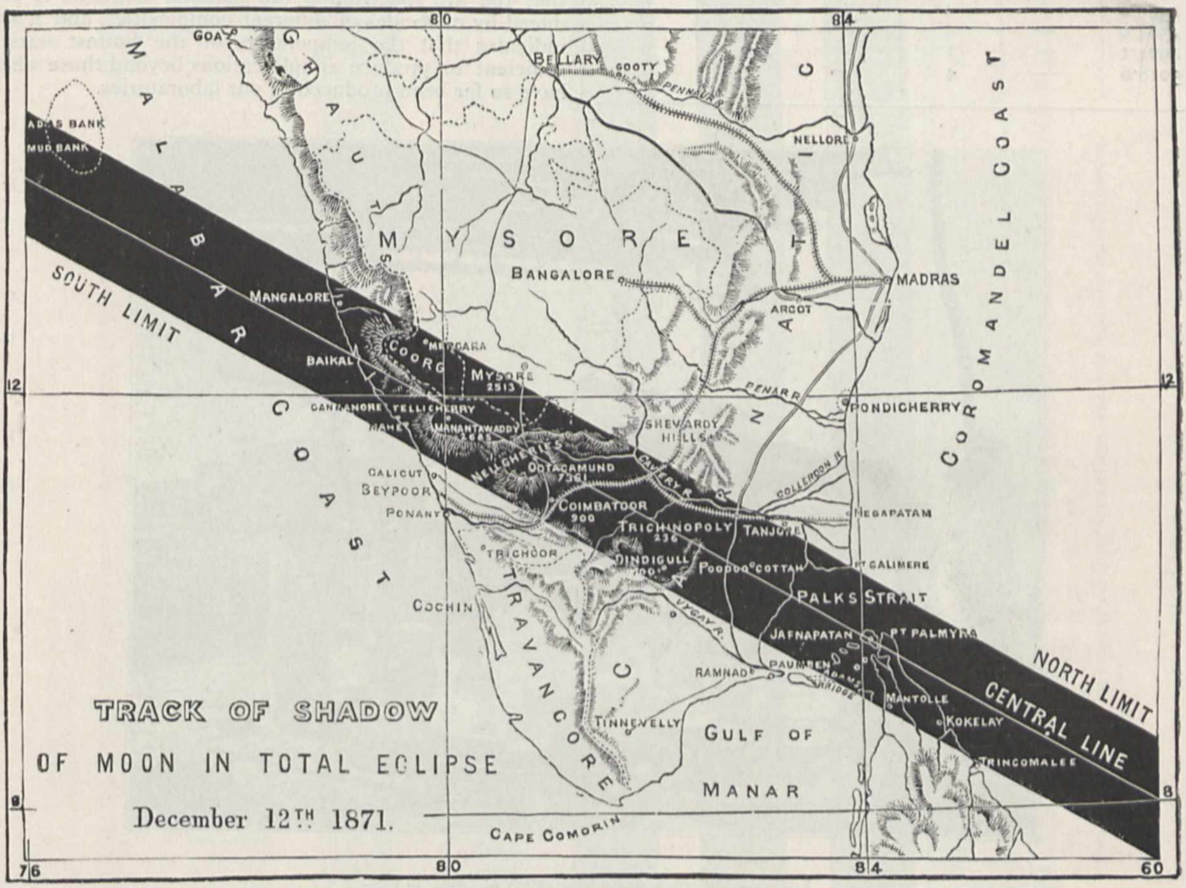

FIG. 13.

[1] Continued from page 321.

[1] *Phil. Trans.*, 1873, pp. 253 and 639.

of the lines under different conditions, as observed by Kayser and Runge (K. and R.) and myself (L.) in the arc, and by Thalèn (T.) and myself in sparks :—

Lines of Iron which are enhanced in Spark.

Wave-length.	Intensity in flame.	Intensity in arc (K. and R). Max. = 10.	Length in arc (L). Max. = 10.	Intensity in spark (T). Max. = 10.	Intensity in hot spark (L). Max. = 10.
4233·3	—	1	—	—	4
4508·5	—	1	—	—	4
4515·5	—	1	—	—	4
4520·4	—	1	—	—	2
4522·8	—	1	3	—	4
4549·6	—	4	5	—	6
4584·0	—	2	4	—	7
4924·1	—	1	3	6	6
5018·6	—	4	—	—	6

hottest stars are shown in the diagram on p. 368, and for the sake of comparison, the behaviour of a group of three lines, which are among the most marked at lower temperatures, is also indicated. In addition, the diagram shows the inversion in intensities of the spark and arc lines in the spectrum of a relatively cool star—such as α Orionis (Fig. 16).

The facts illustrated by the diagram indicate that the enhanced lines may be absent from the spectrum of a star, either on account of too low or too high a temperature. In the case of low temperature, however, iron is represented among the lines in the spectrum, but at the highest temperature all visible indications of its presence seem to have vanished.

This result affords a valuable confirmation of my view, that the arc spectrum of the metallic elements is produced by molecules of different complexities, and it also indicates that the temperature of the hottest stars is sufficient to produce simplifications beyond those which have so far been produced in our laboratories.

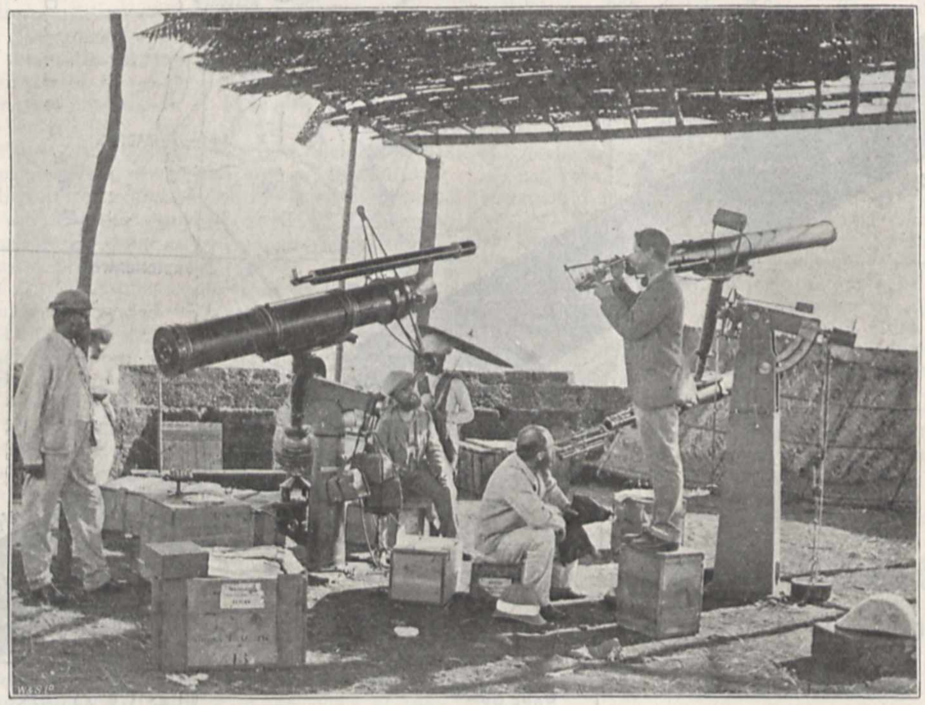

FIG. 14.—The spectroscope observatory at Baikal in 1871.

Combining this with former results, we seem justified in concluding that, in a space heated to the temperature of the hottest spark, and shielded from a lower temperature, these lines would constitute the spectrum of iron.

To enforce what I have previously written concerning the value of the solar work in relation to the study of the physics and chemistry of the stars, it is worth while to consider for a moment the behaviour of these lines of iron which are found to brighten as the temperature is increased and which play such an important part in the chromosphere spectrum, in stellar spectra (Fig. 15).

Defining the hottest stars as those in which the ultra-violet spectrum is most extended, it is known that absorption is indicated by few lines only. In these stars iron is practically represented by the enhanced lines alone; those which build up, for the most part, the arc spectrum are almost or entirely absent.

The intensities of the enhanced lines in some of the

We may say broadly that the stars Bellatrix, α Cygni, and Arcturus represent three very different stages of star life from the point of view we are considering.

In Bellatrix the metallic lines, both enhanced and cool, are almost entirely absent. In α Cygni we get the enhanced metallic lines alone; in Arcturus they are generally absent; this statement is true for the sun, the spectrum of which is almost identical with that of Arcturus.

Now it has been found from the study of the photographs of the chromosphere obtained in 1893 and 1896, that among the bright lines recorded the enhanced lines hold a most important place. I have already given copies of two of the photographs obtained in 1893. I can now add untouched copies of an enlargement of one of the photographs obtained in 1896, which has quite recently been published by the Royal Society. The photograph was obtained by Mr. Shackleton, attached to Sir George Baden Powell's expedition to Novaya Zemlya (Figs. 17, 18).

So far as the work has gone, the comparison of the enhanced lines with the spectrum of the chromosphere reveals several facts of importance. In the case of iron, I have already pointed out (*Roy. Soc. Proc.*, vol. lx. p. 475) that the enhanced lines were all present in the chromosphere during the eclipse of 1896, and most of them in that of 1893, their intensities being greater than those of the corresponding Fraunhofer lines. Many of the characteristic arc lines of iron also appear in the chromosphere, but the presence of the enhanced lines with such great intensities indicates that at least in some parts of the chromosphere the temperature of the iron vapour is considerably higher than that of the iron vapour which is most effective in producing the Fraunhofer lines. A

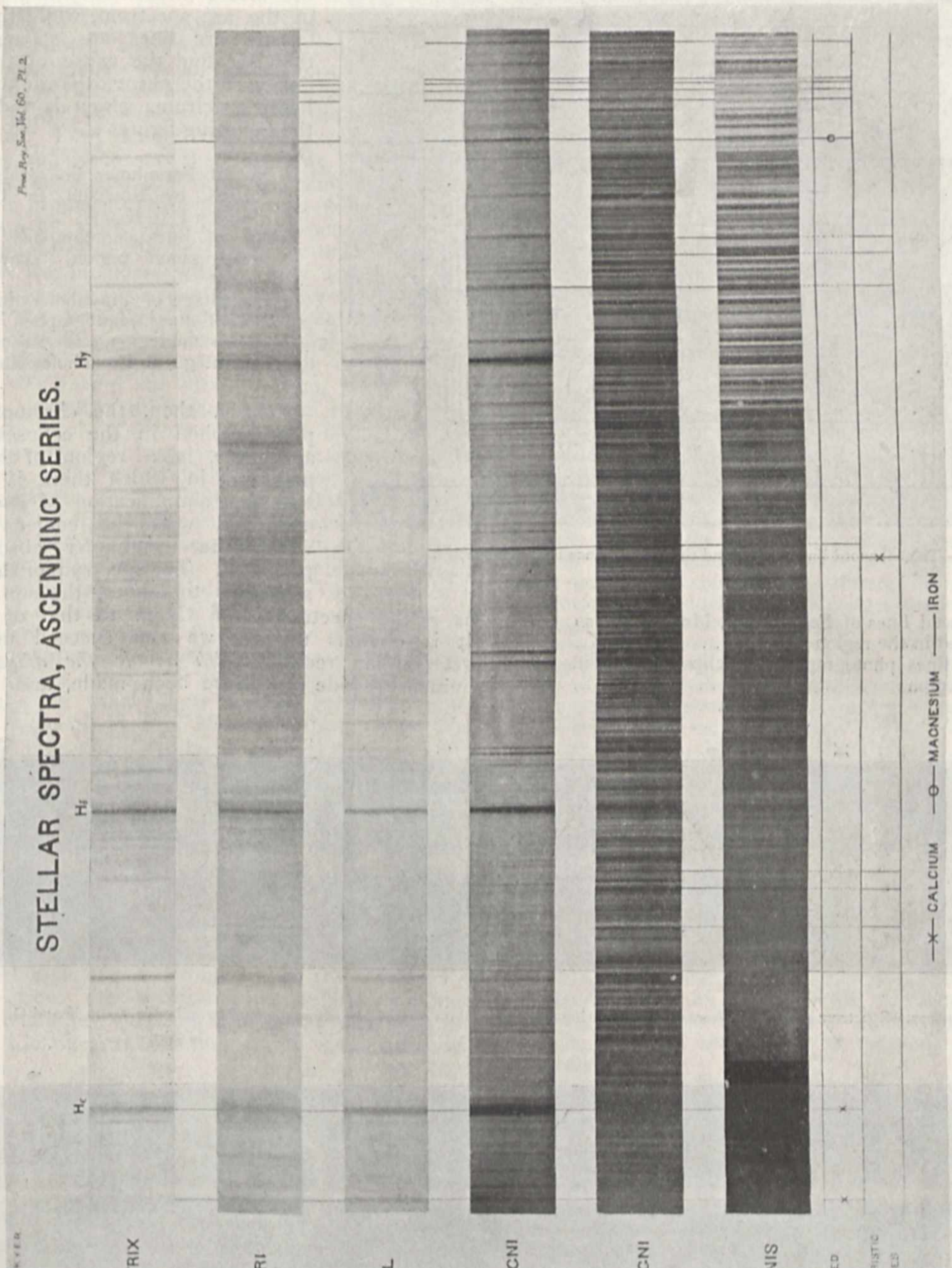

FIG. 15.—Map showing cool and enhanced lines of iron, and their behaviour in stellar spectra.

similar result is obtained when other substances are considered. The special importance of the enhanced lines in the chromosphere is shown by the following figures relating to substances which have been most completely studied.

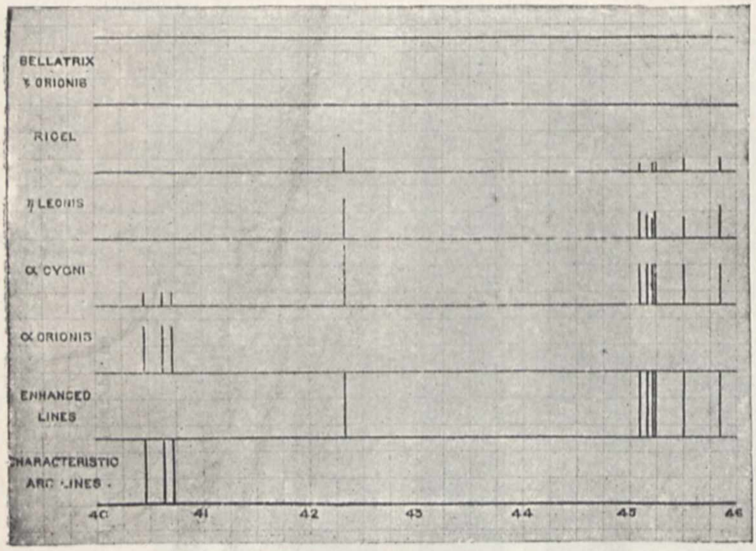

FIG. 16.—The enhanced lines of iron and their appearance in stellar spectra.

No. of enhanced lines of Fe, Mg, Ca, Mn, Ni, Ti, so far tabulated in the region F to K	63
No. of these lines photographed in eclipse of 1893 in the same region	28
Percentage of enhanced lines of Fe, &c., in eclipse of 1893	44
No. of enhanced lines photographed in eclipse of 1896	41
Percentage of enhanced lines of Fe, &c., in eclipse of 1896	65

These numbers show that the chromospheric spectrum is largely composed of enhanced metallic lines in addition to the lines of hydrogen and helium.

In the Fraunhofer spectrum enhanced lines may be regarded as wanting, for in the case of iron and magnesium, at least, they only appear with the feeble intensities which they have in the arc spectrum, while the characteristic arc lines are strong. Here, then, we find the cause of the dissimilarity of the chromospheric and Fraunhofer spectrum, which is indicated by the following figures :—

No. of Fraunhofer lines tabulated by Rowland in the region F to K	5694
No. of lines photographed in the same region, eclipse 1893	164
Percentage of Fraunhofer lines	3
No. of lines photographed in the same region, eclipse 1896	464
Percentage of Fraunhofer lines	8

Clearly, then, the chromosphere as photographed in the eclipses of 1893 and 1896, is a region of high temperature, in which there is a corresponding simplification of spectrum as compared with the cooler region in which the Fraunhofer absorption is produced. The spectrum of the chromosphere is to that of the sun generally as is the spectrum of *a* Cygni to that of Arcturus. It is obvious that if we can succeed in 1898 to get similar records *with double the dispersion*, an immense stride will have been made, and hence the programme for the coming eclipse to which I have drawn attention.

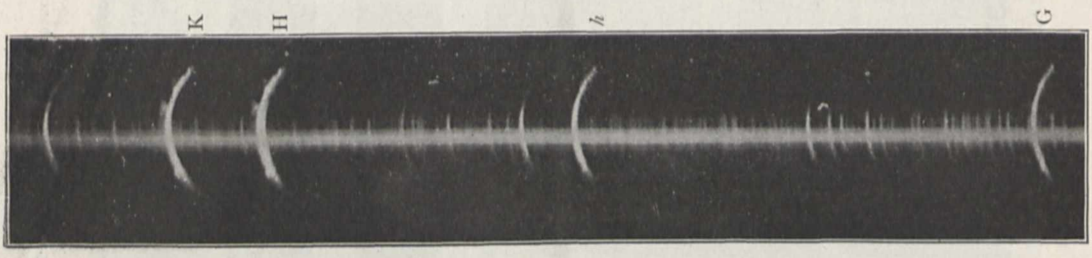

FIG. 17.—Spectrum of chromosphere obtained during the total eclipse of 1896, showing lines photographed between K and G.

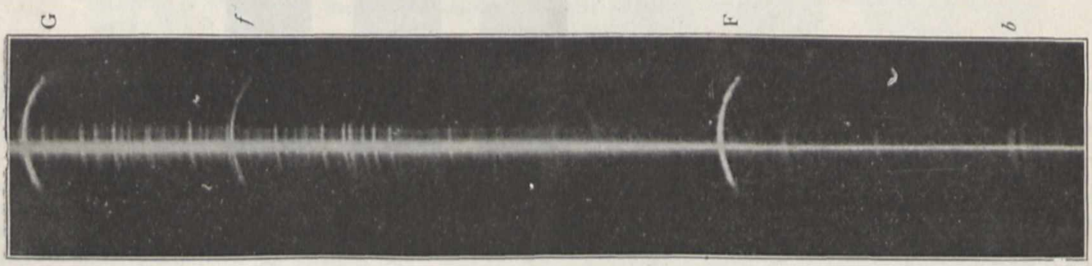

FIG. 18.—Spectrum of chromosphere obtained during the total eclipse of 1896, showing lines photographed between G and *b*.

NORMAN LOCKYER.

(*To be continued.*)

THE BRITISH ASSOCIATION.

CANADA is giving a warm reception to the members of the British Association who have gone over to attend the Toronto meeting. A large number of members arrived in Montreal on Monday, and there was a reception at the McGill University, at which about two hundred persons were present. The *Times* reports that the visitors were received by Vice-Principal Johnson, the Governors, and the Fellows, and were conducted in parties over the University, its fine collections, laboratories, &c. Luncheon was served in Molson Hall, and the President of the Governors proposed the toast of the Association. Lord Lister responded, and Sir John Evans proposed the toast of the University. The National Anthem was sung, and afterwards the company drove in carriages to Mount Royal, where a reception was held, which was much appreciated by the Association.

The programme of the meeting at Toronto shows that the time of the members will be fully occupied. The first general meeting took place yesterday as we went to press, and the President's address was delivered in the evening. The Sections meet to-day, and the addresses of the Sectional Presidents were to be delivered this morning. Following our usual custom, we print this week the complete addresses of the President and of the Presidents of Sections A and B. Other sectional addresses will appear in future numbers, and also reports of the work of the Sections.

A large garden party will be given this afternoon at the Royal Canadian Yacht Club, Toronto Island. His Excellency, the Governor-General, and the Countess of Aberdeen will this evening give a reception to the members in the Parliament buildings of the Province of Ontario. To-morrow there will be meetings of the Sections until about three o'clock, when there will be a Convocation of the University of Toronto to confer the honorary degree of Doctor of Laws upon Lord Kelvin, Lord Lister, Sir John Evans and the President of the American Association, Prof. Wolcott Gibbs. After Convocation is over, there will be several garden parties for the members. On Saturday morning the Sections will meet and adjourn early, in order that the members may take the various excursions which have been arranged from Saturday to Monday. On Tuesday next there will be a Convocation of Trinity University to confer the honorary degree of D.C.L. on Lord Lister, Sir John Evans and Prof. Forsyth; this ceremony will be followed by several garden parties. In the evening there will be a conversazione, with music and refreshments, in the main building of the University of Toronto for all the members of the Association. On Wednesday the Sections will meet for their concluding meeting, and in the afternoon the second and last general meeting of the Association will take place.

INAUGURAL ADDRESS BY SIR JOHN EVANS, K.C.B., D.C.L., LL.D., SC.D., TREAS.R.S., V.P.S.A., FOR.SEC.G.S., CORRESPONDANT DE L'INSTITUT DE FRANCE, &C., PRESIDENT.

ONCE more has the Dominion of Canada invited the British Association for the Advancement of Science to hold one of the annual meetings of its members within the Canadian territory; and for a second time has the Association had the honour and pleasure of accepting the proferred hospitality.

In doing so, the Association has felt that if by any possibility the scientific welfare of a locality is promoted by its being the scene of such a meeting, the claims should be fully recognised of those who, though not dwelling in the British Isles, are still inhabitants of that Greater Britain whose prosperity is so intimately connected with the fortunes of the Mother Country.

Here, especially, as loyal subjects of one beloved Sovereign, the sixtieth year of whose beneficent reign has just been celebrated with equal rejoicing in all parts of her Empire; as speaking the same tongue, and as in most instances connected by the ties of one common parentage, we are bound together in all that can promote our common interests.

There is, in all probability, nothing that will tend more to advance those interests than the diffusion of science in all parts of the British Empire, and it is towards this end that the aspirations of the British Association are ever directed, even if in many instances the aim may not be attained.

We are, as already mentioned, indebted to Canada for previous hospitality, but we must also remember that, since the time when we last assembled on this side of the Atlantic, the Dominion has provided the Association with a President, Sir William Dawson, whose name is alike well known in Britain and America, and whose reputation is indeed world-wide. We rejoice that we have still among us the pioneer of American geology, who among other discoveries first made us acquainted with the "Air-breathers of the Coal," the terrestrial or more properly arboreal Saurians of the New Brunswick and Nova Scotia Coal-measures.

On our last visit to Canada, in 1884, our place of assembly was Montreal, a city which is justly proud of her McGill University; to-day we meet within the buildings of another of the Universities of this vast Dominion—and in a city, the absolute fitness of which for such a purpose must have been foreseen by the native Indian tribes when they gave to a small aggregation of huts upon this spot the name of Toronto—" the place of meetings."

Our gathering this year presents a feature of entire novelty and extreme interest, inasmuch as the sister Association of the United States of America—still mourning the loss of her illustrious President, Prof. Cope—and some other learned societies, have made special arrangements to allow of their members coming here to join us. I need hardly say how welcome their presence is, nor how gladly we look forward to their taking part in our discussions, and aiding us by interchange of thought. To such a meeting the term "international" seems almost misapplied. It may rather be described as a family gathering, in which our relatives more or less distant in blood, but still intimately connected with us by language, literature, and habits of thought, have spontaneously arranged to take part.

The domain of science is no doubt one in which the various nations of the civilised world meet upon equal terms, and for which no other passport is required than some evidence of having striven towards the advancement of natural knowledge. Here, on the frontier between the two great English-speaking nations of the world, who is there that does not inwardly feel that anything which conduces to an intimacy between the representatives of two countries, both of them actively engaged in the pursuit of science, may also, through such an intimacy, react on the affairs of daily life, and aid in preserving those cordial relations that have now for so many years existed between the great American Republic and the British Islands, with which her early foundations are indissolubly connected? The present year has witnessed an interchange of courtesies which has excited the warmest feelings of approbation on both sides of the Atlantic. I mean the return to its proper custodians of one of the most interesting of the relics of the Pilgrim Fathers, the Log of the *Mayflower*. May this return, trifling in itself, be of happy augury as testifying to the feelings of mutual regard and esteem which animate the hearts both of the donors and of the recipients!

At our meeting in Montreal the President was an investigator who had already attained to a foremost place in the domains of Physics and Mathematics, Lord Rayleigh. In his address he dealt mainly with topics, such as Light, Heat, Sound, and Electricity, on which he is one of our principal authorities. His name and that of his fellow-worker, Prof. Ramsay, are now and will in all future ages be associated with the discovery of the new element, Argon. Of the ingenious methods by which that discovery was made, and the existence of Argon established, this is not the place to speak. One can only hope that the element will not always continue to justify its name by its inertness.

The claims of such a leader in physical science as Lord Rayleigh to occupy the Presidential chair are self-evident, but possibly those of his successor on this side of the Atlantic are not so immediately apparent. I cannot for a moment pretend to place myself on the same purely scientific level as my distinguished friend and for many years colleague, Lord Rayleigh, and my claims, such as they are, seem to me to rest on entirely different grounds.

Whatever little I may have indirectly been able to do in assisting to promote the advancement of science, my principal efforts have now for many years been directed towards attempting to forge those links in the history of the world, and especially of humanity, that connect the past with the present, and towards tracing that course of evolution which plays as important a part in the physical and moral development of man as it does in that of the animal and vegetable creation.

It appears to me, therefore, that my election to this important post may, in the main, be regarded as a recognition by this Association of the value of Archæology as a science.

Leaving all personal considerations out of question, I gladly hail this recognition, which is, indeed, in full accordance with the attitude already for many years adopted by the Association towards Anthropology, one of the most important branches of true Archæology.

It is no doubt hard to define the exact limits which are to be assigned to Archæology as a science, and Archæology as a branch of History and Belles Lettres. A distinction is frequently drawn between science on the one hand, and knowledge of learning on the other; but translate the terms into Latin, and the distinction at once disappears. In illustration of this I need only cite Bacon's great work on the "Advancement of Learning," which was, with his own aid, translated into Latin under the title, "De Augmentis Scientiarum."

It must, however, be acknowledged that a distinction does exist between Archæology proper, and what, for want of a better word, may be termed Antiquarianism. It may be interesting to know the internal arrangements of a Dominican convent in the middle ages; to distinguish between the different mouldings characteristic of the principal styles of Gothic architecture; to determine whether an English coin bearing the name of Henry was struck under Henry II., Richard, John, or Henry III., or to decide whether some given edifice was erected in Roman, Saxon, or Norman times. But the power to do this, though involving no small degree of detailed knowledge and some acquaintance with scientific methods, can hardly entitle its possessors to be enrolled among the votaries of science.

A familiarity with all the details of Greek and Roman mythology and culture must be regarded as a literary rather than a scientific qualification; and yet when among the records of classical times we come upon traces of manners and customs which have survived for generations, and which seem to throw some rays of light upon the dim past, when history and writing were unknown, we are, I think, approaching the boundaries of scientific Archæology.

Every reader of Virgil knows that the Greeks were not merely orators, but that with a pair of compasses they could describe the movements of the heavens and fix the rising of the stars; but when by modern Astronomy we can determine the heliacal rising of some well-known star, with which the worship in some given ancient temple is known to have been connected, and can fix its position on the horizon at some particular spot, say three thousand years ago, and then find that the axis of the temple is directed exactly towards that spot, we have some trustworthy scientific evidence that the temple in question must have been erected at a date approximately 1100 years B.C. If on or close to the same site we find that more than one temple was erected, each having a different orientation, these variations, following as they may fairly be presumed to do the changing position of the rising of the dominant star, will also afford a guide as to the chronological order of the different foundations. The researches of Mr. Penrose seem to show that in certain Greek temples, of which the date of foundation is known from history, the actual orientation corresponds with that theoretically deduced from astronomical data.

Sir J. Norman Lockyer has shown that what holds good for Greek temples applies to many of far earlier date in Egypt, though up to the present time hardly a sufficient number of accurate observations have been made to justify us in foreseeing all the instructive results that may be expected to arise from Astronomy coming to the aid of Archæology.

The intimate connection of Archæology with other sciences is in no case so evident as with respect to Geology, for when considering subjects such as those I shall presently discuss, it is almost impossible to say where the one science ends and the other begins.

By the application of geological methods many archæological questions relating even to subjects on the borders of the historical period have been satisfactorily solved. A careful examination of the limits of the area over which its smaller coins are found has led to the position of many an ancient Greek city being accurately ascertained; while in England it has only been by treating the coins of the Ancient Britons, belonging to a period before the Roman occupation, as if they were actual fossils, that the territories under the dominion of the various kings and princes who struck them have been approximately determined. In arranging the chronological sequence of these coins, the evolution of their types—a process almost as remarkable, and certainly as well-defined, as any to be found in nature —has served as an efficient guide. I may venture to add that the results obtained from the study of the morphology of this series of coins were published ten years before the appearance of Darwin's great work on the "Origin of Species."

When we come to the consideration of the relics of the Early Iron and Bronze Ages, the aid of Chemistry has of necessity to be invoked. By its means we are able to determine whether the iron of a tool or weapon is of meteoritic or volcanic origin, or has been reduced from iron-ore, in which case considerable knowledge of metallurgy would be involved on the part of those who made it. With bronze antiquities the nature and extent of the alloys combined with the copper may throw light not only on their chronological position, but on the sources whence the copper, tin and other metals of which they consist were originally derived. I am not aware of there being sufficient differences in the analysis of the native copper from different localities in the region in which we are assembled, for Canadian Archæologists to fix the sources from which the metal was obtained which was used in the manufacture of the ancient tools and weapons of copper that are occasionally discovered in this part of the globe.

Like Chemistry, Mineralogy and Petrology may be called to the assistance of Archæology in determining the nature and source of the rocks of which ancient stone implements are made; and, thanks to researches of the followers of those sciences, the old view that all such implements formed of jade and found in Europe must of necessity have been fashioned from material imported from Asia can no longer be maintained. In one respect the Archæologist differs in opinion from the Mineralogist—namely, as to the propriety of chipping off fragments from perfect and highly-finished specimens for the purpose of submitting them to microscopic examination.

I have hitherto been speaking of the aid that other sciences can afford to Archæology when dealing with questions that come almost, if not quite, within the fringe of history, and belong to times when the surface of our earth presented much the same configuration as regards the distribution of land and water, and hill and valley, as it does at present, and when, in all probability, the climate was much the same as it now is. When, however, we come to discuss that remote age in which we find the earliest traces that are at present known of Man's appearance upon earth, the aid of Geology and Palæontology becomes absolutely imperative.

The changes in the surface configuration and in the extent of the land, especially in a country like Britain, as well as the modifications of the fauna and flora since those days, have been such that the Archæologist pure and simple is incompetent to deal with them, and he must either himself undertake the study of these other sciences or call experts in them to his assistance. The evidence that Man had already appeared upon the earth is afforded by stone implements wrought by his hands, and it falls strictly within the province of the Archæologist to judge whether given specimens were so wrought or not; it rests with the Geologist to determine their stratigraphical or chronological position, while the Palæontologist can pronounce upon the age and character of the associated fauna and flora.

If left to himself, the Archæologist seems too prone to build up theories founded upon form alone, irrespective of geological conditions. The Geologist, unaccustomed to archæological details, may readily fail to see the difference between the results of the operations of Nature and those of Art, and may be liable to trace the effects of man's handiwork in the chipping, bruising and wearing which in all ages result from natural forces; but the united labours of the two, checked by those of the Palæontologist, cannot do otherwise than lead towards sound conclusions.

It will, perhaps, be expected of me that I should on the present occasion bring under review the state of our present knowledge with regard to the Antiquity of Man; and probably no fitter place could be found for the discussion of such a topic than the adopted home of my venerated friend, the late Sir Daniel Wilson, who first introduced the word "prehistoric" into the English language.

Some among us may be able to call to mind the excitement, not only among men of science, but among the general public, when, in 1859, the discoveries of M. Boucher de Perthes and Dr. Rigollot in the gravels of the valley of the Somme, at Abbeville and Amiens, were confirmed by the investigations of the late Sir Joseph Prestwich, myself, and others, and the co-existence of Man with the extinct animals of the Quaternary fauna, such as the mammoth and woolly-haired rhinoceros, was first virtually established. It was, at the same time, pointed out that these relics belonged to a far earlier date than the ordinary stone weapons found upon the surface, which usually showed signs of grinding or polishing, and that, in fact, there were two Stone Ages in Britain. To these the terms Neolithic and Palæolithic were subsequently applied by Sir John Lubbock.

The excitement was not less, when, at the meeting of this Association at Aberdeen in the autumn of that year, Sir Charles Lyell, in the presence of the Prince Consort, called attention to the discoveries in the valley of the Somme, the site of which he had himself visited, and to the vast lapse of time indicated by the position of the implements in drift-deposits a hundred feet above the existing river.

The conclusions forced upon those who examined the facts on the spot did not receive immediate acceptance by all who were interested in Geology and Archæology, and fierce were the controversies on the subject that were carried on both in the newspapers and before various learned societies.

It is at the same time instructive and amusing to look back on the discussions of those days. While one class of objectors accounted for the configuration of the flint implements from the gravels by some unknown chemical agency, by the violent and continued gyratory action of water, by fracture resulting from pressure, by rapid cooling when hot or by rapid heating when cold, or even regarded them as aberrant forms of fossil fishes, there were others who, when compelled to acknowledge that the implements were the work of men's hands, attempted to impugn and set aside the evidence as to the circumstances under which they had been discovered. In doing this they adopted the view that the worked flints had either been introduced into the containing beds at a comparatively recent date, or if they actually formed constituent parts of the gravel then that this was a mere modern alluvium resulting from floods at no very remote period.

In the course of a few years the main stream of scientific thought left this controversy behind, though a tendency to cut down the lapse of time necessary for all the changes that have taken place in the configuration of the surface of the earth and in the character of its occupants since the time of the Palæolithic gravels, still survives in the inmost recesses of the hearts of not a few observers.

In his Address to this Association at the Bath meeting of 1864, Sir Charles Lyell struck so true a note that I am tempted to reproduce the paragraph to which I refer :—

"When speculations on the long series of events which occurred in the glacial and post-glacial periods are indulged in, the imagination is apt to take alarm at the immensity of the time required to interpret the monuments of these ages, all referable to the era of existing species. In order to abridge the number of centuries which would otherwise be indispensable, a disposition is shown by many to magnify the rate of change in prehistoric times by investing the causes which have modified the animate and inanimate world with extraordinary and excessive energy. It is related of a great Irish orator of our day that when he was about to contribute somewhat parsimoniously towards a public charity, he was persuaded by a friend to make a more liberal donation. In doing so he apologised for his first apparent want of generosity by saying that his early life had been a constant struggle with scanty means, and that 'they who are born to affluence cannot easily imagine how long a time it takes to get the chill of poverty out of one's bones.' In like manner we of the living generation, when called upon to make grants of thousands of centuries in order to explain the events of what is called the modern period, shrink naturally at first from making what seems so lavish an expenditure of past time. Throughout our early education we have been accustomed to such strict economy in all that relates to the chronology of the earth and its inhabitants in remote ages, so fettered have we been by old traditional beliefs, that even when our reason is convinced, and we are persuaded that we ought to make more liberal grants of time to the Geologist, we feel how hard it is to get the chill of poverty out of our bones."

Many, however, have at the present day got over this feeling, and of late years the general tendency of those engaged upon the question of the antiquity of the human race has been in the direction of seeking for evidence by which the existence of Man upon the earth could be carried back to a date earlier than that of the Quaternary gravels.

There is little doubt that such evidence will eventually be forthcoming, but, judging from all probability, it is not in Northern Europe that the cradle of the human race will eventually be discovered, but in some part of the world more favoured by a tropical climate, where abundant means of subsistence could be procured, and where the necessity for warm clothing did not exist.

Before entering into speculations on this subject, or attempting to lay down the limits within which we may safely accept recent discoveries as firmly established, it will be well to glance at some of the cases in which implements are stated to have been found under circumstances which raise a presumption of the existence of man in pre-Glacial, Pliocene, or even Miocene times.

Flint implements of ordinary Palæolithic type have, for instance, been recorded as found in the Eastern Counties of England, in beds beneath the Chalky Boulder Clay; but on careful examination the geological evidence has not to my mind proved satisfactory, nor has it, I believe, been generally accepted. Moreover, the archæological difficulty that Man, at two such remote epochs as the pre-Glacial and the post-Glacial, even if the term Glacial be limited to the Chalky Boulder Clay, should have manufactured implements so identical in character that they cannot be distinguished apart, seems to have been entirely ignored.

Within the last few months we have had the report of worked flints having been discovered in the late Pliocene Forest Bed of Norfolk, but in that instance the signs of human workmanship upon the flints are by no means apparent to all observers.

But such an antiquity as that of the Forest Bed is as nothing when compared with that which would be implied by the discoveries of the work of men's hands in the Pliocene and Miocene beds of England, France, Italy, and Portugal, which have been accepted by some Geologists. There is one feature in these cases which has hardly received due attention, and that is the isolated character of the reputed discoveries. Had man, for instance, been present in Britain during the Crag Period, it would be strange indeed if the sole traces of his existence that he left were a perforated tooth of a large shark, the sawn rib of a manatee, and a beaming full face, carved on the shell of a pectunculus!

In an address to the Anthropological Section at the Leeds meeting of this Association in 1890 I dealt somewhat fully with these supposed discoveries of the remains of human art in beds of Tertiary date; and I need not here go further into the question. Suffice it to say that I see no reason why the verdict of "not proven" at which I then arrived should be reversed.

In the case of a more recent discovery in Upper Burma in beds at first pronounced to be Upper Miocene, but subsequently "definitely ascertained to be Pliocene," some of the flints are of purely natural and not artificial origin, so that two questions arise: first, Were the fossil remains associated with the worked flints or with those of natural forms? And second, Where they actually found in the bed to which they have been assigned, or did they merely lie together on the surface?

Even the *Pithecanthropus erectus* of Dr. Eugène Dubois from Java meets with some incredulous objectors from both the physiological and the geological sides. From the point of view of the latter the difficulty lies in determining the exact age of what are apparently alluvial beds in the bottom of a river valley.

When we return to Palæolithic man, it is satisfactory to feel that we are treading on comparatively secure ground, and that the discoveries of the last forty years in Britain alone enable us to a great extent to reconstitute his history. We may not know the exact geological period when first he settled in the British area, but we have good evidence that he occupied it at a time when the configuration of the surface was entirely different from what it is at present: when the river valleys had not been cut down to anything like their existing depth, when the fauna of the country was of a totally different character from that of the present day, when the extension of the southern part of the island seaward was in places such that the land was continuous with that of the continent, and when in all probability a far more rainy climate prevailed. We have proofs of the occupation of the country by man during the long lapse of time that was necessary for the excavation of the river valleys. We have found the old floors on which his habitations were fixed, we have been able to trace him at work on the manufacture of flint instruments, and by building up the one upon the other the flakes struck off by the primæval workman in those remote times we have been able to reconstruct the blocks of flint which served as his material.

That the duration of the Palæolithic Period must have extended over an almost incredible length of time is sufficiently proved by the fact that valleys, some miles in width and of a depth of from 100 to 150 feet, have been eroded since the deposit of the earliest implement-bearing beds. Nor is the apparent duration of this period diminished by the consideration that the floods which hollowed out the valleys were not in all probability of such frequent occurrence as to teach Palæolithic man by experience the danger of settling too near to the streams, for had he kept to the higher slopes of the valley there would have been but little chance of his implements having so constantly formed constituent parts of the gravels deposited by the floods.

The examination of British cave-deposits affords corroborative evidence of this extended duration of the Palæolithic Period. In Kent's Cavern at Torquay, for instance, we find in the lowest deposit, the breccia below the red cave-earth, implements of flint and chert corresponding in all respects with those of the high level and most ancient river gravels. In the cave-earth these are scarcer, though implements occur which also have their analogues in the river deposits; but, what is more remarkable, harpoons of reindeer's horn and needles of bone are present, identical in form and character with those of the caverns of the Reindeer Period in the South of France, and suggestive of some bond of union or identity of descent between the early troglodytes, whose habitations were geographically so widely separated the one from the other.

In a cavern at Creswell Crags, on the confines of Derbyshire and Nottinghamshire, a bone has moreover been found engraved with a representation of parts of a horse in precisely the same style as the engraved bones of the French caves.

It is uncertain whether any of the River-drift specimens belong to so late a date as these artistic cavern-remains; but the greatly superior antiquity of even these to any Neolithic relics is testified by the thick layer of stalagmite, which had been deposited in Kent's Cavern before its occupation by men of the Neolithic and Bronze Periods.

Towards the close of the period covered by the human occupation of the French caves, there seems to have been a dwindling in the number of the larger animals constituting the Quaternary fauna, whereas their remains are present in abundance in the lower and therefore more recent of the valley gravels. This circumstance may afford an argument in favour of regarding the period represented by the later French caves as a continuation of that during which the old river gravels were deposited, and yet the great change in the fauna that has taken place since the latest of the cave-deposits included in the Palæolithic Period is indicative of an immense lapse of time.

How much greater must have been the time required for the more conspicuous change between the old Quaternary fauna of the river gravels and that characteristic of the Neolithic Period!

As has been pointed out by Prof. Boyd Dawkins, only thirty-one out of the forty-eight well-ascertained species living in the post-Glacial or River-drift Period survived into prehistoric or Neolithic times. We have not, indeed, any means at command for estimating the number of centuries which such an important change indicates; but when we remember that the date of the commencement of the Neolithic or Surface Stone Period is still shrouded in the mist of a dim antiquity, and that prior to that commencement the River-drift Period had long come to an end; and when we further take into account the almost inconceivable ages that even under the most favourable conditions the excavation of wide and deep valleys by river action implies, the remoteness of the date at which the Palæolithic Period had its beginning almost transcends our power of imagination.

We find distinct traces of river action from 100 to 200 feet above the level of existing streams and rivers, and sometimes at a great distance from them; we observe old fresh-water deposits on the slopes of valleys several miles in width; we find that long and lofty escarpments of rock have receded unknown distances since their summits were first occupied by Palæolithic man; we see that the whole side of a wide river valley has been carried away by an invasion of the sea, which attacked and removed a barrier of chalk cliffs from 400 to 600 feet in height; we find that what was formerly an inland river has been widened out into an arm of the sea, now the highway of our fleets, and that gravels which were originally deposited in the bed of some ancient river now cap isolated and lofty hills.

And yet, remote as the date of the first known occupation of Britain by man may be, it belongs to what, geologically speaking, must be regarded as a quite recent period, for we are now in a position to fix with some degree of accuracy its place on the geological scale. Thanks to investigations ably carried out at Hoxne in Suffolk, and at Hitchin in Hertfordshire, by Mr. Clement Reid, under the auspices of this Association and of the Royal Society, we know that the implement-bearing beds at those places undoubtedly belong to a time subsequent to the deposit of the Great Chalky Boulder Clay of the Eastern Counties of England. It is, of course, self-evident that this vast deposit, in whatever manner it may have been formed, could not, for centuries after its deposition was complete, have presented a surface inhabitable by man. Moreover, at a distance but little further north, beds exist which also, though at a somewhat later date, were apparently formed under Glacial conditions. At Hoxne the interval between the deposit of the Boulder Clay and of the implement bearing beds is distinctly proved to have witnessed at least two noteworthy changes in climate. The beds immediately reposing on the Clay are characterised by the presence of alder in abundance, of hazel, and yew, as well as by that of numerous flowering plants indicative of a temperate climate very different from that under which the Boulder Clay itself was formed. Above these beds characterised by temperate plants, comes a thick and more recent series of strata, in which leaves of the dwarf Arctic willow and birch abound, and which were in all probability deposited under conditions like those of the cold regions of Siberia and North America.

At a higher level and of more recent date than these—from which they are entirely distinct—are the beds containing Palæolithic implements, formed in all probability under conditions not essentially different from those of the present day. However this may be, we have now conclusive evidence that the Palæolithic implements are, in the Eastern Counties of England, of a date long posterior to that of the Great Chalky Boulder Clay.

It may be said, and said truly, that the implements at Hoxne cannot be shown to belong to the beginning rather than to some later stage of the Palæolithic Period. The changes, however, that have taken place at Hoxne in the surface configuration of the country prove that the beds containing the implements cannot belong to the close of that period.

It must, moreover, be remembered that in what are probably the earliest of the Palæolithic deposits of the Eastern Counties, those at the highest level, near Brandon in Norfolk, where the gravels contain the largest proportion of pebbles derived from Glacial beds, some of the implements themselves have been manufactured from materials not native to the spot but brought from a distance, and derived in all probability either from the Boulder Clay or from some of the beds associated with it.

We must, however, take a wider view of the whole question, for it must not for a moment be supposed that there are the slightest grounds for believing that the civilisation, such as it was, of the Palæolithic Period originated in the British Isles. We find in other countries implements so identical in form and character with British specimens that they might have been manufactured by the same hands. These occur over large areas in France under similar conditions to those that prevail in England. The same forms have been discovered in the ancient river gravels of Italy, Spain, and Portugal. Some few have been recorded from the north of Africa, and analogous types occur in considerable numbers in the south of that continent. On the banks of the Nile, many hundreds of feet above its present level, implements of the European types have been discovered; while in Somaliland, in an ancient river valley at a great elevation above the sea, Mr. Seton-Karr has collected a large number of implements formed of flint and quartzite, which, judging from their form and character, might have been dug out of the drift deposits of the Somme or the Seine, the Thames or the ancient Solent.

In the valley of the Euphrates implements of the same kind have also been found, and again further east in the lateritic deposits of Southern India they have been obtained in considerable numbers. It is not a little remarkable, and is at the same time highly suggestive, that a form of implement almost peculiar to Madras reappears among implements from the very ancient gravels of the Manzanares at Madrid. In the case of the

African discoveries we have as yet no definite Palæontological evidence by which to fix their antiquity, but in the Narbadá Valley of Western India Palæolithic implements of quartzite seem to be associated with a local fauna of Pleistocene age, comprising, like that of Europe, the elephant, hippopotamus, ox, and other mammals of species now extinct. A correlation of the two faunas with a view of ascertaining their chronological relations is beset with many difficulties, but there seems reason for accepting this Indian Pleistocene fauna as in some degree more ancient than the European.

Is this not a case in which the imagination may be fairly invoked in aid of science? May we not from these data attempt in some degree to build up and reconstruct the early history of the human family? There, in Eastern Asia, in a tropical climate, with the means of subsistence readily at hand, may we not picture to ourselves our earliest ancestors, gradually developing from a lowly origin, acquiring a taste for hunting—if not, indeed, being driven to protect themselves from the beasts around them—and evolving the more complicated forms of tools or weapons from the simpler flakes which had previously served them as knives? May we not imagine that, when once the stage of civilisation denoted by these Palæolithic implements had been reached, the game for the hunter became scarcer, and that his life in consequence assumed a more nomad character? Then, and possibly not till then, may a series of migrations to "fresh woods and pastures new" not unnaturally have ensued; and these, following the usual course of "westward towards the setting sun," might eventually lead to a Palæolithic population finding its way to the extreme borders of Western Europe, where we find such numerous traces of its presence.

How long a term of years may be involved in such a migration it is impossible to say, but that such a migration took place the phenomena seem to justify us in believing. It can hardly be supposed that the process that I have shadowed forth was reversed, and that Man, having originated in North-western Europe, in a cold climate where clothing was necessary and food scarce, subsequently migrated eastward to India and southward to the Cape of Good Hope! As yet, our records of discoveries in India and Eastern Asia are but scanty; but it is there that the traces of the cradle of the human race are, in my opinion, to be sought, and possibly future discoveries may place upon a more solid foundation the visionary structure that I have ventured to erect.

It may be thought that my hypothesis does not do justice to what Sir Thomas Browne has so happily termed "that great antiquity, America." I am, however, not here immediately concerned with the important Neolithic remains of all kinds with which this great continent abounds. I am now confining myself to the question of Palæolithic man and his origin, and in considering it I am not unmindful of the Trenton implements, though I must content myself by saying that the "turtle-back" form is essentially different from the majority of those on the wide dissemination of which I have been speculating; and, moreover, as many here present are aware, the circumstances of the finding of these American implements are still under careful discussion.

Leaving them out of the question for the present, it may be thought worth while to carry our speculations rather further, and to consider the relations in time between the Palæolithic and the Neolithic Periods. We have seen that the stage in human civilisation denoted by the use of the ordinary forms of Palæolithic implements must have extended over a vast period of time if we have to allow for the migration of the primeval hunters from their original home, wherever it may have been in Asia or Africa, to the west of Europe, including Britain. We have seen that, during this migration, the forms of the weapons and tools made from silicious stones had become, as it were, stereotyped, and further, that, during the subsequent extended period implied by the erosion of the valleys, the modifications in the form of the implements, and the changes in the fauna associated with the men who used them, were but slight.

At the close of the period during which the valleys were being eroded, comes that represented by the latest occupation of the caves by Palæolithic man, when both in Britain and in the south of France the reindeer was abundant; but among the stone weapons and implements of that long troglodytic phase of man's history, not a single example with the edge sharpened by grinding has as yet been found. All that can safely be said is that the larger implements, as well as the larger mammals, had become scarcer; that greater power in chipping flint had been attained; that the arts of the engraver and the sculptor had considerably developed; and that the use of the bow had probably been discovered.

Directly we encounter the relics of the Neolithic Period, often, in the case of the caves lately mentioned, separated from the earlier remains by a thick layer of underlying stalagmite, we find flint hatchets polished at the edge and on the surface, cutting at the broad (and not at the narrow) end, and other forms of implements associated with a fauna in all essential respects identical with that of the present day.

Were the makers of these polished weapons the direct descendants of Palæolithic ancestors whose occupation of the country was continuous from the days of the old river gravels? or had these long since died out, so that after Western Europe had for ages remained uninhabited, it was re-peopled in Neolithic times by the immigration of some new race of men? Was there, in fact, a "great gulf fixed" between the two occupations? or was there in Europe a gradual transition from the one stage of culture to the other?

It has been said that "what song the Syrens sang, or what name Achilles assumed when he hid himself among women, though puzzling questions, are not beyond all conjecture"; and though the questions now proposed may come under the same category, and must await the discovery of many more essential facts before they receive definite and satisfactory answers, we may, I think, throw some light upon them if we venture to take a few steps upon the seductive, if insecure, paths of conjecture. So far as I know, we have as yet no trustworthy evidence of any transition from the one age to the other, and the gulf between them remains practically unbridged. We can, indeed, hardly name the part of the world in which to seek for the cradle of Neolithic civilisation, though we know that traces of what appear to have been a stone-using people have been discovered in Egypt, and that what must be among the latest of the relics of their industry have been assigned to a date some 3500 to 4000 years before our era. The men of that time had attained to the highest degree of skill in working flint that has ever been reached. Their beautifully-made knives and spear-heads seem indicative of a culminating point reached after long ages of experience; but whence these artists in flint came, or who they were, is at present absolutely unknown, and their handiworks afford no clue to help us in tracing their origin.

Taking a wider survey, we may say that, generally speaking, not only the fauna but the surface configuration of the country were, in Western Europe at all events, much the same at the commencement of the Neolithic Period as they are at the present day. We have, too, no geological indications to aid us in forming any chronological scale.

The occupation of some of the caves in the south of France seems to have been carried on after the erosion of the neighbouring river valleys had ceased, and, so far as our knowledge goes, these caves offer evidence of being the latest in time of those occupied by Man during the Palæolithic Period. It seems barely possible that, though in the north of Europe there are no distinct signs of such late occupation, yet that, in the south, Man may have lived on, though in diminished numbers; and that in some of the caves—such, for instance, as those in the neighbourhood of Mentone—there may be traces of his existence during the transitional period that connects the Palæolithic and Neolithic Ages. If this were really the case, we might expect to find some traces of a dissemination of Neolithic culture from a North Italian centre, but I much doubt whether any such traces actually exist.

If it had been in that part of the world that the transition took place, how are we to account for the abundance of polished stone hatchets found in Central India? Did Neolithic man return eastward by the same route as that by which in remote ages his Palæolithic predecessor had migrated westward? Would it not be in defiance of all probability to answer such a question in the affirmative? We have, it must be confessed, nothing of a substantial character to guide us in these speculations; but, pending the advent of evidence to the contrary, we may, I think, provisionally adopt the view that owing to failure of food, climatal changes, or other causes, the occupation of Western Europe by Palæolithic man absolutely ceased, and that it was not until after an interval of long duration that Europe was re-peopled by a race of men immigrating from some other part of the globe where the human race had survived, and in course of ages had developed a higher stage of culture than that of Palæolithic man.

I have been carried away by the liberty allowed for conjecture into the regions of pure imagination, and must now return to the realms of fact, and one fact on which I desire for a short time to insist is that of the existence at the present day, in close juxtaposition with our own civilisation, of races of men who, at all events but a few generations ago, lived under much the same conditions as did our own Neolithic predecessors in Europe.

The manners and customs of these primitive tribes and peoples are changing day by day, their languages are becoming obsolete, their myths and traditions are dying out, their ancient processes of manufacture are falling into oblivion, and their numbers are rapidly diminishing, so that it seems inevitable that ere long many of these interesting populations will become absolutely extinct. The admirable Bureau of Ethnology instituted by our neighbours in the United States of America has done much towards preserving a knowledge of the various native races in this vast continent; and here in Canada the annual Archæological Reports presented to the Minister of Education are rendering good service in the same cause.

Moreover the Committee of this Association appointed to investigate the physical characters, languages, and industrial and social conditions of the North-western tribes of the Dominion of Canada is about to present its twelfth and final report, which, in conjunction with those already presented, will do much towards preserving a knowledge of the habits and languages of those tribes. It is sad to think that Mr. Horatio Hale, whose comprehensive grasp of the bearings of ethnological questions, and whose unremitting labours have so materially conduced to the success of the Committee, should be no longer among us. Although this report is said to be final, it is to be hoped that the Committee may be able to indicate lines upon which future work in the direction of ethnological and archæological research may be profitably carried on in this part of Her Majesty's dominions.

It is, however, lamentable to notice how little is being, or has been, officially done towards preserving a full record of the habits, beliefs, arts, myths, languages, and physical characteristics of the countless other tribes and nations more or less uncivilised, which are comprised within the limits of the British Empire. At the meeting of this Association held last year at Liverpool, it was resolved by the General Committee "that it is of urgent importance to press upon the Government the necessity of establishing a Bureau of Ethnology for Greater Britian, which by collecting information with regard to the native races within and on the borders of the Empire will prove of immense value to science and to the Government itself." It has been suggested that such a bureau might with the greatest advantage and with the least outlay and permanent expense be connected either with the British Museum or with the Imperial Institute, and the project has already been submitted for the consideration of the Trustees of the former establishment.

The existence of an almost unrivalled ethnological collection in the Museum, and the presence there of officers already well versed in ethnological research, seem to afford an argument in favour of the proposed bureau being connected with it. On the other hand, the Imperial Institute was founded with an especial view to its being a centre around which every interest connected with the dependencies of the Empire might gather for information and support. The establishment within the last twelve months of a Scientific Department within the Institute, with well-appointed laboratories and a highly-trained staff, shows how ready are those concerned in its management to undertake any duties that may conduce to the welfare of the outlying parts of the British Empire; a fact of which I believe that Canada is fully aware. The Institute is therefore likely to develop, so far as its scientific department is concerned, into a bureau of advice in all matters scientific and technical, and certainly a Bureau of Ethnology, such as that suggested, would not be out of place within its walls.

Wherever such an institution is to be established, the question of its existence must of necessity rest with Her Majesty's Government and Treasury, inasmuch as without funds, however moderate, the undertaking cannot be carried on. I trust that in considering the question it will always be borne in mind that in the relations between civilised and uncivilised nations and races it is of the first importance that the prejudices, and especially the religious or semi-religious and caste prejudices, of the latter should be thoroughly well known to the former. If but a single "little war" could be avoided in consequence of the knowledge acquired and stored up by the Bureau of Ethnology preventing such a misunderstanding as might culminate in warfare, the cost of such an institution would quickly be saved.

I fear that it will be thought that I have dwelt too long on primæval man and his modern representatives, and that I should have taken this opportunity to discuss some more general subject, such as the advances made in the various departments of science since last this Association met in Canada. Such a subject would no doubt have afforded an infinity of interesting topics on which to dilate. Spectrum analysis, the origin and nature of celestial bodies, photography, the connection between heat, light, and electricity, the practical applications of the latter, terrestrial magnetism, the liquefaction and solidification of gases, the behaviour of elements and compounds under the influence of extreme cold, the nature and uses of the Röntgen rays, the advances in bacteriology and in prophylactic medicine, might all have been passed under review, and to many of my audience would have seemed to possess greater claims to attention than the subject that I have chosen.

It must, however, be borne in mind that most, if not indeed all, of these topics will be discussed by more competent authorities in the various Sections of the Association by means of the Presidential addresses or otherwise. Nor must it be forgotten that I occupy this position as a representative of Archæology, and am therefore justified in bringing before you a subject in which every member of every race of mankind ought to be interested—the antiquity of the human family and the scenes of its infancy.

Others will direct our thoughts in other directions, but the further we proceed the more clearly shall we realise the connection and inter-dependence of all departments of science. Year after year, as meetings of this Association take place, we may also foresee that "many shall run to and fro and knowledge shall be increased." Year after year advances will be made in science; and in reading that Book of Nature that lies ever open before our eyes, successive stones will be brought for building up that Temple of Knowledge of which our fathers and we have laboured to lay the foundations. May we not well exclaim with old Robert Recorde?—

"Oh woorthy temple of Goddes magnificence: Oh throne of glorye and seate of the lorde: thy substance most pure what tonge can describe? thy signes are so wondrous, surmountinge mannes witte, the effects of thy motions so diuers in kinde: so harde for to searche, and worse for to fynde—Thy woorkes are all wonderous, thy cunning unknowen: yet seedes of all knowledge in that booke are sowen—And yet in that boke who rightly can reade, to all secrete knowledge it will him straighte leade" (Preface to Robert Recorde's "Castle of Knowledge," 1556.)

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. A. R. FORSYTH, M.A., SC.D., F.R.S., PRESIDENT OF THE SECTION.

ONE of the most important events of the past year, connected with the affairs of this Section, has been the reception by the Prime Minister, Lord Salisbury, of a deputation to represent the need for the establishment of a National Physical Laboratory to carry out investigations of certain definite types. Such institutions exist in France and Germany, and have proved of the highest usefulness in a field of work that includes the wide range from pure research to the most direct applications of science to industry. The desire for such an institution in England has long been felt, and as far back as 1891 Prof. Oliver Lodge, when presiding over our Section at the Cardiff meeting, argued in its favour. It has frequently been discussed since that date, particularly in 1895, when Sir Douglas Galton dealt with it so ably in his presidential address at Ipswich, and also in a communication to our Section. The subject was then formally referred to a committee of physicists, who, at last year's meeting in Liverpool, presented a report containing a working scheme for developing the Kew Observatory into an institution of the desired character. The recommendations of the report were approved by a unanimous vote of this Section; and were subsequently adopted by the Association. Thereupon a joint committee, representing the various scientific bodies throughout the United Kingdom interested in the matter, was constituted to further the plan: in particular, to urge upon the Government the establishment of such a Laboratory, and, if possible, to obtain from them the funds which are a preliminary necessity for that purpose. It was a deputation from this joint committee which, headed by Lord Lister, waited upon the Prime Minister on February 16 last. His reply to the deputation was manifestly sympathetic with the request; there is consequently reasonable ground for supposing that the Government will take the matter into their favourable consideration.

After having said, by way of preface, thus much upon the chief event of the past year arising partly from our direct action, I wish to turn to the main line of my address, and to ask, for a brief time, your attention and your consideration for the subject of pure mathematics. If, remembering the brilliant address made at the Montreal meeting, you regret that Lord Kelvin is not again now occupying this position: or if, remembering the interest aroused by Prof. J. J. Thomson's address last year, you regret that the fascinating tale then opened is not being resumed by some one with imagination enough and knowledge enough to continue it: I can, not unselfishly, share your regret.

It appears, however, from the practice of the Council and the General Committee, to be their policy that mathematicians belonging to the extreme right (if the phrase may be used) shall from time to time be nominated to the presidency of the Section. It is, I think, the case that this Section has always had assigned to it the subjects of Mathematics and Physics. In their development, pure mathematics has continued to be associated with applied mathematics, and applied mathematics with physics. So far as I know, there is no substantial reason why any change should be made, and so far as I have been able to observe, there is a strong consensus of opinion that no change by way of separation need be tried. Wide as is the range of our discussions, distracting as is the occasional variety in the matter of the papers we receive, the complexity of our Section, if in any respect a disadvantage, does not appreciably discount the advantages it otherwise secures. Specialisation in all our subjects has become almost a necessity for progress; but excessive obedience need not be paid to that necessity. On the one hand, there will be danger of imperfect appreciation if a subject is so completely restricted to a few specialists that it is ignored by all but them; and, on the other hand, there will be danger of unsound growth if subject and thinkers alike become isolated, and cease to take an active interest in the methods, the processes, and the results other than those which directly concern them. Accordingly, I think that our group of sciences, which form a continuous range, are better united than divided.

Aristotle declared that it is unbecoming to praise the gods. Observing his canon, I shall say nothing as to the wisdom and the justice of our Executive in sometimes selecting a pure mathematician to preside over this Section. I shall only appeal to your indulgence in accepting the opportunity they have thus given me of speaking more specially about my own subject.

I make this appeal the more earnestly, for two particular reasons. One of these is based upon the conflicting views, popularly held and sometimes summarily expressed, about the subject and those who are addicted to it. It is true that the day has gone by, when it is necessary to give serious consideration to attacks upon mathematical studies, and particularly upon analysis, such as were made by the metaphysician Hamilton: attacks no longer thought worthy of any answer. Feelings of hostility, if ever they were widely held, have given way to other feelings, which in the mildest form suggest toleration and acquiescence, and in the most extreme form suggest solemn respect and distant wonder. By common consent, we are allowed without reproach to pursue our aims; though those aims sometimes attract but little sympathy. It is not so long since, during one of the meetings of the Association, one of the leading English newspapers briefly described a sitting of this Section in the words, "Saturday morning was devoted to pure mathematics, and so there was nothing of any general interest": still, such toleration is better than undisguised and ill-informed hostility. But the attitude of respect, I might almost say of reverence, is even more trying: we mathematicians are supposed to be of a different mould, to live far up the heights above the driving gales of controversy, breathing a rarer intellectual atmosphere, serene in impenetrable calm. It is difficult for us to maintain the gravity of demeanour proper to such superior persons; and perhaps it is best to confess at once that we are of the earth, earthy, that we have our differences of opinion and of judgment, and that we can even commit the Machiavelian crime of making blunders.

The other of my reasons for claiming your indulgence is of a graver character, and consists in the difficulty of framing general explanations about the subject. The fact is that mathematics do not lend themselves readily to general exposition. Clifford, it is true, could lecture and enchant his audience: and yet even his lectures ranged about the threshold of the temple of mathematical knowledge and made no attempt to reveal the shrines in the sanctuary. The explanation of this initial difficulty is, however, at hand. Our vocabulary is highly technical, perhaps as technical as is that of moral philosophers: and yet even the technicality of a vocabulary can be circumvented by prolixity of statement. But the ideas and the subject-matter in any branch of our study, when even only moderately developed, are so abstract as to demand an almost intolerable prolixity of statement if an attempt is made to popularise them. Moreover, of the many results obtained, there are few that appeal to an unprofessional sympathy. Adams could discover a new planet by subjecting observations made of the known planets to the most profound calculations; and the world, not over curious about the process, could appreciate the significant result. But such instances are rare; for the most part, our particular results must remain somewhat intangible, somewhat incomprehensible, to those who dwell resolutely and completely outside the range of mathematical knowledge.

What then am I to do? It would be pleasant to me, though it might not prove satisfying to you, to discourse of the present state of one branch or of several branches of mathematics, and particularly to indicate what seem to be lines of possible and probable growth in the future. Instead of pursuing this course, I shall keep my remarks of a general character as far as possible, and shall attempt, not merely to describe briefly some of the relations of pure mathematics to other branches of science, but also to make a bold claim that the unrestricted cultivation of pure mathematics is desirable in itself and for its own sake. Some—I should like to believe many—who are here will concede this claim to the fullest extent and without reservation; but I doubt whether this is so in general. And yet the claim is one which needs to be made before an English-speaking audience. For it is a curious fact that, although the United Kingdom has possessed some of the very greatest of pure mathematicians in the second half of this century, the subject has there received but a scant share of attention as compared with that which it has found in France, in Germany, in Italy, in Sweden and Norway, or in the United States. I am not oblivious of the magnificent contributions to other parts of our science made alike by British leaders and British followers; their fame is known to the world. But apathy rather than attention has been the characteristic feature of our attitude towards pure mathematics; and it seems to me a misfortune, alike for the intellectual activity of the nation and for the progress of the subject, that English thought has had relatively so small an influence upon its vast modern developments.

Now it is not enough for my purpose to be told that the British Association includes all science in its scope, and consequently includes pure mathematics. A statement thus made might be framed in a spirit of mere sufferance; what I wish to secure is a recognition of the subject as one which, being full of life and overflowing with a power of growth, is worthy of the most absorbing devotion. The most cursory examination of the opinions of scientific men leads at once to the conclusion, that there are two views of the subject, both accurate so far as they go, both inadequate whether alone or combined, which to some extent explain if they do not justify what may be called the English attitude in the past. Let me deal with these in succession.

One of these estimates has been framed by what is called the practical man; he regards the subject as a machine which is to provide him with tables, as far as tables can be calculated; and with the simplest formulæ and the most direct rules, whenever tables cannot be calculated. Results, not methods, are his want; it is sufficient for him that an authoritative statement as to a result shall be made; all else is ignored. And for what is beyond, in the shape of work that does nothing to meet his special wants, or of the processes that have led to the results he uses, he cares little or nothing. In fact, he would regard mathematics as a collection of formulæ and an aggregate of processes to grind out numerical results; whatever else there is in it, may be vain and is useless. In his view, it is to be the drudge of the practical sciences.

Now it is undoubtedly an advantage in any case that labour should be saved and time economised; and where this can be done, either by means of calculations made once for all or by processes that lead to results admitting simple formulation, any mathematician will be glad, particularly if his own work should lead to some such issue. But he should not be expected to consider that his science has thus fulfilled its highest purpose; and perhaps he is not unreasonable if, when he says that such results are but a very small part, and not the most interesting part, of his science, he should claim a higher regard for the whole of it. Indeed, I rather suspect that some change is coming; the practical man himself is changing. The developments in the training for a profession, for example, that of an engineer, and the demands that arise in the practice of the profession, are such as to force gradually a complete change of view. When I look into the text-books that he uses, it seems to me a necessity that an engineer should now possess a mathematical skill and knowledge in some directions which, not so very long since, could not freely be found among the professional mathematicians themselves. And as this change is gradually effected, perhaps the practical man will gradually change his estimate of the scope of mathematical science.

I pass from the practical man to some of the natural philosophers. Many of them, though certainly far from all of them, expound what they consider proper and economical limits to the development of pure mathematics. Their wisdom gives varied reasons; it speaks in tones of varied appreciation; but there can be no doubt as to its significance and its meaning. Their aim is to make pure mathematics, not indeed the drudge, but the handmaid of the sciences. The demand requires examination, and deserves respectful consideration. There is no question of giving or withholding help in furthering, in every possible fashion and with every possible facility, the progress of natural philosophy; there is no room for difference upon that matter. The difference arises when the opinion is expressed or the advice is tendered that the activity of mathematicians and all their investigations should be consciously limited, and directed solely and supremely, to the assistance and the furtherance of natural philosophy.

One group of physicists, adopting a distinctly aggressive attitude in imposing limits so as to secure prudence in the pursuit of pure mathematics, regard the subject as useful solely for arriving at results connected with one or other of the branches of natural philosophy; they entertain an honest dislike, not merely to investigations that do not lead to such results, but to the desirability of carrying out such investigations; and some of them have used highly flavoured rhetoric in expressing their dislike. It would be easy—but unconvincing—to suggest that, with due modifications in statement, they might find themselves faced with the necessity of defending some of their own researches against attacks as honestly delivered by men absorbed in purely practical work. But such a suggestion is no reply, for it does not in the least touch the question at issue; and I prefer to meet their contention with a direct negative.

By way of illustration let me take a special instance: it is nót selected as being easier to confute than any other, but because it was put in the forefront by one of the vigorous advocates of the contention under discussion—a man of the highest scientific distinction in his day. He wrote: "Measured [by the utility of the power they give] partial differential equations are very useful, and therefore stand very high [in the range of pure mathematics] as far as the second order. They apply to that point in the most important way to the great problems of nature, and are worthy of the most careful study. Beyond that order they apply to nothing." This last statement, it may be remarked, is inaccurate; for partial differential equations, of an order higher than the second, occur—to give merely a few examples—in investigations as to the action of magnetism on polarised light, in researches on the vibrations of thick plates or of curved bars, in the discussion of such hydrodynamical questions as the motion of a cylinder in fluid or the damping of air-waves owing to viscosity.

Putting this aside, what is more important is the consideration of the partial differential equations of the second order that are found actually to occur in the investigations. Each case as it arises is discussed solely in connection with its particular problem; one or two methods are given, more or less in the form of rules; if these methods fail, the attempt at solution subsides. The result is a collection of isolated processes, about as unsatisfactory a collection as is the chapter labelled Theory of Numbers in many text-books on algebra, when it is supposed to represent that great branch of knowledge. Moreover, this method suffers from the additional disadvantage of suggesting little or no information about equations of higher orders.

But when the equations are considered, not each by itself but as ranged under a whole system, then the investigation of the full theory places these processes in their proper position, gives them a meaning which superficially they do not exhibit, and indicates the way in which each solution satisfies the general conditions of existence of a solution. For the full theory of partial differential equations of the second order in, say, two independent variables establishes the conditions of existence of a solution, the limitations upon the conditions which make that solution unique, the range of variation within which that solution exists, the modes of obtaining expressions for it when it can be expressed in a finite form, and an expression for the solution when it cannot be expressed in a finite form. Of course, the actual derivation of the solution of particular equations is dependent upon analytical skill, as is always the case in any piece of calculating work; but the general theory indicates the possibilities and the limitations which determine the kind of solution to be expected. But not only does the general theory effect much by way of coordinating isolated processes—and, in doing so, lead to new results—but it gives important indications for dealing with equations of higher orders, and it establishes certain theorems about them merely by simple generalisations.

In fact, the special case quoted is one more instance, added to the many instances that have occurred in the past, in which the utilitarian bias in the progress of knowledge is neither the best stimulus nor in the long run the most effective guide towards securing results. It may be—it frequently is—at first the only guide possible, and for a time it continues the best guide, but it does not remain so for ever. It would be superfluous, after Cayley's address in 1883, to show how branches of mathematical physics, thus begun and developed, have added to knowledge in their own direction; they have suggested, they have even created, most fascinating branches of pure mathematics, which, when developed, have sometimes proved of reciprocal advantage to the source from which they sprang. But for proper and useful development they must be free from the restrictions which the sterner group of natural philosophers would lay upon them.

Now I come to another group of natural philosophers who will unreservedly grant my contention thus far; who will yield a ready interest to our aims and our ideas, but who consider that the possibility of applying our results in the domain of physical science should regulate, or at least guide, advance in our work. Some of these entertain this view because they think that possibility of early application is, in the last resource, the real test of useful development; some, because they fear that the profusion of papers annually published and the bewildering specialisation in each branch, are without purpose, and may ultimately lead to isolation or separation of whole sections of mathematics from the general progress of science.

The danger arising from excess of activity seems to me unreal; at any rate there are not signs of it at home at the present day, and I would gladly see more workers at pure mathematics, though not of course at the expense of attention paid to any other branch. But for results that are trivial, for investigations that have no place in organic growth and development, or in illustration and elucidation, surely the natural end is that they soon subside into mere tricks of "curious pleasure or ingenious pain." However numerous they may be, they do not possess intrinsic influence sufficient to cause evil consequences, and any attempt at repression will, if successful, inevitably and unwisely repress much more.

More attention must be paid to the suggestion that mathematicians should be guided in their investigations by the possibility of practical issues. That they are so guided to a great extent is manifest from many of the papers written in that spirit; that they cannot accept practical issues as the sole guide would seem sufficiently justified by the consideration that practical issues widen from year to year and cannot be foreseen in the absence of a divining spirit. Moreover, if such a principle were adopted, many an investigation undertaken at the time for its intrinsic interest would be cast on one side unconsidered, because it does not satisfy an external test that really has nothing to do with the case, and may change its form of application from time to time.

To emphasise this opinion that mathematicians would be unwise to accept practical issues as the sole guide or the chief guide in the current of their investigations, it may be sufficient to recall a few instances from history in which the purely mathematical discovery preceded the practical application and was not an elucidation or an explanation of observed phenomena. The fundamental properties of conic sections were known to the Greeks in the fourth and the third centuries before the Christian era; but they remained unused for a couple of thousand years until Kepler and Newton found in them the solution of the universe. Need I do more than mention the discovery of the planet Neptune by Adams and Leverrier, in which the intricate analysis used had not been elaborated for such particular applications? Again, it was by the use of refined analytical and geometrical reasoning upon the properties of the wave-surface that Sir W. R. Hamilton inferred the existence of conical refraction which, down to the time when he made his inference, had been "unsupported by any facts observed, and was even opposed to all the analogies derived from experience."

It may be said that these are time-honoured illustrations, and that objections are not entertained as regards the past, but fears are entertained as regards the present and future. Very well; let me take one more instance, by choosing a subject in which the purely mathematical interest is deemed supreme, the theory of functions of a complex variable. That at least is a theory in pure mathematics, initiated in that region and developed in that region; it is built up in scores of papers, and its plan certainly has not been, and is not now, dominated or guided by considerations of applicability to natural phenomena. Yet what has turned out to be its relation to practical issues? The investigations of Lagrange and others upon the construction of maps appear as a portion of the general property of conformal representation; which is merely the general geometrical method of regarding functional relations in that theory. Again, the interesting and important investigations upon discontinuous two-dimensional fluid motion in hydrodynamics, made in the last twenty years, can all be, and now are all, I believe, deduced from similar considerations by interpreting functional relations between complex variables. In the dynamics of a rotating heavy body, the only substantial extension of our knowledge made since the time of Lagrange has accrued from associating the general properties of functions with the discussion of the equations of motion. Further, under the title of conjugate functions, the theory has been applied to various questions in electrostatics, particularly in connection with condensers and electrometers. And, lastly, in the domain of physical astronomy, some of the most conspicuous advances made in the last few years have been achieved by introducing into the discussion the ideas, the principles, the methods, and the results of the theory of functions. It is unnecessary to speak in detail of this last matter, for I can refer you to Dr. G. W. Hill's interesting "Presidential Address to the American Mathematical Society" in 1895; but without doubt the refined and extremely difficult work of Poincaré and others in physical astronomy has been possible only by the use of the most elaborate developments of some pure mathematical subjects, developments which were made without a thought of such applications.

Now it is true that much of the theory of functions is as yet devoid of explicit application to definite physical subjects; it may be that these latest applications exhaust the possibilities in that direction for any immediate future; and it is also true that whole regions of other theories remain similarly unapplied. Opinion and divination as to the future would be as vain as they are unnecessary; but my contention does not need to be supported by speculative hopes or uninformed prophecy.

If in the range of human endeavour after sound knowledge there is one subject that needs to be practical, it surely is Medicine. Yet in the field of Medicine it has been found that branches such as biology and pathology must be studied for themselves and be developed by themselves with the single aim of increasing knowledge; and it is then that they can be best applied to the conduct of living processes. So also in the pursuit of mathematics, the path of practical utility is too narrow and irregular, not always leading far. The witness of history shows that, in the field of natural philosophy, mathematics will furnish more effective assistance if, in its systematic development, its course can freely pass beyond the ever-shifting domain of use and application.

What I have said thus far has dealt with considerations arising from the outside. I have tried to show that, in order to secure the greatest benefit for those practical or pure sciences which use mathematical results or methods, a deeper source of possible advantage can be obtained by developing the subject independently than by keeping the attention fixed chiefly upon the applications that may be made. Even if no more were said, it might be conceded that the unrestricted study of mathematics would thereby be justified. But there is another side to this discussion, and it is my wish now to speak very briefly from the point of view of the subject itself, regarded as a branch of knowledge worthy of attention in and for itself, steadily growing and full of increasing vitality. Unless some account be taken of this position, an adequate estimate of the subject cannot be framed; in fact, nearly the greater part of it will thus be omitted from consideration. For it is not too much to say that, while many of the most important developments have not been brought into practical application, yet they are as truly real contributions to human knowledge as are the disinterested developments of any other of the branches included in the scope of pure science.

It will readily be conceded for the present purpose that knowledge is good in and by itself, and that the pursuit of pure knowledge is an occupation worthy of the greatest efforts which the human intellect can make. A refusal to concede so much would, in effect, be a condemnation of one of the cherished ideals of our race. But the mere pursuit or the mere assiduous accumulation of knowledge is not the chief object; the chief object is to possess it sifted and rationalised: in fact, organised into truth. To achieve this end, instruments are requisite that may deal with the respective well-defined groups of knowledge, and for one particular group, we use the various sciences. There is no doubt that, in this sense, mathematics is a great instrument; there remains for consideration the decision as to its range and function—are they such as to constitute it an independent science, or do they assign it a position in some other science?

I do not know of any canonical aggregate of tests which a subject should satisfy before it is entitled to a separate establishment; but, in the absence of a recognised aggregate, some important tests can be assigned which are necessary, and may, perhaps, be sufficient. A subject must be concerned with a range of ideas forming a class distinct from all other classes; it must deal with them in such a way that new ideas of the same kind can be associated and assimilated; and it should derive a growing vigour from a growing increase of its range. For its progress, it must possess methods as varied as its range, acquiring and constructing new processes in its growth; and new methods on any grand scale should supersede the older ones, so that increase of ideas and introduction of new principles should lead both to simplification and to increase of working power within the subject. As a sign of its vitality, it must ever be adding to knowledge and producing new results, even though within its own range it propound some questions that have no answer and other questions that for a time defy solution; and results already achieved should be an intrinsic stimulus to further development in the extension of knowledge. Lastly, at least among this list, let me quote Sylvester's words: "It must unceasingly call forth the faculties of observation and comparison; one of its principal methods must be induction; it must have frequent recourse to experimental trial and verification, and it must afford a boundless scope for the highest efforts of imagination and invention." I do not add as a test that it must immediately be capable of practical application to something outside its own range, though of course its processes may be also transferable to other subjects, or, in part, derivable from them.

All these tests are satisfied by pure mathematics: it can be claimed without hesitation or exaggeration that they are satisfied with ample generosity. A complete proof of this declaration would force me to trespass long upon your time, and so I propose to illustrate it by references to only two or three branches.

First, I would refer to the general theory of invariants and covariants. The fundamental object of that theory is the investigation and the classification of all dependent functions which conserve their form unaltered in spite of certain general transformations effected in the functions upon which they depend. Originally it began as the observation of a mere analytical property of a particular expression, interesting enough in itself, but absolutely isolated. This then suggested the inverse question: What is the general law of existence of such functions if they exist as more than mere casual and isolated occurrences? and how can they all be determined? The answer to these questions led to the construction of the algebraical theory of invariants for linear transformations, and subsequently to the establishment of covariantive forms in all their classes. Next came the question of determining what is practically the range of their existence: that is, is there a complete finite system of such functions in each particular case? and if there is, how is it composed, when in a form that ought to admit of no further reduction? These questions, indeed, are not yet fully answered.

While all this development of the theory of invariants was made upon these lines, without thought of application to other subjects, it was soon clear that it would modify them greatly. It has invaded the domain of geometry, and has almost re-created the analytical theory; but it has done more than this, for the investigations of Cayley have required a full reconsideration of the very foundations of geometry. It has exercised a profound influence upon the theory of algebraical equations; it has made its way into the theory of differential equations; and the generalisation of its ideas is opening out new regions of the most advanced and profound functional analysis. And so far from its course being completed, its questions fully answered, or its interest extinct, there is no reason to suppose that a term can be assigned to its growth and its influence.

As one reference has already been made to the theory of functions of a complex variable, in regard to some of the ways in which it is providing new methods in applied mathematics, I shall deal with it quite briefly now. The theory was, in effect, founded by Cauchy; but, outside his own investigations, it at first made slow and hesitating progress. At the present day, its fundamental ideas may be said almost to govern most departments of the analysis of continuous quantity. On many of them, it has shed a completely new light; it has educed relations between them before unknown. It may be doubted whether any subject is at the present day so richly endowed with variety of method and fertility of resource; its activity is prodigious, and no less remarkable than its activity is its freshness. All this development and increase of knowledge are due to the fact that we face at once the difficulty which even the schoolboy meets in dealing with quadratic equations, when he obtains "impossible" roots; instead of taking the wily x as our subject of operation, we take the still wilier $x + y\sqrt{-1}$ for that purpose, and the result is a transfiguration of analysis.

In passing, let me mention one other contribution which this theory has made to knowledge lying somewhat outside our track. During the rigorous revision to which the foundations of the theory have been subjected in its re-establishment by Weierstrass, new ideas as regards number and continuity have been introduced. With him and with others influenced by him, there has thence sprung a new theory of higher arithmetic; and with its growth, much has concurrently been effected in the elucidation of the general notions of number and quantity. I have already pointed out that the foundations of geometry have had to be re-considered on account of results finding their origin in the theory of invariants and covariants. It thus appears to be the fact that, as with Plato, or Descartes, or Leibnitz, or Kant, the activity of pure mathematics is again lending some assistance to the better comprehension of those notions of time, space, number, quantity, which underlie a philosophical conception of the universe.

The theory of groups furnishes another illustration in the same direction. It was begun as a theory to develop the general laws that govern operations of substitution and transformation of elements in expressions that involve a number of quantities: it soon revolutionised the theory of equations. Wider ideas successively introduced have led to successive extensions of the original foundation, and now it deals with groups of operations of all kinds, finite and infinite, discrete and continuous, with far-reaching and fruitful applications over practically the whole of our domain.

So one subject after another might be considered, all leading to the same conclusion. I might cite the theory of numbers, which has attracted so many of the keenest intellects among men, and has grown to be one of the most beautiful and wonderful theories among the many in the wide range of pure mathematics; or without entering upon the question whether geometry is a pure or an applied science, I might review its growth alike in its projective, its descriptive, its analytical, and its numerative divisions; or I might trace the influence of the idea of continuity in binding together subjects so diverse as arithmetic, geometry, and functionality. What has been said already may, however, suffice to give some slight indication of the vast and ever-widening extent of pure mathematics. No less than in any other science knowledge gathers force as it grows, and each new step once attained becomes the starting-point for steady advance in further exploration. Mathematics is one of the oldest of the sciences; it is also one of the most active, for its strength is the vigour of perpetual youth.

In conclusion, a few words are due to the personal losses caused since our last meeting. It is but little more than two years since Cayley passed away; his life had been full of work, unhasting and unresting in the almost placid course of his great mental strength. While Cayley was yet alive, one name used to be coupled with his when reference was made to English pure mathematics; the two great men were regarded as England's not unworthy contribution to the exploration of the most abstract of the sciences. These fellow-workers, diverse in temperament, in genius, in method, were bound by a friendship that was ended only by death. And now Sylvester too has gone; full of years and honours; though he lived long, he lived young, and he was happily active until practically the very end. Overflowing with an exuberant vitality alike in thought and work, he preserved through life the somewhat rare faculty of instilling his enthusiasm into others. Among his many great qualities, not the least forcible were his vivid imagination, his eager spirit, and his abundant eloquence. When he spoke and wrote of his investigations, or of the subject to which the greater part of his thinking life had been devoted, he did it with the fascination of conviction; and at times—for instance, in his presidential address to this Section at Exeter in 1869—he became so possessed with his sense of the high mission of mathematics, that his utterances had the lofty note of the prophet and the seer.

One other name must be singled out as claiming the passing tribute of our homage; for, in February last, the illustrious and venerable Weierstrass died. He was unconnected with our Association; but science is wider than our body, and we can recognise and salute a master of marvellous influence and unchallenged eminence.

Thus, even to mention no others, pure mathematics has in a brief period lost three of the very greatest of its pioneers and constructors who have ever lived. We know their genius; and the world of thought, though poorer by their loss, is richer by their work.

> Tho' much is taken, much abides, and tho'
> We are not now that strength which in old days
> Moved earth and heaven; that which we are, we are:
> One equal temper of heroic hearts,
> Made weak by time and fate, but strong in will
> To strive, to seek, to find, and not to yield.

Knowledge cannot halt though her heroes fall: the example of their life-long devotion to her progress, and the memory of their achievements, can inspire us and, if need be, can stimulate us in realising the purpose for which we are banded together as an Association—the advancement of science.

SECTION B.

CHEMISTRY.

Opening Address by Prof. William Ramsay, Ph.D., LL.D., Sc.D., F.R.S., President of the Section.

An Undiscovered Gas.

A sectional address to members of the British Association falls under one of three heads. It may be historical, or actual, or prophetic; it may refer to the past, the present, or the future. In many cases, indeed in all, this classification overlaps. Your former Presidents have given sometimes a historical introduction, followed by an account of the actual state of some branch of our science, and, though rarely, concluding with prophetic remarks. To those who have an affection for the past, the historical side appeals forcibly; to the practical man, and to the investigator engaged in research, the actual, perhaps, presents more charm; while to the general public, to whom novelty is often more of an attraction than truth, the prophetic aspect excites most interest. In this address I must endeavour to tickle all palates; and perhaps I may be excused if I take this opportunity of indulging in the dangerous luxury of prophecy, a luxury which the managers of scientific journals do not often permit their readers to taste.

The subject of my remarks to-day is a new gas. I shall describe to you later its curious properties; but it would be unfair not to put you at once in possession of the knowledge of its most remarkable property—it has not yet been discovered. As it is still unborn, it has not yet been named. The naming of a new element is no easy matter. For there are only twenty-six letters in our alphabet, and there are already over seventy elements. To select a name expressible by a symbol which has not already been claimed for one of the known elements is difficult, and the difficulty is enhanced when it is at the same time required to select a name which shall be descriptive of the properties (or want of properties) of the element.

It is now my task to bring before you the evidence for the existence of this undiscovered element.

It was noticed by Döbereiner, as long ago as 1817, that certain elements could be arranged in groups of three. The choice of the elements selected to form these triads was made on account of their analogous properties, and on the sequence of their atomic weights, which had at that time only recently been discovered. Thus calcium, strontium, and barium formed such a group; their oxides, lime, strontia, and baryta are all easily slaked, combining with water to form soluble lime-water, strontia-water, and baryta-water. Their sulphates are all sparingly soluble, and resemblance had been noticed between their respective chlorides and between their nitrates. Regularity was also displayed by their atomic weights. The numbers then accepted were 20, 42·5, and 65; and the atomic weight of strontium, 42·5, is the arithmetical mean of those of the other two elements, for $(65+20)/2=42{\cdot}5$. The existence of other similar groups of three was pointed out by Döbereiner, and such groups became known as "Döbereiner's triads."

Another method of classifying the elements, also depending on their atomic weights, was suggested by Pettenkofer, and afterwards elaborated by Kremers, Gladstone, and Cooke. It consisted in seeking for some expression which would represent the differences between the atomic weights of certain allied elements. Thus, the difference between the atomic weight of lithium, 7, and sodium, 23, is 16; and between that of sodium and of potassium, 39, is also 16. The regularity is not always so conspicuous; Dumas, in 1857, contrived a somewhat complicated expression which, to some extent, exhibited regularity in the atomic weights of fluorine, chlorine, bromine, and iodine; and also of nitrogen, phosphorus, arsenic, antimony and bismuth.

The upshot of these efforts to discover regularity was that, in 1864, Mr. John Newlands, having arranged the elements in eight groups, found that when placed in the order of their atomic weights, "the eighth element, starting from a given one, is a kind of repetition of the first, like the eighth note of an octave in music." To this regularity he gave the name "The Law of Octaves."

The development of this idea, as all chemists know, was due to the late Prof. Lothar Meyer, of Tübingen, and to Prof. Mendeléeff, of St. Petersburg. It is generally known as the "Periodic Law." One of the simplest methods of showing this arrangement is by means of a cylinder divided into eight segments by lines drawn parallel to its axis; a spiral line is then traced round the cylinder, which will, of course, be cut by these lines eight times at each revolution. Holding the cylinder vertically, the name and atomic weight of an element is written at each intersection of the spiral with a vertical line, following the numerical order of the atomic weights. It will be found, according to Lothar Meyer and Mendeléeff, that the elements grouped down each of the vertical lines form a natural class; they possess similar properties, form similar compounds, and exhibit a graded relationship between their densities, melting-points, and many of their other properties. One of these vertical columns, however, differs from the others, inasmuch as on it there are three groups, each consisting of three elements with approximately equal atomic weights. The elements in question are iron, cobalt, and nickel; palladium, rhodium, and ruthenium; and platinum, iridium, and osmium. There is apparently room for a fourth group of three elements in this column, and it may be a fifth. And the discovery of such a group is not unlikely, for when this table was first drawn up Prof. Mendeléeff drew attention to certain gaps, which have since been filled up by the discovery of gallium, germanium, and others.

The discovery of argon at once raised the curiosity of Lord Rayleigh and myself as to its position in this table. With a density of nearly 20, if a diatomic gas, like oxygen and nitrogen, it would follow fluorine in the periodic table; and our first idea was that argon was probably a mixture of three gases, all of which possessed nearly the same atomic weights, like iron, cobalt, and nickel. Indeed, their names were suggested, on this supposition, with patriotic bias, as Anglium, Scotium, and Hibernium! But when the ratio of its specific heats had, at least in our opinion, unmistakably shown that it was molecularly monatomic, and not diatomic, as at first conjectured, it was necessary to believe that its atomic weight was 40, and not 20, and that it followed chlorine in the atomic table, and not fluorine. But here arises a difficulty. The atomic weight of chlorine is 35·5, and that of potassium, the next element in order in the table, is 39·1; and that of argon, 40, follows, and does not precede, that of potassium, as it might be expected to do. It still remains possible that argon, instead of consisting wholly of monatomic molecules, may contain a small percentage of diatomic molecules; but the evidence in favour of this supposition is, in my opinion, far from strong. Another possibility is that argon, as at first conjectured, may consist of a mixture of more than one element; but, unless the atomic weight of one of the elements in the supposed mixture is very high, say 82, the case is not bettered, for one of the elements in the supposed trio would still have a higher atomic weight than potassium. And very careful experiments, carried out by Dr. Norman Collie and myself, on the fractional diffusion of argon, have disproved the existence of any such element with high atomic weight in argon, and, indeed, have practically demonstrated that argon is a simple substance, and not a mixture.

The discovery of helium has thrown a new light on this subject. Helium, it will be remembered, is evolved on heating certain minerals, notably those containing uranium; although it appears to be contained in others in which uranium is not present, except in traces. Among these minerals are clèveite, monazite, fergusonite, and a host of similar complex mixtures, all containing rare elements, such as niobium, tantalum, yttrium, cerium, &c. The spectrum of helium is characterised by a remarkably brilliant yellow line, which had been observed as long ago as 1868 by Profs. Frankland and Lockyer in the spectrum of the sun's chromosphere, and named "helium" at that early date.

The density of helium proved to be very close to 2·0, and, like argon, the ratio of its specific heat showed that it, too, was a monatomic gas. Its atomic weight therefore is identical with its molecular weight, viz. 4·0, and its place in the periodic table is between hydrogen and lithium, the atomic weight of which is 7·0.

The difference between the atomic weights of helium and argon is thus 36, or 40 – 4. Now there are several cases of such a difference. For instance, in the group the first member of which is fluorine we have—

Fluorine...	19	
		16·5
Chlorine..	35·5	
		19·5
Manganese	55	

In the oxygen group—

Oxygen	16	
		16
Sulphur	32	
		20·3
Chromium	52·3	

In the nitrogen group—

Nitrogen	14	
		17
Phosphorus	31	
		20·4
Vanadium	51·4	

And in the carbon group—

Carbon	12	
		16·3
Silicon	28·3	
		19·8
Titanium	48·1	

These instances suffice to show that approximately the differences are 16 and 20 between consecutive members of the corresponding groups of elements. The total differences between the extreme members of the short series mentioned are—

Manganese – Fluorine	36
Chromium – Oxygen...	36·3
Vanadium – Nitrogen	37·4
Titanium – Carbon	36·1

This is approximately the difference between the atomic weights of helium and argon, 36.

There should, therefore, be an undiscovered element between helium and argon, with an atomic weight 16 units higher than than of helium, and 20 units lower than that of argon, namely 20. And if this unknown element, like helium and argon, should prove to consist of monatomic molecules, then its density should be half its atomic weight, 10. And pushing the analogy still further, it is to be expected that this element should be as indifferent to union with other elements as the two allied elements.

My assistant, Mr. Morris Travers, has indefatigably aided me in a search for this unknown gas. There is a proverb about looking for a needle in a haystack; modern science, with the aid of suitable magnetic appliances, would, if the reward were sufficient, make short work of that proverbial needle. But here is a supposed unknown gas, endowed no doubt with negative properties, and the whole world to find it in. Still, the attempt had to be made.

We first directed our attention to the sources of helium—minerals. Almost every mineral which we could obtain was heated in a vacuum, and the gas which was evolved examined. The results are interesting. Most minerals give off gas when heated, and the gas contains, as a rule, a considerable amount of hydrogen, mixed with carbonic acid, questionable traces of nitrogen, and carbonic oxide. Many of the minerals, in addition, gave helium, which proved to be widely distributed, though only in minute proportion. One mineral—malacone—gave appreciable quantities of argon; and it is noteworthy that argon was not found except in it (and, curiously, in much larger amount than helium), and in a specimen of meteoric iron. Other specimens of meteoric iron were examined, but were found to contain mainly hydrogen, with no trace of either argon or helium. It is probable that the sources of meteorites might be traced in this manner, and that each could be relegated to its particular swarm.

Among the minerals examined was one to which our attention had been directed by Prof. Lockyer, named eliasite, from which he said that he had extracted a gas in which he had observed spectrum lines foreign to helium. He was kind enough to furnish us with a specimen of this mineral, which is exceedingly rare, but the sample which we tested contained nothing but undoubted helium.

During a trip to Iceland in 1895, I collected some gas from the boiling springs there; it consisted, for the most part, of air, but contained somewhat more argon than is usually dissolved when air is shaken with water. In the spring of 1896 Mr. Travers and I made a trip to the Pyrenees to collect gas from the mineral springs of Cauterets, to which our attention had been directed by Dr. Bouchard, who pointed out that these gases are rich in helium. We examined a number of samples from the various springs, and confirmed Dr. Bouchard's results, but there was no sign of any unknown lines in the spectrum of these gases. Our quest was in vain.

We must now turn to another aspect of the subject. Shortly after the discovery of helium, its spectrum was very carefully examined by Profs. Runge and Paschen, the renowned spectroscopists. The spectrum was photographed, special attention being paid to the invisible portions, termed the "ultra-violet" and "infra-red." The lines thus registered were found to have a harmonic relation to each other. They admitted of division into two sets, each complete in itself. Now, a similar process had been applied to the spectrum of lithium and to that of sodium, and the spectra of these elements gave only one series each. Hence, Profs. Runge and Paschen concluded that the gas, to which the provisional name of helium had been given, was, in reality, a mixture of two gases, closely resembling each other in properties. As we know no other elements with atomic weights between those of hydrogen and lithium, there is no chemical evidence either for or against this supposition. Prof. Runge supposed that he had obtained evidence of the separation of these imagined elements from each other by means of diffusion; but Mr. Travers and I pointed out that the same alteration of spectrum, which was apparently produced by diffusion, could also be caused by altering the pressure of the gas in the vacuum tube; and shortly after Prof. Runge acknowledged his mistake.

These considerations, however, made it desirable to subject helium to systematic diffusion, in the same way as argon had been tried. The experiments were carried out in the summer of 1896 by Dr. Collie and myself. The result was encouraging. It was found possible to separate helium into two portions of different rates of diffusion, and consequently of different density by this means. The limits of separation, however, were not very great. On the one hand, we obtained gas of a density close on 2·0; and on the other, a sample of density 2·4 or thereabouts. The difficulty was increased by the curious behaviour, which we have often had occasion to confirm, that helium possesses a rate of diffusion too rapid for its density. Thus, the density of the lightest portion of the diffused gas, calculated from its rate of diffusion, was 1·874; but this corresponds to a real density of about 2·0. After our paper, giving an account of these experiments, had been published, a German investigator, Herr A. Hagenbach, repeated our work and confirmed our results.

The two samples of gas of different density differ also in other properties. Different transparent substances differ in the rate at which they allow light to pass through them. Thus, light travels through water at a much slower rate than through air, and at a slower rate through air than through hydrogen. Now Lord Rayleigh found that helium offers less opposition to the passage of light than any other substance does, and the heavier of the two portions into which helium had been split offered more opposition than the lighter portion. And the retardation of the light, unlike what has usually been observed, was nearly proportional to the densities of the samples. The spectrum of these two samples did not differ in the minutest particular; therefore it did not appear quite out of the question to hazard the speculation that the process of diffusion was instrumental, not necessarily in separating two kinds of gas from each other, but actually in removing light molecules of the same kind from heavy molecules. This idea is not new. It had been advanced by Prof. Schützenberger (whose recent death all chemists have to deplore), and later, by Mr. Crookes, that what we term the atomic weight of an element is a mean; that when we say the atomic weight of oxygen is 16, we merely state that the average atomic weight is 16; and it is not inconceivable that a certain number of molecules have a weight somewhat higher than 32, while a certain number have a lower weight.

We therefore thought it necessary to test this question by direct experiment with some known gas; and we chose nitrogen, as a good material with which to test the point. A much larger and more convenient apparatus for diffusing gases was built by Mr. Travers and myself, and a set of systematic diffusions of nitrogen was carried out. After thirty rounds, corresponding to 180 diffusions, the density of the nitrogen was unaltered, and that of the portion which should have diffused most slowly, had there been any difference in rate, was identical with that of the most quickly diffusing portion—*i.e.* with that of the portion which passed first through the porous plug. This attempt, therefore, was unsuccessful; but it was worth carrying out, for it is now certain that it is not possible to separate a gas of undoubted chemical unity into portions of different density by diffusion. And these experiments rendered it exceedingly improbable that the difference in density of the two fractions of helium was due to separation of light molecules of helium from heavy molecules.

The apparatus used for diffusion had a capacity of about two litres. It was filled with helium, and the operation of diffusion was carried through thirty times. There were six reservoirs, each full of gas, and each was separated into two by diffusion. To the heavier portion of one lot, the lighter portion of the next was added, and in this manner all six reservoirs were successfully passed through the diffusion apparatus. This process was carried out thirty times, each of the six reservoirs having had its gas diffused each time, thus involving 180 diffusions. After this process, the density of the more quickly diffusing gas was reduced to 2·02, while that of the less quickly diffusing had increased to 2·27. The light portion on re-diffusion hardly altered in density, while the heavier portion, when divided into three portions by diffusion, showed a considerable difference in density between the first third and the last third. A similar set of operations was carried out with a fresh quantity of helium, in order to accumulate enough gas to obtain a sufficient quantity for a second series of diffusions. The more quickly diffusing portions of both gases were mixed and rediffused. The density of the lightest portion of these gases was 1·98; and after other 15 diffusions, the density of the lightest portion had not decreased. The end had been reached; it was not possible to obtain a lighter portion by diffusion. The density of the main body of this gas is therefore 1·98; and its refractivity, air being taken as unity, is 0·1245. The spectrum of this portion does not differ in any respect from the usual spectrum of helium.

As re-diffusion does not alter the density or the refractivity of this gas, it is right to suppose that either one definite element has now been isolated; or that if there are more elements than one present, they possess the same, or very nearly the same, density and refractivity. There may be a group of elements, say three, like iron, cobalt, and nickel; but there is no proof that this idea is correct, and the simplicity of the spectrum would be an argument against such a supposition. This substance, forming by far the larger part of the whole amount of the gas, must, in the present state of our knowledge, be regarded as pure helium.

On the other hand, the heavier residue is easily altered in density by re-diffusion, and this would imply that it consists of a small quantity of a heavy gas mixed with a large quantity of the light gas. Repeated re-diffusion convinced us that there was only a very small amount of the heavy gas present in the mixture. The portion which contained the largest amount of heavy gas was found to have the density 2·275, and its refractive index was found to be 0·1333. On re-diffusing this portion of gas until only a trace sufficient to fill a Plücker's tube was left, and then examining the spectrum, no unknown lines could be detected, but, on interposing a jar and spark gap, the well-known blue lines of argon became visible; and even without the jar the red lines of argon, and the two green groups were distinctly visible. The amount of argon present, calculated from the density, was 1·64 per cent., and from the refractivity 1·14 per cent. The conclusion had therefore to be drawn that the heavy constituent of helium, as it comes off the minerals containing it, is nothing new, but, so far as can be made out, merely a small amount of argon.

If, then, there is a new gas in what is generally termed helium, it is mixed with argon, and it must be present in extremely minute traces. As neither helium nor argon has been induced to form compounds, there does not appear to be any method, other than diffusion, for isolating such a gas, if it exists, and that method has failed in our hands to give any evidence of the existence of such a gas. It by no means follows that the gas does not exist; the only conclusion to be drawn is that we have not yet stumbled on the material which contains it. In fact, the haystack is too large and the needle too inconspicuous. Reference to the periodic table will show that between the elements aluminium and indium there occurs gallium, a substance occurring only in the minutest amount on the earth's surface; and following silicon, and preceding tin, appears the element germanium, a body which has as yet been recognised only in one of the rarest of minerals, argyrodite. Now, the amount of helium in fergusonite, one of the minerals which yields it in reasonable quantity, is only 33 parts by weight in 100,000 of the mineral; and it is not improbable that some other mineral may contain the new gas in even more minute proportion. If, however, it is accompanied in its still undiscovered source by argon and helium, it will be a work of extreme difficulty to effect a separation from these gases.

In these remarks it has been assumed that the new gas will resemble argon and helium in being indifferent to the action of reagents, and in not forming compounds. This supposition is worth examining. In considering it, the analogy with other elements is all that we have to guide us.

We have already paid some attention to several triads of elements. We have seen that the differences in atomic weights between the elements fluorine and manganese, oxygen and chromium, nitrogen and vanadium, carbon and titanium, is in each case approximately the same as that between helium and argon, viz. 36. If elements further back in the periodic table be examined, it is to be noticed that the differences grow less, the smaller the atomic weights. Thus, between boron and scandium, the difference is 33; between beryllium (glucinum) and calcium, 31; and between lithium and potassium, 32. At the same time, we may remark that the elements grow liker each other, the lower the atomic weights. Now, helium and argon are very like each other in physical properties. It may be fairly concluded, I think, that in so far they justify their position. Moreover, the pair of elements which show the smallest difference between their atomic weights is beryllium and calcium; there is a somewhat greater difference between lithium and potassium. And it is in accordance with this fragment of regularity that helium and argon show a greater difference. Then again, sodium, the middle element of the lithium triad, is very similar in properties both to lithium and potassium; and we might, therefore, expect that the unknown element of the helium series should closely resemble both helium and argon.

Leaving now the consideration of the new element, let us turn our attention to the more general question of the atomic weight of argon, and its anomalous position in the periodic scheme of the elements. The apparent difficulty is this: The atomic weight of argon is 40; it has no power to form compounds, and thus possesses no valency; it must follow chlorine in the periodic table, and precede potassium; but its atomic weight is greater than that of potassium, whereas it is generally contended that the elements should follow each other in the order of their atomic weights. If this contention is correct, argon should have an atomic weight smaller than 40.

Let us examine this contention. Taking the first row of elements, we have:

$$Li=7,\ Be=9{\cdot}8,\ B=11,\ C=12,\ N=14,\ O=16,\ F=19,\ ?=20.$$

The differences are:

$$2{\cdot}8,\ 1{\cdot}2,\ 1{\cdot}0,\ 2{\cdot}0,\ 2{\cdot}0,\ 3{\cdot}0,\ 1{\cdot}0.$$

It is obvious that they are irregular. The next row shows similar irregularities. Thus:

$$(?=20),\ Na=23,\ Mg=24{\cdot}3,\ Al=27,\ Si=28,\ P=31,\ S=32,\ Cl=35{\cdot}5,\ A=40.$$

And the differences:

$$3{\cdot}0,\ 1{\cdot}3,\ 2{\cdot}7,\ 1{\cdot}0,\ 3{\cdot}0,\ 1{\cdot}0,\ 3{\cdot}5,\ 4{\cdot}5.$$

The same irregularity might be illustrated by a consideration of each succeeding row. Between argon and the next in order, potassium, there is a difference of $-0{\cdot}9$; that is to say, argon has a higher atomic weight than potassium by 0·9 unit; whereas it might be expected to have a lower one, seeing that potassium follows argon in the table. Further on in the table there is a similar discrepancy. The row is as follows:

$$Ag = 108,\ Cd = 112,\ In = 114,\ Sn = 119,\ Sb = 120{\cdot}5,\ Te = 127{\cdot}7,\ I = 127.$$

The differences are:

$$4{\cdot}0,\ 2{\cdot}0,\ 5{\cdot}0,\ 1{\cdot}5,\ 7{\cdot}2,\ -0{\cdot}7.$$

Here, again, there is a negative difference between tellurium and iodine. And this apparent discrepancy has led to many and careful redeterminations of the atomic weight of tellurium. Prof. Brauner, indeed, has submitted tellurium to methodical fractionation, with no positive results. All the recent determinations of its atomic weight give practically the same number, 127·7.

Again, there have been almost innumerable attempts to reduce the differences between the atomic weights to regularity, by contriving some formula which will express the numbers which represent the atomic weights, with all their irregularities. Needless to say, such attempts have in no case been successful. Apparent success is always attained at the expense of accuracy, and the numbers reproduced are not those accepted as the true atomic weights. Such attempts, in my opinion, are futile. Still, the human mind does not rest contented in merely chronicling such an irregularity; it strives to understand why such an irregularity should exist. And, in connection with this, there are two matters which call for our consideration. These are: Does some circumstance modify these "combining proportions" which we term "atomic weights"? And is there any reason to suppose that we can modify them at our will? Are they true "constants of nature," unchangeable, and once for all determined? Or are they constant merely so long as other circumstances, a change in which would modify them, remain unchanged?

In order to understand the real scope of such questions, it is necessary to consider the relation of the "atomic weights" to other magnitudes, and especially to the important quantity termed "energy."

It is known that energy manifests itself under different forms, and that one form of energy is quantitatively convertible into another form, without loss. It is also known that each form of energy is expressible as the product of two factors, one of which has been termed the "intensity factor," and the other the "capacity factor." Prof. Ostwald, in the last edition of his "Allgemeine Chemie," classifies some of these forms of energy as follows:

Kinetic energy is the product of	Mass	into	the square of velocity.
Linear	,,	,,	Length into force.
Surface	,,	,,	Surface into surface tension.
Volume	,,	,,	Volume into pressure.
Heat	,,	,,	Heat capacity (entropy) into temperature.
Electrical	,,	,,	Electrical capacity into potential.
Chemical	,,	,,	"Atomic weight" into affinity.

In each statement of factors, the "capacity factor" is placed first, and the "intensity factor" second.

In considering the "capacity factors," it is noticeable that they may be divided into two classes. The two first kinds of energy, kinetic and linear, are *independent of the nature of the material* which is subject to the energy. A mass of lead offers as much resistance to a given force, or, in other words, possesses as great inertia as an equal mass of hydrogen. A mass of iridium, the densest solid, counterbalances an equal mass of lithium, the lightest known solid. On the other hand, surface energy deals with molecules, and not with masses. So does volume energy. The volume energy of two grammes of hydrogen, contained in a vessel of one litre capacity, is equal to that of thirty-two grammes of oxygen at the same temperature, and contained in a vessel of equal size. Equal masses of tin and lead have not equal capacity for heat; but 119 grammes of tin has the same capacity as 207 grammes of lead; that is, equal atomic masses have the same heat capacity. The quantity of electricity conveyed through an electrolyte under equal difference of potential is proportional, not to the mass of the dissolved body, but to its equivalent; that is, to some simple fraction of its atomic weight. And the capacity factor of chemical energy is the atomic weight of the substance subjected to the energy. We see, therefore, that while mass or inertia are important adjuncts of kinetic and linear energies, all other kinds of energy are connected with atomic weights, either directly or indirectly.

Such considerations draw attention to the fact that quantity of matter (assuming that there exists such a carrier of properties as we term "matter") need not necessarily be measured by its inertia, or by gravitational attraction. In fact the word "mass" has two totally distinct significations. Because we adopt the convention to measure quantity of matter by its mass, the word "mass" has come to denote "quantity of matter." But it is open to any one to measure a quantity of matter by any other of its energy factors. I may, if I choose, state that those quantities of matter which possess equal capacities for heat are equal; or that "equal numbers of atoms" represent equal quantities of matter. Indeed, we regard the value of material as due rather to what it can do, than to its mass; and we buy food, in the main, on an atomic, or perhaps, a molecular basis, according to its content of albumen. And most articles depend for their value on the amount of food required by the producer or the manufacturer.

The various forms of energy may therefore be classified as those which can be referred to an "atomic" factor, and those which possess a "mass" factor. The former are in the majority. And the periodic law is the bridge between them; and yet, an imperfect connection. For the atomic factors, arranged in the order of their masses, display only a partial regularity. It is undoubtedly one of the main problems of physics and chemistry to solve this mystery. What the solution will be is beyond my power of prophecy; whether it is to be found in the influence of some circumstance on the atomic weights, hitherto regarded as among the most certain "constants of nature"; or whether it will turn out that mass and gravitational attraction are influenced by temperature, or by electrical charge, I cannot tell. But that some means will ultimately be found of reconciling these apparent discrepancies, I firmly believe. Such a reconciliation is necessary, whatever view be taken of the nature of the universe and of its mode of action; whatever units we may choose to regard as fundamental among those which lie at our disposal.

In this address I have endeavoured to fulfil my promise to combine a little history, a little actuality, and a little prophecy. The history belongs to the Old World; I have endeavoured to share passing events with the New; and I will ask you to join with me in the hope that much of the prophecy may meet with its fulfilment on this side of the ocean.

NOTES.

We are glad to learn that Lord Armstrong, who has for the past few days been suffering from a slight sunstroke, is now much better. Dr. Gibb, of Newcastle, who was hastily summoned to Bamburgh Castle on Sunday, anticipates that if the progress is maintained his lordship will be quite well again by the end of the week.

Lieut. de Gerlache's expedition to the Antarctic regions left Antwerp on Monday on board the steamer *Belgica*.

Prof. Corfield has been elected an honorary member of the Royal Society of Public Health of Belgium, of which he has been a corresponding member for some years.

A special number of the *Rendiconti della R. Accademia dei Lincei* announces the award of the following prizes, besides others for essays of a literary character:—The Royal Prize for physics to Prof. Adolfo Bartoli, of Padua, for his two monographs on the specific heat of water between the temperatures of 0° and 35°, and on the heat of the sun, and for other investigations. For the Ministerial Prize for physical and chemical science eight competitors entered, and the judges have awarded a prize of 1000 lire to Prof. Carlo Bonacini, of Modena, for his essays on orthochromatic and colour photography, and on the reflection and other properties of Röntgen rays; also awards of 250 lire each to Prof. Carlo Cattaneo, of Turin, for his notes on the conductivity of electrolytes and on the velocity of ions, and to Prof. Pietro Bartolotti for chemical investigations relating to the compound Rottlerine and other derivatives.

Science of August 6 prints a long article, by Mr. Cyrus Adler, on the movement towards an international catalogue of scientific works, and reprints the official reports of the proceedings of the conference held at the Royal Society a year ago. The report which the American delegates, Prof. Simon Newcomb and Dr. John S. Billings, presented to the Secretary of State, was, in accordance with their suggestion, referred to the Secretary of the Smithsonian Institution for his views as to the propriety and feasibility of the work proposed being undertaken by that Institution, and as to the probable cost. After considering the matter, Mr. S. P. Langley replied that if the work should be assigned to the Institution, a grant of not less than ten thousand dollars per annum would be required to carry it out. This reply and the documents to which it refers were transmitted to the U.S. Senate and House of Representatives towards the close of last year; and though no result has yet been reached, it is hoped that Congress will give support to the proposal, so that when the time comes the funds needed for cataloguing the scientific publications of the United States will be granted.

The meeting of the French Association for the Advancement of Science, which opened at St. Étienne on August 5, was concluded on Saturday last. At the opening of the meeting, the President, M. Marey, gave an address on "La méthode graphique et les sciences experimentales." The address is printed in the *Revue Scientifique* for August 7. Next year's meeting will be at Nantes, while in 1899 the congress will be held at Boulogne, in order that visits may be exchanged with members of the British Association at Dover.

The St. Petersburg correspondent of the *Times* reports that at Peterhof on Tuesday morning thirty-six members of the twelfth International Medical Congress, which is to be opened to-day at Moscow, were presented to the Tsar. Each country sending delegates was represented by a small deputation of its more eminent men now present in St. Petersburg, chosen from about 6000 altogether who are expected to attend the congress. The British representatives who had the honour of a presentation were Sir William MacCormac, Sir William Stokes, from Ireland, Prof. Stevenson, from Scotland, and Prof. Stevenson, of Netley, on behalf of the medical department of the British Army. On arriving at Peterhof by boat the party were met by Dr. Hirsch, the Tsar's medical attendant, and conducted in Court carriages to the Palace, where luncheon was served to them in the Hall of Mirrors. Each group was subsequently introduced to the presence of the Emperor, who gave them a cordial welcome. The Tsaritsa was also present.

WE regret to record the death of Mr. S. E. Peal, of Assam, a frequent correspondent to these columns, and the author of a number of papers on astronomical subjects. We also notice the announcement of the death of Mr. Samuel Laing, author of "Modern Science and Modern Thought," and many other popular works of a similar character; Hofrath Dr. Alfred Ritter von Arneth, president of the Vienna Academy of Sciences; Prof. de Volson Wood, professor of mechanical engineering in the Stevens Institute of Technology, Hoboken, and formerly professor of mathematics and mechanics in the same institute; and Dr. Tholozan, Correspondant in the Section of Medicine and Surgery of the Paris Academy of Sciences.

THE death is announced of Mr. Albert Marth, for many years a Fellow of the Royal Astronomical Society, to the publications of which he contributed a large number of valuable papers, particularly ephemerides for the satellites of the planets, and for physical observations of Mars and Jupiter. Mr. Marth (says the *Athenæum*) was born at Colberg, in Pomerania, on May 5, 1828, but came to England after he had completed his studies at Berlin and Königsberg, and was connected with the observatories at Regent's Park and Durham, afterwards assisting Lassell with his nebular and other observations at Malta. He discovered the small planet Amphitrite, No. 29, at Mr. Bishop's observatory in 1854. During the last nine years of his life he had been in charge of Colonel Cooper's observatory at Markree Castle, Co. Sligo; but his health had been failing, and he died somewhat suddenly whilst on a visit to his native country.

A LARGE party of Prof. W. K. Brooks' biological students from the Johns Hopkins University, under the charge of Prof. J. E. Humphrey, are at present at Jamaica studying the tropical fauna and flora, and carrying on research work at the north-east side of the island.

WE learn from *Science* that the following grants have been made to the United States Geological Survey for the present fiscal year: The topographical surveys, 175,000 dols.; for geological surveys and researches, 100,000 dols.; for investigation of coal and gold in Alaska, 5000 dols.; palæontology, 10,000 dols.; chemistry, 7000 dols.; gauging streams and water-supply, 50,000 dols.; mineral resources, 20,000 dols. There are also allowances for illustrations, printing, &c.

THE September issue of the *American Naturalist* will appear under entirely new management. The magazine has been purchased from the estate of the late Prof. Edward D. Cope by a number of gentlemen who are interested in the advancement of the natural sciences, and Dr. Robert P. Bigelow, of the Massachusetts Institute of Technology, Boston, has accepted the post of editor-in-chief. He will be assisted by an editorial committee and by a board of associate editors.

AT the beginning of next year, the Boyden Premium of the Franklin Institute, Philadelphia, will be awarded. The premium is the sum of one thousand dollars, and it will be to "any resident of North America who shall determine by experiment whether all rays of light, and other physical rays, are or are not transmitted with the same velocity." The memoirs describing in detail the apparatus employed in investigating this question, the mode of experimenting, and the results obtained, must be sent in before January 1, 1898.

WE learn from the *Times* that it has been decided to appoint, in place of Mr. Rigby, late Superintendent of the Government Factory at Enfield, who has retired on a pension, a new officer with the title of Deputy-Director-General of the Ordnance Factories. He will receive a salary of 1500*l.* a year, with a suitable residence and with title to a pension. He will have primary charge of the Enfield Factory, but he will also assist the Director-General, Sir William Anderson, F.R.S., in his duties at Woolwich.

THE ninth International Congress of Hygiene and Demography will be held in Madrid from April 10 to 17, 1898.

WE have received from the Hon. Stephen Coleridge, Hon. Secretary of the Anti-vivisection Society, a copy of some correspondence which recently took place in the *St. James's Gazette* with reference to the use of curare in the practice of vivisection. Under the influence of curare it is believed that animals are still conscious of pain, and allegations have been made against some physiologists that they had used curare as an anæsthetic. We do not, as a rule, devote any attention to the reckless assertions so often made by anti-vivisectionists, our reason being that they are usually the result of ignorance which moves us more to pity than to anger. But as the correspondence referred to has apparently been sent to us for comment, we do not hesitate to say that we are astounded at the audacious impudence of persons who venture to criticise matters about which they know nothing. Not content with the Home Secretary's reply to Mr. Weir, in the House of Commons, that the charges made as to the mis-use of curare morphia were "absolutely baseless," Mr. Coleridge extracts from certain papers by Messrs. Bayliss, Hill, and Gulland the words: "Throughout the experiments morphia was the anæsthetic used," and triumphantly points to the "atrocious suffering" involved in experiments so performed. In the course of a reply to this attack, Mr. Leonard Hill said: "It is my invariable rule to perform all cutting operations on animals under complete chloroform anæsthesia. . . . It would be idle to repeat the full details of anæsthesia in every paper published in a journal of pure science—details which are, as a matter of course, recognised by all scientific readers. From such papers the officials of the Anti-vivisection Society piece together defamatory statements. The experiments decried by Edward Berdoe and Mr. Coleridge were carried out without the infliction of pain, and at the end of these experiments the animals were killed. It is my habit to inject from half to one grain of acetate of morphia into small dogs—a dose so large that it cannot be given with safety to an adult man. Such doses produce absolute coma in the animals. The use of morphia to prolong anæsthesia after the initial use of chloroform is a practice common among surgeons." We accept entirely Mr. Hill's explanation, and are amazed at the perverse interpretations which Mr. Coleridge and his fellow-agitators are good enough to put upon the writings of physiologists. The chief source of regret to us is that organisations like the Anti-vivisection Society, existing as they do upon the gullibility of an ill-informed public, should be permitted to publish their irresponsible accusations without fear of punishment. It would be better for humanity as well as science if such societies were not allowed to exist.

A MOTOR-CAR race from Paris to Trouville, which excited as much interest as that from Paris to Dieppe, took place on Sunday last. A correspondent of the *Times* reports that twenty-two motor cars, of various designs, and twenty-six motor cycles started from St. Germain between 10 and 10.30 in the morning. The distance to Trouville is about 108 miles, and the motor cycle and car first in the Dieppe race also arrived first on Sunday. M. Jannin on his cycle rode the distance under four hours, while M. Gille's Hourgières-Bollée carriage covered the 108 miles in four hours and twenty minutes. Twelve of the motor cycles arrived in less than six hours, and seven of the cars in the same time.

THE first annual report on the work of the Geological Survey of Cape Colony has just been published. Prof. G. S. Corstorphine, the geologist to the Colony, throws cold water upon the belief that payable coal-seams exist beneath the Karoo. He points out that, though it is just possible a coal-seam may be found among the shales of the Karoo, the hope of such a lucky find becomes almost daily less, and the probability of an extensive coal deposit underlying the Karoo is very slight. Deep boring has been again and again advocated as a sure means of discovering coal, but Dr. Corstorphine says that so far there is no evidence geologically to warrant the renewal of such an expensive procedure as deep boring with such a purpose; and this conclusion is not only based upon the work carried out under his direction last year, but also on the results of previous investigations. The question of a probably water-supply from deep boring having been brought before the Commission, Dr. Corstorphine and Mr. A. W. Rogers (assistant geologist) made a preliminary survey of the Oudtshoorn and Prince Albert districts with a view to the selection of a site for a deep bore-hole. But so little is known about the structure of the country and the rocks composing it, that the people who are crying out for deep boring for artesian water will have to exercise a little patience, unless they are willing to provide a large sum of money to be spent upon a series of purely experimental bores. A large amount of information has yet to be obtained before a geologist would give an opinion as to the existence of considerable quantities of water at great depths in the Colony. The Commission, therefore, concludes (and rightly so) that an expenditure upon a deep bore-hole with the idea of finding water would be premature in the present state of knowledge.

AT the International Meteorological Conference held at Paris in September last, Mr. C. L. Wragge, Government Meteorologist for Queensland, drew attention to the importance of establishing an observatory on the top of Mount Kosciusko, in the south-eastern extremity of Australia, at a height of about 8000 feet. The conference expressed the opinion that such a station would possess really scientific importance, and that it would be useful to publish hourly observations made there. We are glad to learn from the *Brisbane Courier* that the Hon. R. Barr-Smith, of Torrens Park, South Australia, has offered to subscribe the whole amount necessary for the establishment of a tentative station at the summit of the above-mentioned mountain. Mr. Wragge hopes that the comparison of the results with those obtained from low-level stations will ensure a permanent observatory being established in the interest of Australasia.

DURING the last thirteen years the Indian Department of Revenue and Agriculture has published memoranda on the snowfall in the mountain districts, with forecasts of the probable character of the south-west monsoons. These statements and forecasts are drawn up by the Government Meteorological Reporter, and have been found of considerable use in predicting the probability of abnormal rainfall during the monsoon period of June to September, heavy and prolonged snowfall in the Western Himalayan area either preventing or delaying the extension of the monsoon current during the rainy season. The investigation seems to show that the general conditions are not unfavourable to the establishment of at least normal monsoon currents, although in parts there has been more snow than usual. It also shows that there has been a cyclical variation in the rainfall during the past five years; 1892–94 were characterised by excessive rain, and in the next two years the rainfall was deficient. The conclusion drawn from this is that there is considerable probability that the present year will be one of deficient rainfall and be the last year of the cycle, but that it will be much less unfavourable than last year. The variations in the rainfall are supposed to be due to some general, but as yet unknown, causes affecting a much larger area than India.

THERE are so many unsettled points with reference to the mode of formation of hailstones that careful observations of the internal structure of hailstones are always of interest, inasmuch as they may prove of assistance in working out the development-history of these meteoric objects. For this reason we are glad to note the following details which Dr. Alex. Hodgkinson observed in hailstones that fell in Wilmslow, Cheshire, during a storm of great severity on August 5. The hail varied in size from seven-eighths of an inch downwards, and the general shape was more or less conical with convex bases. As to the internal structure, Dr. Hodgkinson writes as follows:—"A nucleus of variable size existed in each hailstone, and this was surrounded by an outer layer of clear ice. In some of the larger specimens an intermediate zone of slightly opaque ice was seen, but more transparent than the nucleus. Under the microscope, with a power of about twenty diameters, the structure of the nucleus was seen to be coarsely crystalline, and profusely interspersed with minute air-bubbles, reminding one forcibly of the vacuoles which so frequently occur in specimens of quartz, and give rise to its opalescent appearance. The intermediate zone, when present, was constituted by the existence of similar vacuoles, but far smaller, and no crystalline structure was here perceptible. The outermost layer consisted of clear ice, apparently structureless under the microscope; but with the naked eye, and by variations in the incident light, this might be seen to possess a coarsely radiating structure, as if composed of large radiating crystals. On embedding a hailstone in a piece of perforated card, and examining with polarised light, there was no indication of tangential and radial strain in the body as a whole. The outer and intermediate layers were isotropic, but the individual crystals of the nucleus were distinctly double-refractive."

A DETAILED description of the large seismometrograph recently placed in the observatory of Catania is contributed by Prof. A. Riccò to vol. x. series 4ª of the *Dagli Atti dell'Accademia Gioenia di Scienze Naturali in Catania.* Prof. Riccò was led to erect this instrument because those of a similar character at Rome and Rocca di Papa gave such excellent results. The description is accompanied by an excellent photograph taken by means of the flash light.

FROM Profs. Elster and Geitel we have received a reprint of their last paper, published in *Wiedemann's Annalen*, dealing with the relation between the photo-voltaic current and the kathodic absorption of light when the angle of incidence and direction of polarisation of the incident light are varied. The experiments, which were conducted with the assistance of the Elizabeth Thompson Science Fund, of Boston, show that the current, so far as it depends on these factors, is determined by the amount of light absorbed at the kathode, and the agreement between the curves representing the current and the absorption affords a striking confirmation of the theory of metallic reflection.

ABOUT a quarter of a century has elapsed since Father Bertelli made his first observations on the microseismic movements of the ground. The accuracy of his results was soon disputed, because his pendulums were suspended from a bracket attached to the wall of his observatory, and a long and indecisive controversy ensued, which has at last been concluded by an interesting paper by M. S. Arcidiacono in the *Bollettino* of the Italian Seismological Society. The observations described in this memoir were made for more than three years with two normal tromometers at the observatory of Catania, one suspended from a thick wall, the other from an isolated column built on an old lava-stream. It was found that the mean displacement of the former instrument almost invariably exceeded that of the latter, though not differing greatly from it. Both tromometers show a prevalence of north-and-south displacements, due probably to the existence of Etna on the north; but the planes of oscillation coincided on only 381 days, while they differed on 667 days. One important conclusion is that, so far as regards magnitude of displacement, a normal tromometer suspended from the wall of a building may give useful results.

Aus dem Archiv der Deutschen Seewarte (nineteenth year, 1896), published under the direction of the Seewarte, contains four contributions of interest. The first of these is by Herrn. E. Knipping, who points out the importance of examining minutely the log-books of vessels with regard to the meteorological records, and the publication of the chief and most important results gained thereform. He gives also samples of how the observations may be most simply printed and used as references. The appendix contains an application of the method advocated, the log-books employed being those that were contained in home-coming ships during the month of January in 1894. The second article is an addition to a previous one, which dealt with experiments relating to the "stopping down," or reduction in size, of the sidelights carried by ships. "Tafeln für die Vorausberechnung der Sternbedeckungen" is the title of the third part, and is contributed by Dr. Carl Stechert. This contains a description of the formulæ and tables for the prediction of occultations of stars, and a clear explanation as to how they should be used, both as regards approximate and very accurate results. The fourth and last part is perhaps the most important, and is by Herrn. E. Engelenburg, formerly Director of the Königl. Niederl. Meteorolog. Observatoriums in Utrecht. The subject dealt with is the aerodynamic theory of storms, and is preceded by a capital historical summary of the various theories propounded down to the present day. The article is too long and important to be discussed here, so we must simply refer our readers directly to the original. We may mention that a very useful "Litteratur-Nachweis" is attached.

AN interesting memoir, by C. T. Mörner, has recently appeared in the *Zeitschrift für physiologische Chemie*, dealing with a method of preserving fish, much employed in many parts of the northern districts of Sweden. The freshly-caught fish are cleaned, washed, and placed in wooden casks, and are then covered with brine. The casks are then closed and made air-tight, and placed in the open air in a sunny place, and allowed to remain there for from five to six weeks. The process of fermentation, which soon ensues, is controlled by means of a small vent-hole, which is opened from time to time. If the fermentation becomes too active the casks are placed in the shade, or some cooler place is chosen for them. As soon as the requisite stage in the process has been reached, the casks are opened, and the now-finished article is packed in smaller vessels for storage and distribution. This article of diet, known in Swedish as "surfisk," is eaten either raw or toasted. Mörner has endeavoured to ascertain what is the nature of the chemical products elaborated during the process of fermentation to which the fish are submitted. As accounting for the disagreeable odour which characterises this preparation, Mörner found amongst the gases emitted during fermentation the offensive-smelling methylmercaptan. Amongst the organic acids discovered in the "surfisk," whilst absent in the fresh fish, succinic acid, butyric acid, formic, acetic, and valeric acids were detected, whilst large quantities of ammonia and some ptomaïnes were also found, amongst the latter being choline and leucine. Curiously indol, skatol, phenol, putrescine and cadaverine, so characteristic of putrefactive processes in general, were absent in this preparation.

THE increasing interest taken in British bird-life has encouraged the editors of *Knowledge* to permanently devote a considerable space in each number to notes on the habits, distribution, migration, and so on, of British birds. A number of well-known ornithologists, including, amongst others, Messrs. W. Eagle Clarke, J. Cordeaux, H. E. Dresser, W. Warde Fowler, J. A. Harvie-Brown, and Thomas Southwell, have promised contributions. The department will be conducted by Mr. Harry F. Witherby.

THE discovery of human and other remains in the Halberstadt Cave, Jamaica, and of rock-carvings at St. John's, aroused considerable interest in the subject of aboriginal remains in the island. An exhibition of such remains, illustrating the life and customs of the aboriginal inhabitants, was held in the museum of the Institute of Jamaica at the end of 1895, and it not only brought to notice a number of relics not previously known, but led to various discoveries and investigations of importance to the anthropology of Jamaica and of the West Indies generally. Mr. J. E. Duerden, the curator of the museum, has prepared an interesting illustrated report upon these "Aboriginal Indian Remains in Jamaica," and has included in it a note, by Prof. A. C. Haddon, upon the craniology of the aborigines.

THE following are among the papers and other publications which have come under our notice within the past few days: *Indian Museum Notes* (vol. iv. No. 2), issued by the Trustees of the Indian Museum, Calcutta. Among the contents are short papers on a new species of Buprestid beetle (*Julodis atkinsoni*); two new species of gall-aphid; the well-known "Pipsa" fly (*Simulium indicum*); and many useful notes on insect pests and remedies.—The fifth part of Mr. Oswin A. J. Lee's fine work, entitled "Among British Birds in their Nesting Haunts, illustrated by the Camera" (Edinburgh: David Douglas), has been published. Previous issues of this work have already been noticed; the present part contains the plates illustrating the nests of the Corn Crake, Chaffinch, Capercaillie, Snipe, Mute Swan, Golden-crested Wren, and Sandpiper. Brief descriptions of the habits and haunts of these birds accompany the plates.—A critical review of the methods of determining minerals is contributed to the *Journal of the Franklin Institute* (August), by Dr. Joseph W. Richards.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*, ♂) from India, presented by Mr. Walter Chamberlain; four Common Hedgehogs (*Erinaceus europæus*), British, presented by Mr. Evelyn Pelly; a Slender-billed Cockatoo (*Licmetis nasica*) from South Australia, presented by Mrs. M. D. Vibart; a Matamata Terrapin (*Chelys fimbriata*) from North Brazil, presented by Mr. W. J. Crummach; two Ribbon Snakes (*Tropidonotus saurita*), seven Striped Snakes (*Tropidonotus ordinatus sirtalis*), four Dekay's Snakes (*Ischnognathus dekayi*), two American Milk Snakes (*Coronella triangulum*), two Grass Snakes (*Contia vernalis*) from North America, presented by Mr. J. H. Fleming; a Grey Lemur (*Hapalemur griseus*) from Madagascar, three Altai Deer (*Cervus* sp. inc., ♂ ♀ ♀) from the Altai Mountains, a Circasian Wild Goat (*Capra caucasica*, ♂) from Caucasus, deposited; one Greater Vasa Parrot (*Coracopsis vasa*) from Madagascar, purchased; two Viscachas (*Lagostomus trichodactylus*), three Barbary Wild Sheep (*Ovis tragelaphus*), a Spotted Tinamou (*Nothura maculosa*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

CONDITIONS FOR BEST TELESCOPIC DEFINITION.—Dr. T. J. J. See brings together a few facts and remarks regarding the conditions essential to good seeing with large telescopes and high magnifying powers (*Astr. Nachr.*, No. 3438). These are based not only on his own experience under very favourable circumstances during the past year, but many of the suggestions developed are, as he says, the outcome of Mr. Douglass' work on atmospheric currents and their relation to astronomical seeing. At the Harvard station in Peru the seeing at three o'clock in the morning was nearly always bad, caused, as was discovered, by a current of cold air from the valley draining the great mountains above and rushing down the adjacent gorge flowing over the observatory, and completely ruining the seeing almost instantaneously. Such currents as these must always be avoided when fixing upon a position for an observatory, and this is one of many causes which produce bad definition. The country in which good conditions might be depended on should be free from mountains and cyclonic causes which disturb the equilibrium of the atmosphere. A high and dry table-land, distant from oceanic influence, like the northern part of Arizona, presents conditions which are almost ideal when snow is not present. Mountain sites are always less satisfactory than broad table-lands, because currents forced up from below are cooled by expansion due to diminished pressure, and rapid changes are likely to take place when the wind is strong. When covered with snow and overflown by currents of a different temperature, mountain sites are wholly incapable of giving good definition.

ASTRONOMICAL PHOTOGRAPHY FOR SMALL AND LARGE APERTURES.—In this column (April 8, vol. lv. p. 544) we have previously referred to the remarks which Dr. Isaac Roberts published in *Knowledge* (vol. xx. p. 100) regarding the probable limit in the length of the time of exposure for astronomical photography. In these he showed that his experience led him to conclude that in consequence of prolonged exposure to the latent sky luminosity the film of the negative darkened on development to a degree that would obscure faint nebulosity and faint stars, and that longer exposures of the plates would not reveal additional details of nebulosity, nor more faint star images.

Prof. F. L. O. Wadsworth is not, however, inclined to agree with Dr. Roberts' statement in every particular, and contributes to the *Astronomischen Nachrichten* (No. 3439) an article of great interest, dealing with the question under discussion, in which he states his reasons. This should be read by all who employ the camera for astronomical photography, whether the apertures they use be half an inch or twelve inches. We give here the conclusions.

The absolute intensity of the image of a celestial object, and therefore the absolute photographic light action (product of intensity by time), for a given time of exposure will vary (1) for extended sources as the square of the angular aperture only; (2) for point sources as the product of the square of the angular aperture times the square of the linear aperture.

The contrast between the image of any celestial object (not very near the horizon) and the general field depends upon (1) the brightness of the sky at the time; (2) the efficiency of the image-forming lens as regards perfection of figure and curvature of surfaces, &c.; and (3) upon the square of the linear aperture. If the objectives are good, the sky effect (1) and (3) is the most important.

For faint extended objects, such as nebulæ, irresolvable star clouds, &c., in which we have to deal with the delineation of a surface rather than with individual points, this contrast can only be increased by decreasing the focal length. When the sky effect (1) and (3) is predominant, it will vary inversely as the square of the latter quantity.

For point sources the contrast can only be increased by increasing the angular aperture. Under the same conditions as just mentioned in the last paragraph, it will vary directly as the square of this quantity.

It is the degree of contrast and not size of objective (except in so far as this latter influences the contrast) that determines the limiting magnitude of the faintest object that can be photographed. This limiting magnitude for stars depends, therefore, only on the angular aperture, for nebulæ on the focal length.

The time of exposure also depends very largely on the contrast between image and field, and not on the absolute intensity of the former.

As regards the influence of the character of the objective upon the illumination of the field, the refractor seems to have a decided advantage. The angular aperture of the latter should not, however, be greater than 1 to 5.

To photograph the very faintest stars (beyond 17th mag.) a reflector of the largest possible angular aperture, *i.e.* 1 to 3 or 1 to 2, if possible, is the only instrument that can be used.

NEW VARIABLE STARS.—Mr. Thomas D. Anderson communicates to the *Astronomischen Nachrichten* (No. 3440) the discovery of a new variable star in the constellation of Hercules; its position for 1855·0 being R.A. 16h. 55·0m., Declination +31° 26′. Mr. Anderson noted this star some time ago as being a very faint star of the 9th magnitude, and of about the same brightness as B.D. +31°·2949. Several times in the autumn of last year he was unable to see it with his 2·25 inch-refractor, although he could always see a 9·6 mag. star which is not given in the B.D., but whose coordinates for 1855 are approximately 16h. 55·2m. and +31° 34′. This year, on the 22nd and 26th of last month, he has found the missing object, and it was then brighter than the neighbouring star just mentioned. Its brightness was then estimated as being the same as B.D. +31°·2949 with a magnitude of 9·2. As a guide to those who wish to observe this variable, Mr. Anderson says that it lies further from +31°·2951 than from 2949, and also that +31°·2945, 2946, 2949, the variable, and the 9·6 mag. comparison star are nearly in a straight line.

In the same number of the *Astronomischen Nachrichten*, Mr. Stanley Williams gives a list of seven probably new variable stars which he observed on his way to Australia and back. These variables are, however, all of considerable southern declination, but we may mention three of which the variability "appears to be almost beyond doubt."

Star.	R.A. 1875. h. m.	Decl. 1875. ° ′
L 1713 Cœli ...	5 0·0 ...	−35 53
δ Antliæ ...	10 23·8 ...	−29 58
L 4959 Crucis ...	11 51·9 ...	−55 37

PLANETARY NOTES.—At the oppositions of Jupiter in 1895–96 and 1897, M. Quénisset made some interesting observations of this planet, using a refractor of 16 centimetres aperture, which will be found recorded in the *Bulletin de la Société Astronomique de France* for the present month. Accompanying these are some excellent drawings made by him during those periods of observation. We notice that he has adopted the nomenclature of Lord Ross, Knobel, and Campani for the different zones of the planet by which the positions of special surface markings can be easily located. Why should not all observers of Jupiter adopt the same method, for would not comparisons of different observations be thus rendered more simple? Dr. Fontséré's observations of Venus, which appear in the same number of the *Bulletin*, were made in the first months of this year at the Barcelona Observatory. The surface markings seem to have been clearly seen and recorded, while projections on the terminator and limb were very commonly visible. This observer deduces a long period of rotation for this planet. The observations made by Dr. Peyra during the 1896–97 opposition of Mars appear in the *Memorie della Société degli Spettroscopisti Italiani* (vol. xxvi. 4ª). These were made with a 24-centimetre Merz refractor, and are well worth comparing with those of other observers made about the same time. The drawings accompanying the observations are on rather a small scale, and show only the more prominent markings and canals.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

DR. RODET, the well-known bacteriologist of Lyons, has been appointed Professor of Bacteriology in the University of Lyons.

MR. STANLEY DUNKERLEY, of the Department of Applied Mechanics, Cambridge, has been appointed Professor of Applied Mechanics at the Royal Naval College, Greenwich, in succession to Prof. J. H. Cotterill, F.R.S., who is about to retire after over twenty-four years' service.

THE candidates successful in this year's competition for the Whitworth scholarships and exhibitions are as follows:—(1) Scholarships of 150*l.* (tenable for three years): George M.

Russell, George M. Brown, William Du B. Duddell, George Wilson. (2) Exhibitions of 50*l*. (tenable for one year): George Service, Edgar J. Kipps, Frank Piercy, Arthur Morley, A. Marshall Downie, John R. Powell, Alfred D. Owen, Charles C. Allen, William J. Rouse, Arthur E. Holmes, Alfred T. J. Kersey, Edward C. Horsley, John Berry, James Turnbull, Thomas Taylor, Robert L. Wills, James Paton, Henry T. Sisson, Leonard Ward, Arthur W. Loveridge, Timothy A. Thomas, William Bell, John R. Billington, George Powell, Edgar W. Riley, John S. Marshall, John S. Hague, Frederick Walford, James Davidson, Robert Nelson.

THIS year's successful candidates for Royal Exhibitions, National Scholarships, and Free Studentships (Science), awarded by the Department of Science and Art are as follows:—Royal Exhibitions: Robert L. Sherlock, Gilbert E. James, Howard M. Rootham, Andrew W. Lehmann, William Griffiths, Frank H. Phillips, Alfred L. Oke. National Scholarships for Mechanics (Group A): Arthur W. Ashton, Paul S. Couldrey, Frank Mould, Arthur Morley, Albert Hall, Charles H. Stewart, William W. Firth, George Wall, Alfred T. J. Kersey, John S. Hague. Free Studentships for Mechanics: Arthur W. Loveridge, Percy M. Bennett, Hubert W. Bywaters. National Scholarships for Chemistry and Physics: George H. Broom, Percy M. Hampshire, Frank Wade, John A. Brown, George B. Willey, Daniel Robinson, William L. Odell, Robert L. Bennett, John A. Cunningham, Oswald F. Hudson. Free Studentships for Physics and Chemistry: Victor Lough, Charles Headland, Donald J. Browne. National Scholarships for Biology: Frank Cavers, George E. Nicholls.

THE forty-fourth Report of the Department of Science and Art has just been issued. For the benefit of those who are not familiar with this Departmental publication, it may be remarked that the contents are not merely concerned with museums connected with the Department of Science and Art, and statistics and reports upon the progress of education in science and art during 1896; for appended to the volume is the report of the Director-General of the Geological Survey of the United Kingdom (for an abstract of which see p. 178), and also the Report of the Committee on Solar Physics. The number of students under instruction in Science and Art Department Classes in 1896 was 196,185; these were distributed among 10,500 classes in 2583 separate schools. It is satisfactory to learn that practical instruction in science is making progress; but some little time must elapse before sufficient laboratories are provided to enable all students in Departmental schools to perform the experimental work, without which scientific teaching is of no value. The efficiency of the practical instruction given in certain science subjects is now judged by inspection and not by examination. This should encourage the practical side of science instruction, and prove of great benefit to the students and the teachers In evening classes as much cannot be done in the development of this kind of instruction as in classes in day schools, but even in these a good beginning has been made in some few cases. It is announced in the Report that it is proposed to divide the Honours stages of the various science subjects into two parts, the first part being intermediate in difficulty between the advanced stage and the second part of Honours. It is also announced that a new syllabus is in preparation to form part of the present elementary stage of biology, and be a preparatory study for biological science in the same way that the new section of the elementary stage of physiography is for physical science.

THE Glasgow University Court has sent us a memorandum referring to the disciplinary or penal powers of the qualifying medical authorities. Upon several occasions the General Medical Council has occupied itself with the question of the expediency of obtaining further disciplinary powers, especially as regards the Universities, to be exercised by the qualifying medical authorities over those to whom they grant diplomas entitling the holders of them to be admitted to the *Medical Register*. It was pointed out by the General Medical Council a year ago that there are six Universities which do not possess any disciplinary powers, and that there are in addition two Universities which possess only partial or limited disciplinary powers over their graduates. The result is this, that however gross the misconduct of a graduate may be, whether as a convicted felon, or declared by the General Medical Council guilty of infamous conduct in a professional respect, for which his name has been removed from the *Medical Register*, he still retains the degree and the title conferred upon him by any one of these Universities. Such a state or condition of matters must cause very considerable regret to the authorities of the University which has conferred the degree, and which it has no power to cancel. The association of their names with such black sheep amongst them must also cause much regret to be felt by the graduates, and must lead them to feel how desirable it is that the authorities of the University should obtain powers to take away degrees which are thus discredited. The Scotch Universities' Commissioners have been appealed to, but they have decided that they have no power by ordinance to alter the status of any graduate, or to confer upon the Universities powers which they do not already possess as regards deprivation in cases of discreditable conduct or proved legal offence. The Privy Council has, however, expressed a desire to aid in the matter, and has indicated that further powers might be obtained by statute, or, in the case of the Scotch Universities, by ordinance. The Glasgow University Court has, therefore, asked the Universities' Commissioners to again consider the question, and to obtain the opinion of all the Scottish Universities upon it, so that their final report may assist in bringing the Universities in line with each other, and satisfy the wish of the General Medical Council.

SCIENTIFIC SERIALS.

American Journal of Science, August.—*Tamiobatis vetustus*, a new form of fossil skate, by C. R. Eastman. The only remains of this fish are a skull found in Powell County, Kentucky; exact site unknown. It is embedded in a greenish-grey limestone of a talcose structure, probably Middle or Upper Devonian. The skull presents some features that are shark-like, and differs notably from the skulls of existing rays. It indicates a very generalised condition, and it is impossible to assign it to any known genus or family, but there are resemblances to the Rhinobatidæ.—The Florencia formation, by O. H. Hershey. This is an ancient stream gravel of North-western Illinois, consisting largely of galena limestone derived from Pleistocene rock gorges.—Native iron in the coal measures of Missouri, by E. T. Allen. Native iron was found at Cameron, Weanbleau, and Holden, Missouri. It was found in every case at such a depth from the surface, and under such conditions, that there can be no doubt as to its terrestrial origin. Besides, the specimens contained no nickel, which is always associated with meteoric iron. In Cameron, Clinton Co., it was found as a vein five or six inches thick, embedded in sandstone at a depth of fifty-one feet.—On Bixbyite, a new mineral, and on the associated topaz, by S. L. Penfield and H. W. Foote. The mineral is found very sparingly on the edge of the desert, about thirty-five miles south-west of Simpson, Utah. The crystals, which are brilliant black, and of metallic lustre, are implanted upon topaz and decomposed garnet and rhyolite, and have evidently been formed by fumarole action. The composition is essentially $FeO.MnO_2$.—The separation of aluminium and beryllium by the action of hydrochloric acid, by F. S. Havens. This method is based upon the fact that the hydrous aluminium chloride $AlCl_3.6H_2O$ is practically insoluble in a mixture of strong HCl and anhydrous ether saturated with HCl gas. The beryllium is determined by weighing as oxide after conversion to the nitrate and ignition.

Bulletin of the American Mathematical Society (June).—"James Joseph Sylvester" is the title of an address delivered by Dr. Fabian Franklin at a memorial meeting at the Johns Hopkins University (May 2). This is an appreciative estimate of the genius of a man whose death "deprived Mathematical Science of a most brilliant mind, and the scientific world in general of one of its foremost workers" (NATURE, March 18, p. 468; *cf.* also March 25, pp. 492–94). Dr. Franklin closes with the remark that "his work, brilliant and memorable as it was, affords no true measure of his intellectual greatness. Those who came within the sphere of his personality could not but feel that, through the force of circumstances, combined with the peculiarities of his poetic temperament, his performance, splendid as it was, has not adequately reflected his magnificent powers. Those of us who were connected with him, cherish his memory as that of a sympathetic friend and generous critic. And in this university, as long as it shall exist, he will be remembered as the man whose genius illuminated its early years, and whose devotion and ardour furnished the most inspiring of all the elements which went to make those years so memorable and so fruitful."—Mr. C. H. Hinton, in Hyperbolea and the solution of equations, communicates some interesting remarks on the system of mathematics in vogue in Hyperbolea, and shows that a consideration of the methods of the Hyperboleans leads to a graphical representation of quantities of which, given an appropriate train of mechanism, not only the real, but also the imaginary roots of an equation can be mechanically found. The author thus defines the locality of Hyperbolea as being a land in which distance is measured by the function $\sqrt{x^2 - y^2}$. —Lie's geometry of contact transformations is a full and useful analysis (pp. 321–350), by Dr. E. O. Lovett, of the Geometrie der Berührungstransformationen of Lie and G. Scheffers (1° vol. Leipzig, 1896).—Dr. M. Bôcher shortly reviews "Plane and Analytic Geometry," by Messrs. F. H. Bailey and F. S. Woods, and concludes that the book deserves praise, not only for clearness of statement, but in the main for rigour of treatment.

Wiedemann's Annalen der Physik und Chemie, No. 8.—Series spectra of oxygen, sulphur, and selenium, by C. Runge and F. Paschen. The three spectra show a regular structure. The lines may be joined in series which obey the laws found by Rydberg and by Kayser and Runge. There is also a regularity of transition from one spectrum to another. As the atomic weight increases, the spectrum as a whole travels towards the region of greater wave-lengths, as has also been found in the case of other allied elements.—Irreversible processes, by O. Wiedeburg. The reversible process in thermodynamics cannot be practically realised, since some of the operations would have to take place in an indefinitely long time, and others in an indefinitely short time. The author attempts to discover a general treatment of irreversible processes, and begins by separating intensities, quantities, resistances, and energies. An advantage is gained, inasmuch as heat and other forms of energy can be treated by the same equations.—Electrolysis of rarefied gases, by E. Wiedemann and G. C. Schmidt. Gaseous hydrochloric acid is subjected to the action of a certain quantity of electricity in a vacuum tube. The chlorine separated is collected by means of mercury. The result of a series of experiments is that only about 53 per cent. of the theoretical quantity of chlorine due to the current is separated, 31 per cent. being separated at the anode, and the rest at the kathode. Mercury haloids gave less than 6 per cent. Electrolysis of gases is therefore different from that of liquids, and does not obey Faraday's laws.—Magnetic behaviour of soft steel, by Anton Abt. Soft Martin steel is as useful for electromagnets as soft iron, when no rapid alternations are called for. The permeability is about the same, and the permanent magnetism is only about 25 per cent. higher.—Determination of capacities by the balance, by V. von Lang. A coil carrying an alternate current is balanced over another, which is in circuit with a condenser. The difference of phase between the two coils, and hence also the attraction between them, is a function of the capacity of the condenser. The attraction is compensated by weight.—Dielectric constants of solids, by H. Starke. The method described of immersing a fragment of the solid in a mixture of two liquids of widely different dielectric capacities, and adjusting them to that of the solid, is greatly simplified by employing the rapidly alternating currents of Nernst's differential exciter. The conductivities of the liquids no longer interfere with the results.

Bollettino della Società Sismologica Italiana, vol. ii., 1896, Nos. 9 and 10.—Comparative study of two normal tromometers differently mounted, by S. Arcidiacono.—The photographic tromometer, by G. Agamennone.—Description of an apparatus for registering microseismic movements, by G. Mugna.—Notices of earthquakes recorded in Italy (Sept. 9–Dec. 31, 1896); the more important being the earthquakes of Livorno (Nov. 29), Frignano (Dec. 8), and the Province of Pisa (Dec. 25), and distant earthquakes of unknown origin on Oct. 30, Nov. 1, 5 and 10. There is no record of the Hereford earthquake of Dec. 17.

Vol. iii., 1897, No. 1.—The seismic period of Epirus in Jan. 1897, by G. Agamennone. — Relation between the frequency of earthquakes and their intensity, by F. de Montessus de Ballore (in French). An attempt to show that the frequency of earthquakes in a district is a fair measure of its seismic activity if the number of years over which the record extends is great, so that, in estimating the activity, the intensity of the shocks may be left out of account.—On a type of seismometrograph for photographic registration, by G. Agamennone.—Notices of earthquakes recorded in Italy (Jan. 1–Feb. 4, 1897), by G. Agamennone; the more important being the earthquakes of Umbria (Jan. 6–7, 19), Persian Gulf? (Jan. 10–11), and Venetia (Jan. 27), and a distant but unknown earthquake on Jan. 3–4.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 9.—M. A. Chatin in the chair.—The Perpetual Secretary announced to the Academy the loss it had sustained by the death of M. Tholozan, Correspondant in the Section of Medicine and Surgery. He also announced the sudden death of Prof. Victor Meyer at Heidelberg. —On the number and symmetry of the ligneous bundles of the appendices (leaves) in their relation to organic perfection, by M. Ad. Chatin A classification is given according to the number of bundles in the petiole.—On the crystalline form of the chloroplatinates of the diamines, by M. J. A. Le Bel. Measurements are given of the axial ratios for a large number of chloroplatinates of amines of the type $NH.R_1R_2$, where R_1 and R_2 may be any pair of the methyl, ethyl, normal or isopropyl, normal butyl, or amyl groups.—On some ketonic ethers, by M. A. Collet. By the action of a bromo-ketone upon an alcoholic solution of potassium acetate, the acetic ethers of phenyl-(α)-hydroxypropyl ketone, phenyl-(α)-hydroxy-ethyl ketone, and phenyl-(α)-hydroxyisopropyl ketone were prepared in the pure state.—Products of the hydrolysis of starch by diastase, by M. P. Petit.—On a theoretical point in dyeing, by M. Léo Vignon. From the results of the experiments given that the cause of the fixing of substantive colours by cotton is of a chemical order, the nitrogen in the substances added becoming pentavalent. An experiment with the three bases $(C_6H_4)_2(NH_2)_2$, $(C_6H_4)_2(N(CH_3)_2)_2$, and $(C_6H_4)_2[N(CH_3)_3I]_2$ directly confirmed this, since cotton absorbs from six to seven per cent. of the first two bases, but practically none of the third.—On a new alkaloid, by MM. Battandier and Th. Malosse. The alkaloid is extracted from the young branches and the bark of *Retama sphærocarpa*, and hence is termed Retamine, a kilogram of the fresh plant giving about four grams. Its composition appears to be given by the formula $C_{15}H_{26}N_2O$.—On the presence of *Pseudocommis vitis* (Debray) in the stem and leaves of *Elodea canadensis*, by M. E. Roze.—On an acarus of the vines of Grenache (*Carpoglyphus passularum*, Rohin), by M. E. L. Trouessart.—On the root of *Suæda* and *Salsola*, by M. Georges Fron.—On the assimilating tissue of stems deprived of leaves, by M. Auguste Boirivant. The suppression of the leaves in a plant gives rise to a deeper green colour in the stems or petioles, due to the production of a larger number of chlorophyll grains than the normal. There is also a modification in the form of the cell of this tissue, the cells being lengthened radially, and the number of cell layers containing chlorophyll is increased.

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